TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

EDINBURGH.

VOL. XX.

EDINBURGH:
PUBLISHED BY ROBERT GRANT & SON, 82 PRINCES STREET.
AND WILLIAMS & NORGATE, 14 HENRIETTA STREET, COVENT GARDEN, LONDON.

MDCCCLIII.

PRINTED BY NEILL AND COMPANY, EDINBURGH.

CONTENTS.

PART I.

PAGE

I. *On the Volcanic Geology of the Vivarais (Ardêche).* By JAMES D. FORBES, Esq., F.R.S., Sec. R.S. Ed., Professor of Natural Philosophy in the University of Edinburgh. (With Six Plates.) . 1

II. *On a Process in the Differential Calculus, and its application to the Solution of certain Differential Equations.* By the Rev. P. KELLAND, M.A., F.R.SS. L. & E., F.C.P.S., late Fellow of Queen's College, Cambridge; Professor of Mathematics, &c., in the University of Edinburgh, ... 39

III. *On the Constitution of Codeine and its Products of Decomposition.* By THOMAS ANDERSON, M.D., ... 57

IV. *On the Equilibrium of Elastic Fluids.* By Mr JAMES CLERK MAXWELL, 87

V. *Dissertation on a Peruvian Musical Instrument like the Syrinx of the Ancients.* By THOMAS STEWART TRAILL, M.D., F.R.S.E., Professor of Medical Jurisprudence in the University of Edinburgh. (With a Plate.) 121

VI. *Some Remarks on the Theories of Cometary Physics.* By C. PIAZZI SMYTH, Esq., F.R.S.E., F.R.A.S., Professor of Practical Astronomy in the University of Edinburgh, and Astronomer-Royal for Scotland, ... 131

VII. *On the Mechanical Action of Heat, especially in Gases and Vapours.* By WM. J. M. RANKINE, Civil Engineer, F.R.S.E., F.R.S.S.A., &c., ... 147

PART II.

PAGE

VIII. *Note as to the Dynamical Equivalent of Temperature in Liquid Water, and the Specific Heat of Atmospheric Air and Steam;* being a Supplement to a Paper *On the Mechanical Action of Heat.* By Wm. J. M. Rankine, Civil Engineer, F.R.S.E., F.R.S.S.A., &c., ... 191

IX. *On the Power and Economy of Single-Acting Expansive Steam Engines*, being a Supplement to the Fourth Section of a Paper *On the Mechanical Action of Heat.* By Wm. J. M. Rankine, Civil Engineer, F.R.S.E., F.R.S.S.A., &c., ... 195

X. *On the Economy of Heat in Expansive Machines*, forming the Fifth Section of a Paper *On the Mechanical Action of Heat.* By Wm. J. M. Rankine, Civil Engineer, F.R.S.E., F.R.S.S.A., &c. (With a Plate.) 205

XI. *Notes on the Geology of the Eildon Hills, in Roxburghshire.* By James D. Forbes, F.R.S., Sec. R.S. Ed., Professor of Natural Philosophy in the University of Edinburgh. (With a Plate.) 211

XII. *On a new Source for obtaining Capric Acid, and Remarks on some of its Salts.* By Mr Thomas Henry Rowney, F.C.S. Communicated by Dr T. Anderson, ... 219

XIII. *On certain Salts and Products of Decomposition of Comenic Acid.* By Mr Henry How. Communicated by Dr T. Anderson, . 225

XIV. *On the Products of the Destructive Distillation of Animal Substances.* Part II. By Thomas Anderson, M.D., F.R.S.E., . . 247

XV. *On the Dynamical Theory of Heat, with numerical results deduced from* Mr Joule's *Equivalent of a Thermal Unit, and* M. Regnault's *Observations on Steam.* By William Thomson, M.A., Fellow of St Peter's College, Cambridge, and Professor of Natural Philosophy in the University of Glasgow, . . . 261

XVI. *On a Method of Discovering experimentally the Relation between the Mechanical Work spent, and the Heat produced by the Compression of a Gaseous Fluid.* By William Thomson, M.A., Fellow of St Peter's College, Cambridge, and Professor of Natural Philosophy in the University of Glasgow, ... 289

PAGE

XVII. *On the Weight of Aqueous Vapour which is condensed on a Cold Surface, under given conditions.* By JAMES DALMAHOY, Esq., F.R.S.E., ... 299

XVIII. *On some remarkable Marine Invertebrata new to the British Seas.* By EDWARD FORBES, F.R.S., F.L.S., Professor of Botany, King's College, London; and J. GOODSIR, F.R.SS.L. & E., Professor of Anatomy in the University of Edinburgh. (With Two Plates.) 307

PART III.

XIX. *On the Total Intensity of Interfering Light.* By Professor STOKES, 317

XX. *Some Observations on the Charr* (Salmo umbla), *relating chiefly to its Generation and Early Life.* By JOHN DAVY, M.D., F.R.SS. L. & E., Inspector-General of Army Hospitals, . . 326

XXI. *On the Total Eclipse of the Sun, on July* 28, 1851, *observed at Göteborg; with a Description of a new Position Micrometer.* By WILLIAM SWAN, F.R.S.E. (With a Plate.) . . . 335

XXII. *Researches on some of the Crystalline Constituents of Opium.* By THOMAS ANDERSON, M.D., F.R.S.E., . . . 347

XXIII. *On a Necessary Correction to the Observed Height of the Barometer depending upon the Force of the Wind.* By Captain HENRY JAMES, R.E., F.R.S., M.R.I.A., F.G.S., &c. 377

XXIV. *Defence of the Doctrine of Vital Affinity.* By WILLIAM PULTENEY ALISON, M.D., &c. &c., Professor of the Practice of Medicine in the University of Edinburgh, ... 385

XXV. *On Meconic Acid and some of its Derivatives.* By Mr HENRY HOW, Assistant to Dr ANDERSON. Communicated by Dr T. ANDERSON, ... 401

XXVI. *Notice of an Antique Marble Bust.* By ANDREW COVENTRY, Esq., 417

PAGE

XXVII. *On the Centrifugal Theory of Elasticity, and its Connection with the Theory of Heat.* By Wm. J. M. Rankine, C.E., F.R.S.E., F.R.S.S.A., &c. 425

XXVIII. *On the Computation of the Specific Heat of Liquid Water at various Temperatures, from the Experiments of* M. Regnault. By Wm. J. M. Rankine, C.E., F.R.S.E., F.R.S.S.A., &c. 441

XXIX. *On the Red Prominences seen during Total Eclipses of the Sun.* Part I. By William Swan, F.R.S.E., . . . 445

XXX. *On the Red Prominences seen during Total Eclipses of the Sun.* Part II. By William Swan, F.R.S.E. (With a Plate.) 467

XXXI. *On the Dynamical Theory of Heat.* Part V. *On the Quantities of Mechanical Energy contained in a Fluid in Different States as to Temperature and Density.* By William Thomson, M.A., Professor of Natural Philosophy in the University of Glasgow, 475

XXXII. *On two New Processes for the Detection of Fluorine when accompanied by Silica; and on the Presence of Fluorine in Granite, Trap, and other Igneous Rocks, and in the Ashes of Recent and Fossil Plants.* By George Wilson, M.D., . . 483

XXXIII. *Contributions to a Knowledge of the Phenomena of the Zodiacal Light.* By Professor C. Piazzi Smyth. (With a Plate.) 489

XXXIV. *On the Total Solar Eclipse of* 1851. By Professor C. Piazzi Smyth. (With a Plate.) 503

PART IV.

XXXV *Observations on the Speculations of* Dr Brown *and other recent Metaphysicians, regarding the Exercise of the Senses.* By Professor W. P. Alison, ... 513

XXXVI. *Summation of a Compound Series, and its Application to a Problem in Probabilities.* By Bishop Terrot, . . 541

PAGE

XXXVII. *On the Optical Phenomena and Crystallisation of Tourmaline, Titanium, and Quartz, within Mica, Amethyst, and Topaz.* By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., and V.P.R.S. Edin. (With a Plate.) . . . 547

XXXVIII. *On the Production of Crystalline Structure in Crystallised Powders, by Compression and Traction.* By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., V.P.R.S. Edin. . . . 555

XXXIX. *On the Absolute Zero of the Perfect Gas Thermometer; being a Note to a Paper on the Mechanical Action of Heat.* By WM. J. M. RANKINE, C.E., F.R.S.E., F.R.S.S.A., &c. . 561

XL. *On the Mechanical Action of Heat.* By WM. J. M. RANKINE, C.E., F.R.S.E., F.R.S.S.A., &c., . . . 565

XLI. *On Nitric Acid as a Source of the Nitrogen found in Plants.* By GEORGE WILSON, M.D., ... 591

XLII. *Some Observations on Fish in relation to Diet.* By JOHN DAVY, M.D., F.R.S. Lond. & Ed., Inspector-General of Army Hospitals, ... 599

XLIII. *On Circular Crystals.* By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., and V.P.R.S. Edin. (With Two Plates.) . 607

Proceedings at Statutory General Meetings, &c., ... 625
List of the present Ordinary Members, in the order of their Election, . 634
List of Non-Resident and Foreign Members, elected under the Old Laws, . 641
Honorary Fellows, ... 641
Fellows Deceased, Resigned, or Cancelled, from 1849 *to* 1853, . 643
Public Institutions, &c., entitled to receive the Transactions and Proceedings of the Society, ... 645
List of Donations, continued from Vol. XVI., page 648, . . . 647
Index, ... 665
Laws of the Society,

CONDITIONS OF THE KEITH PRIZE.

This Prize, the interest of a sum which now amounts to about £800, left by the late ALEXANDER KEITH, Esq. of Ravelston and Dunnottar, will be awarded by the President and Council of the Royal Society of Edinburgh, on the following conditions :—

I. The author of the best paper on a scientific subject (preference being, in all cases given to a paper containing an important discovery in science made in any part of the world), communicated in the first instance to the Royal Society during the sessions 1851–2, 1852–3, or any two succeeding sessions, shall be entitled to the biennial interest of the KEITH FUND, accruing in the respective periods.*

II. The form of the Prize shall be a Gold Medal, of not more than Fifteen Guineas value. The remainder of the sum shall be given in money, to be spent in Plate or otherwise, at the discretion of the receiver.

III. The award being duly intimated to the receiver of the Prize, he is to apply forthwith to the Treasurer of the Society for payment of it; and, failing to do so within six months of the date of the intimation, he shall forfeit the money, but shall be entitled to receive the Medal.

* The proceeds of all preceding biennial periods have, in accordance with the decision of the President and Council, been either awarded to scientific individuals, or added to the capital sum.

NATURAL HISTORY.

ERRATA.

Page 64, line 23, *for* 48 *read* 56.
... 71, ... 7 from the bottom, *for* 215 *read* 216.
... 74, ... 7 from the top, *for* 20 *read* 21.
... 74, ... 10 from the top, *for* 5·23 *read* 8·23.
... 78, ... 18 from the top, *for* 8·662 *read* 8·62.

BRITISH
NATURAL HISTORY

MAP
OF PART OF THE
VIVARAIS (ARDÊCHE)
Showing
THE LIMITS OF THE TRUE VOLCANIC FORMATIONS.
Ribaldasse
Besson
Tredos
Chamrhades
le Pradut
Champol
St Andeol
la Mindre
Sictrou
le Pratt du Bois
Gerbier des Jones
la Valliere
Source de la Loire
Tanus
Mezenc
Masmont
Rudet
Lauvond
Bourlatiers
Perrenarde
la Chamy
Rambaud
Lesaunes
Mezilhac
Fontfreide
le Serailler
Combechaude
Sauvezac
Ste Eulalie
les Moulines
Fiollonge Volcan
Sagnes
le Chateau
Chambonet
Bernouard
Pas de Pradal
Usclades
Pallas Longue
Lespeyres
Chambaran
le Pont de la Tor
Pechrol
Issarsat
Chastagnas
la Viole
le Mazayer
Pic de l'Etoile
le Bouchet
Avenac
Saussac
le Villaret
le Chambon
Banzon Volcan
le Pal Volcan
Perverenche
Prunaret
les Oulettes
BURZET
le Chambon
le Roux
la Valette
la Soulier
Montelus
Pranginal
la Bastide
Ayzac
Cumie d'Ayzac
Genestelle
(Old Basalt)
Antraigues
Lespradons
MONTPEZAT
Montpezat
St Pierre de Colombier
Juvinas
le Pratt
le Libauriez
les Champeaux
le Plot
Augerome
le Rigaud
le Fau
la Graveyne Volcan
Mayras
Tlnez
Mouleyrès Volcan
le Croizet
Terrenat
la Tuile
St Theofrede
le Buis
Chandons
Champignon
Nozet
Fugerelle
Vallée de Chadenac
la Motte
Nozac Volcan
le Souquet
la Serre
Rousset
Laudreste
Springerie
Baussers
Montell
Fabras
Nimgles
Teirenc
Combalet
Eschandol
VALS
la Pierre
Justas
Coste Chastanier
la Levade
le Pratt
Bourregard
le Regade Blanche
Brechignac
le Ramadet
Uzel
Lestouries
Fabrias
Myrte
Salendre
Champredon
Prades
Martier
Rivaudat
le Fachat
JAUJAC
le Rocher
les Rayes
Tourret
Fontbonne
Mercuer
Lauchon
Rompude
Lozuel
la Souche
Boudris
la Destourbe
Coupe de Joujac
les Berceaux
Burat
la Terrisse
le Cros
St Antoine
AUBENAS
la Clemenderie
Broute
Boudrire
Vignal
Leres
Saudelerie
Rabertiol
Couderal
les Barres
St Cirgues de Joujac
Vallet
Geneve
Miles
Kilometres
W. & A. K. Johnston, Edinr.

BRITISH MUSEUM
NATURAL HISTORY

Fig. 1 p. 4 The Mezenc

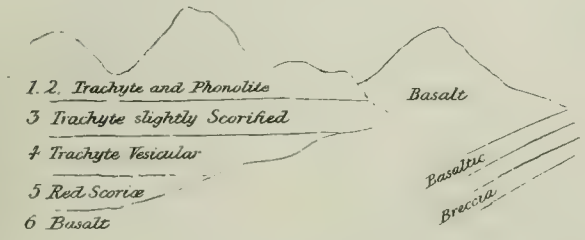


Fig 2 p. 11 Near Pont de la Beaume

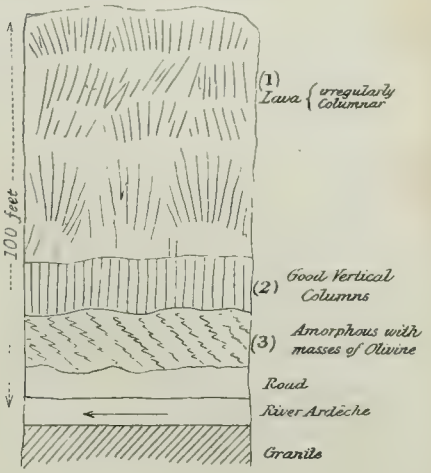
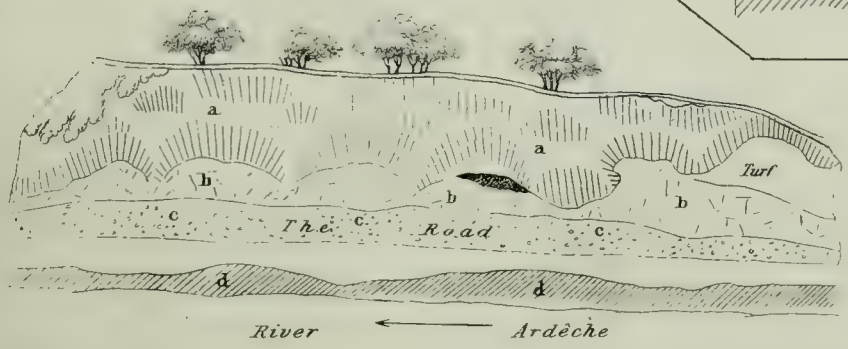


Fig 3 p. 9 Pont de la Beaume



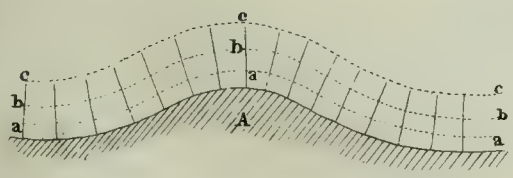


Fig. 4 p 10

Fig 6 p 15
Section of Lava-Cliff of the Alignon

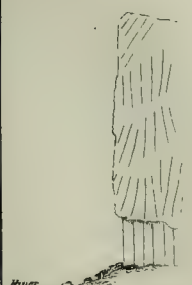


Fig. 7 p. 15 & 29
Near Burzet

Fig. 8 p. 15

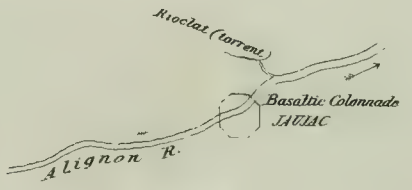


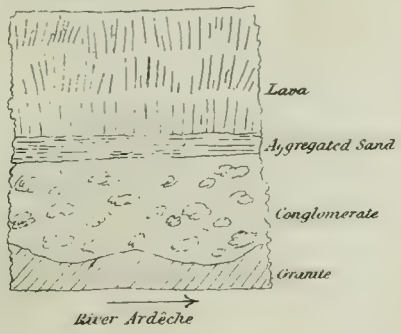
Fig 5 p 16 Environs of Jaujac

Explanation — A Greiss with Granite veins. B Primitive, with Volcanic facing C Hill of Sandstone + ++ points marking the continuity of the Sandstone → Course of the Lava

W. & A. K. Johnston, lith. Edin.r

NATURAL HISTORY

Fig 1. p 17 Section at Neyrac



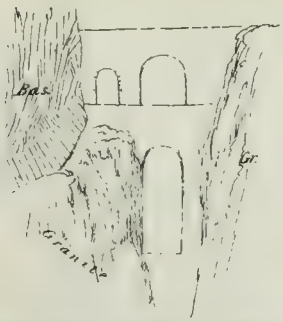
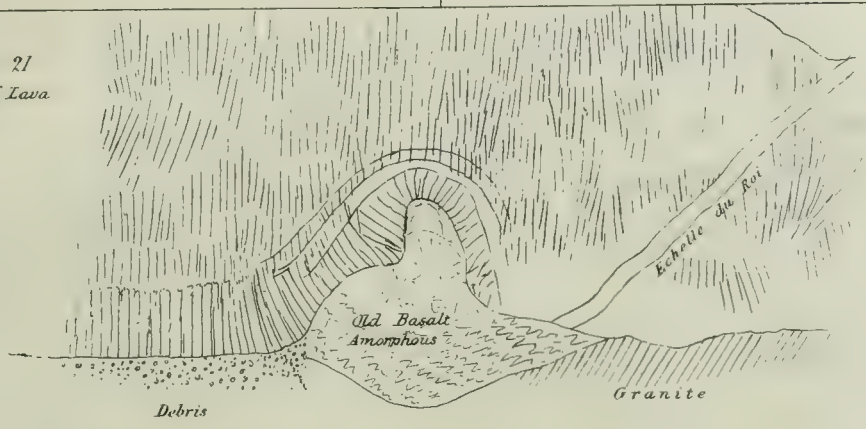


Fig 2. p 20 La Gueule d'Enfer. Thuez

Fig 3 p 21
Superposition of Lava near Thuez



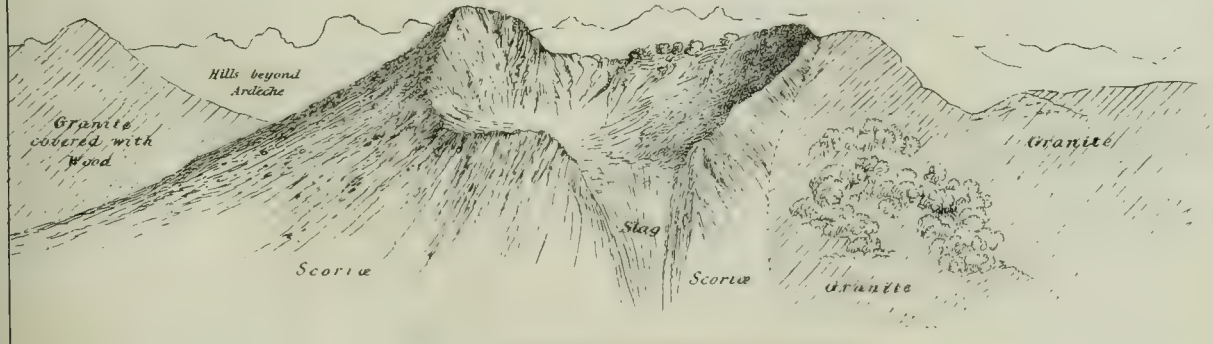


Fig 4 p 22 Gravenne of Montpezat from the North

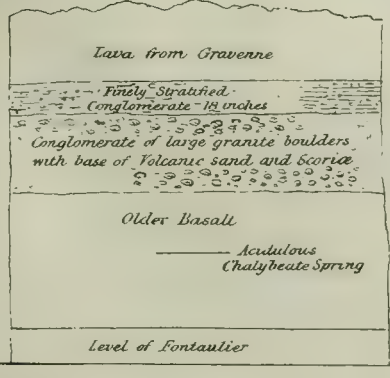
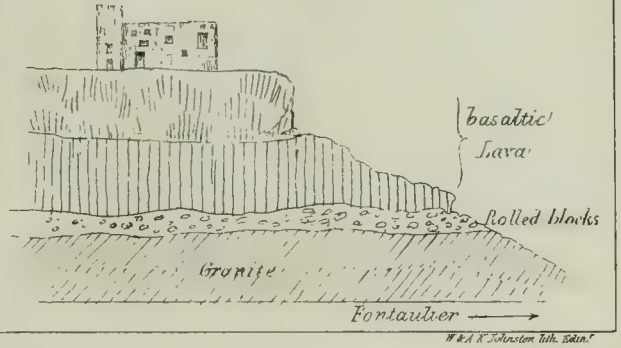

Lava from Gravenne
Finely Stratified Conglomerate 18 inches
Conglomerate of large granite boulders with base of Volcanic sand and Scoriæ
Older Basalt
Acidulous Chalybeate Spring
Level of Fontaulier

Fig 5 p 23
Section at Bridge of Auliere near Montpezat

Fig 6. p 23 Castle of Pourchivol.



W & A K Johnston Lith. Edin.r

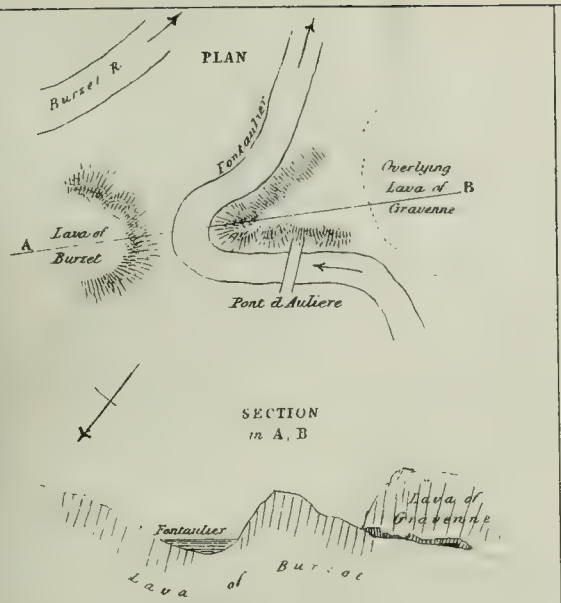


Fig 1. p. 23
Superposition of Lavas in Valley of Montpezat.

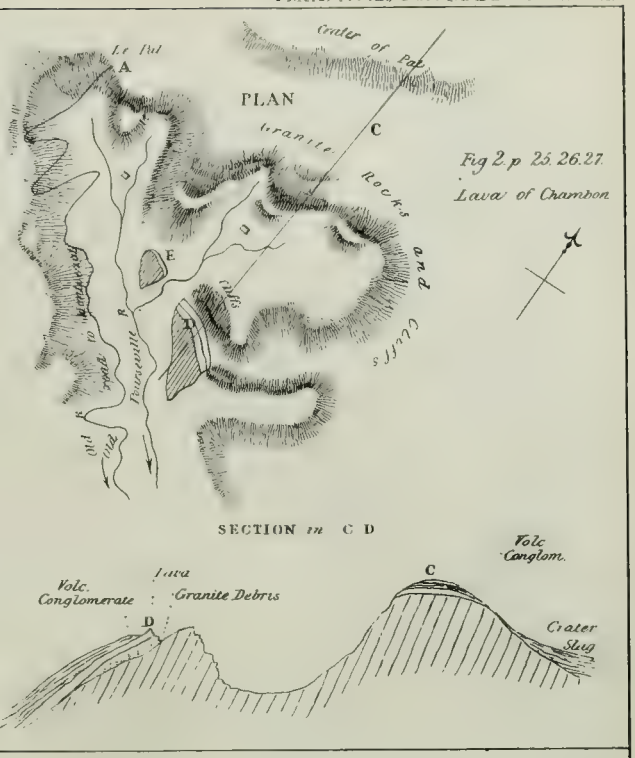
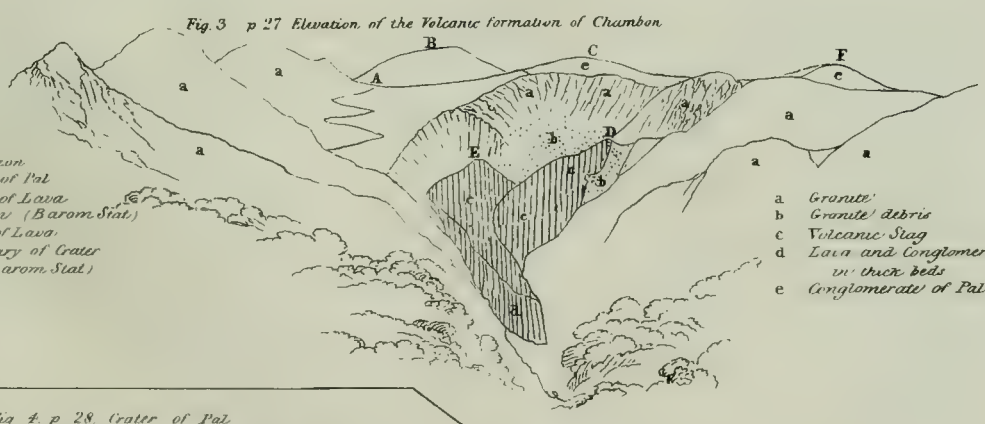


Fig 2. p. 25. 26. 27.
Lava of Chambon

Fig. 3 p. 27 Elevation of the Volcanic formation of Chambon

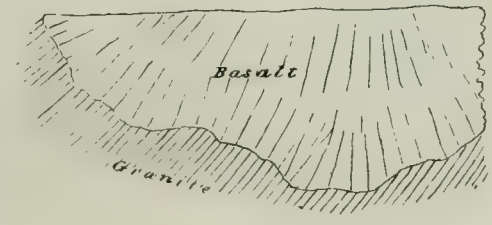


A Col of Pal
B Volcano of Beauzon
C Edge of Crater of Pal
D Highest point of Lava of Chambon (Barom. Stat.)
E Second Mass of Lava.
F Highest boundary of Crater of Pal (Barom. Stat.)

a Granite
b Granite debris
c Volcanic Stag
d Lava and Conglomerate in thick beds
e Conglomerate of Pal

Fig 4. p. 28. Crater of Pal.

Fig 5 Near Burzet. p. 29



W. & A. K. Johnston's lith. Edin.t

NATURAL HISTORY.

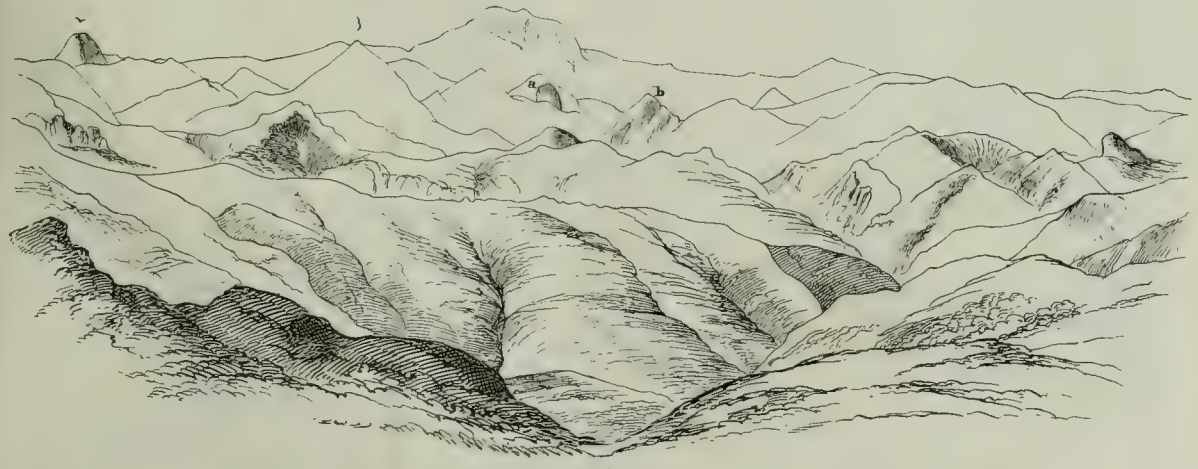
Fig. 1 p 36 Pic de l'Etoile

Fig. 2. p 34 Section in the course of the Volane near Antraigues.

Fig 3. p. 33 Crater of Fiollonge
a Cascade of Raipis b Small Cascade
---- Limit of Volcanic formation

Fig 4 Crater of Fiollonge — Elevation
p. 33

Fig 5. p. 6. and 33 View from Mesilhac



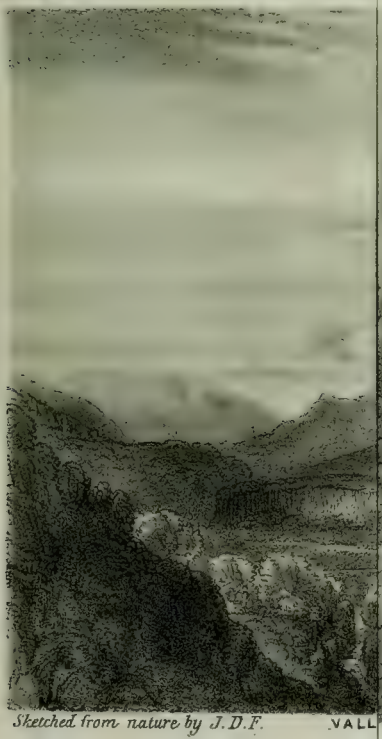
Gerbier des Jones. Charac Mézenc a Roche des Sautrons. b La Pinede

NATURAL HISTORY.

Fig 1 BASALTS AT ANTR

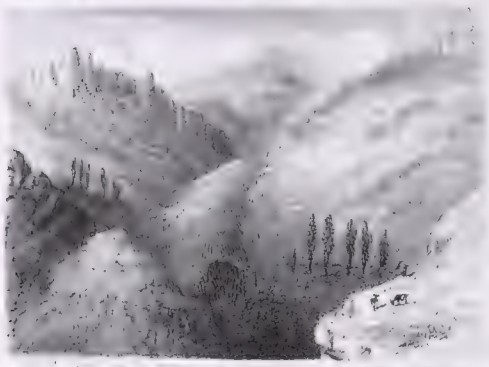
Fig 2 PIC DE L ETOILE & LAVA

Fig 4 CASCADE OF RAIPIS. Page 31



Sketched from nature by J.D.F. VALL

V. DE MOULEYRES. Lithogs by W. & A.K. Johnston Edin.



PANORAMA OF THUEYS.

TRANSACTIONS.

I.—*On the Volcanic Geology of the Vivarais* (*Ardêche*). By JAMES D. FORBES, Esq., F.R.S., Sec. R. S. Ed., Professor of Natural Philosophy in the University of Edinburgh.

(Read 3d and 17th January 1848.)

THE limited district of country which I am about to describe, is one of those which may rank amongst the least frequented in the civilized part of Europe, yet which might justly claim for France the character of romantic beauty which travellers on her beaten highways commonly, and not without reason, deny to her.

The modern department of the Ardêche, corresponding in part to the ancient province of the Vivarais, includes country of very dissimilar features, the southern and eastern part, forming the right bank of the Rhone near Viviers, being comparatively flat; whilst the north-western boundary is the irregular chain of the Cevennes, including the localities more immediately to be described. This chain is not so remarkable for its absolute height, although that be considerable, rising at the Mont Mezenc, in the neighbouring department of the Haute Loire, to an elevation of 5750 English feet above the sea, as from forming the separation of a remarkably elevated tract stretching to the north and west, and which suddenly subsides, at the point of which we now speak, into the wide champaign country of the Lower Rhone, possessing a very different aspect, soil, climate, and population.

The high ground, or *plateau*, of which we have spoken, being thinly peopled, bleak, and steril (in its general character), compared to the fertile and vine-clad banks of the Rhone and Saone, immediately to the eastward, is but little traversed. In fact, only one great road passes through it, the post-road from Paris to Perpignan. It will readily be understood, also, why the Cevennes Mountains themselves are rarely visited, being left between this great road and the more

usual thoroughfare of the Rhone, down which travellers and tourists glide by thousands, without spending a thought upon the intricate country which lies on their right, their attention being rather attracted to the more striking features of the outlying portions of the Alpine chain on the left, where the noble outlines of the mountains of Dauphiné hold out a more tempting prospect of romantic scenery and of geological interest.

Nevertheless, from a very early period of the revival of geological studies in these latter times, the Provinces of the Ardêche and Haute Loire (the ancient Vivarais and Velay) attracted considerable notice. When men were no longer content to ascribe, with VOLTAIRE, the presence of extraneous fossils in mountain rocks to the passage of pilgrims with their scallop shells,—or with others, the scoriæ of ancient volcanoes, covering an area of many square miles, to the iron forges of the Romans,—they began, very properly, to compare the marks of the most apparently recent changes of the earth's surface, not however belonging to the historic period, with those going on under our own eyes, such as the eruptions of volcanoes, and by clearly establishing identity of effects in some cases, were led to almost irresistible conclusions from analogy in others. Thus the presence of volcanic craters, scoriæ, and lava currents, in several parts of central France, could not be doubted by any one who had seen the burnt ground of Etna or Vesuvius. The phenomena should rather be called identical than analogous; but the argument of analogy from the lavas thus detected, to the volcanic origin of lava-like stone or basalt in many countries where no red scoriæ, no declivitous streams of once melted matter, and no indubitable craters could be found, opened up a field for more prolonged study, and more cautious generalization. In this department FAUJAS DE ST FOND, a native of the Vivarais, distinguished himself; and with great industry, and considerable, though not invariable judgment, he set about identifying the features of the unquestionable lava streams of his own country with more obscure, because more ancient ejections of melted matter forming rocks on the surface, not only there, but in distant countries, particularly in Great Britain, whither he made a journey on purpose. To establish indubitably the connection of basalts with lavas was the main object of his large work in folio, on the Extinct Volcanoes of the Vivarais and Velay, published in 1778,* which contains observations of merit, and descriptions generally exact, notwithstanding the rudeness of the engravings by which they are illustrated. This work contains, perhaps, the most complete description of the volcanic district of the department of the Ardêche which has yet been published, and the ample narrative of FAUJAS has been the guide of every subsequent explorer (and they have not been numerous) of this remarkable country. The circumstance of proximity of situation, which led FAUJAS to explore the hills of his own neighbourhood (for

* Recherches sur les Volcans eteints du Vivarais et du Velay, avec un discours sur les volcans brûlans, &c. Grenoble et Paris, 1778.

he resided at Montelimart, exactly upon the opposite bank of the Rhone), subsequently turned the current of attention to a different district, more accessible to tourists, nearer to Paris, and in the close vicinity of a great provincial town. Clermont and its environs, including the Puy de Dôme, naturally withdrew geologists from the remoter and more scattered volcanic features of the Southern Cevennes, and so much has been written and published upon Auvergne proper, as to render any attempt at addition (at least in the way of general description) altogether superfluous. M. BERTRAND'S accurate local descriptions and map of the singular basin of Le Puy (the ancient Velay), and the masterly pencil of Mr SCROPE, have, in a great measure, exhausted the descriptive geology of that most curious, but most difficult field of study. An easier, but rather more neglected subject remained in the province of the Vivarais, the favourite ground of FAUJAS, to which I acknowledge that I was first attracted by the panorama of the basaltic colonnade of Jaujac in Mr SCROPE'S incomparable atlas. Having previously inspected, for my own instruction, the other four great volcanic centres of this region of France, viz., the Monts Dôme, the Monts D'Or, the Cantal, and Le Puy, I meant to finish, as my predecessors had done, with a hasty survey of Vivarais. But I found there a united attraction of scenery and geology, together with that isolation and remoteness which lends a peculiar, though doubtless a selfish charm to a prize which we imagine that others have, in some degree, overlooked, which caused me to fix my quarters in the very first village which I reached, and again, two years later (in 1841), to revisit every point of geological interest, to extend my notes, and to prepare a map and drawings of the volcanic phenomena. These were intended to have been at once reduced into a digested form, and published in the Transactions of one of our Societies ; but, in the same year, a fresh subject of interest was opened to me, and for a time withdrew my attention entirely from any other voluntary pursuit requiring much leisure ; and since that time the theory of glaciers has occupied nearly all my spare moments.* I now resume my original intention of describing the ancient volcanoes of the Vivarais, with the hope of being able to infuse into the general reader some small share of the admiration with which my first visit filled me, and which a second did not abate. I shall first describe the track by which I originally entered these valleys, as serving to point out the circumstances of contrast to which I have above alluded.

The spring of 1839 was late and cold, and in France and the basin of Le Puy, a town situated 625 metres, or nearly 2000 English feet above the sea,† was not the first place to feel the influence of summer. Notwithstanding its great absolute height, the country rises still higher in every direction, save the narrow gorge by which the river Loire struggles out of the circuit of lofty hills forming

* These pages were written in 1847.

† BERTRAND.

three or four pretty distinct ranges which enclose the basin, and which contribute to injure its climate; for there is little wood, and the winds from every quarter sweep unchecked over the extensive and bare *plateaux* of granite and basalt of which it is chiefly composed.* On *the 25th May* I was exposed to a piercing wind, having a temperature of 36°, with frequent showers of hail. A few days later I engaged a horse to carry me across the highest part of the range of the Cevennes, and the sources of the Loire, down to the valleys of the Ardêche. My kind friend M. Bertrand had given me a route which should embrace the most interesting geological points; and with his map as a guide, I started alone with a pony, to sleep at the foot of the Mont Mezenc, at a village bearing the unpromising name of Fay-le-froid. Though it was the last day but one of May, little appearance of spring was visible; indeed, it had hardly an opportunity of making any impression on the singularly bare and rugged features of a country nearly destitute of trees, and often covered for miles with brown angular fragments of basalt.

The position of Fay-le-froid, a meagre village of little more than a single street, is somewhat greener; it lies upon the northern slope of the Mont Mezenc, and near the church is a bed of basalt, containing fragments of trachyte and granite, with abundance of olivine. Early next morning I was on the gentle ascent which leads in two hours to the summit of the Mont Mezenc. A fog prevented me from enjoying the view,† but it afterwards cleared away sufficiently to enable me to examine the geological section below the point called *La Croix de Boutières*, which is 800 feet vertically lower than the top of the Mezenc, and is situated a little to the south of it. Here the escarpment to the south-east forms a sort of imperfect amphitheatre, in which the Salliouse (a rivulet joining the Erieux, a tributary of the Rhone) takes its rise. This hollow, sometimes called *Le Cirque de Cluzels*, presents a section which has justly obtained some celebrity amongst geologists. The peculiarity which it presents is the undoubted *superposition* of trachyte and phonolite or clinkstone (which are felspathic lavas), to common basalt and vesicular scoriæ. The section which I obtained, and from which I took specimens, is shewn in Plate II., fig. 1. There is no proper superposition of trachyte to phonolite; the latter appears to pass into the former, and sometimes to form veins in it. I have no doubt that the trachyte is scorified and rendered vesicular and ochrey by the heat of the basalt injected from below. This appears to be the utmost which can be legitimately inferred from this section; and it is so far a satisfactory conclusion, since it does not contradict the generally established view of the posteriority of basaltic to felspathic lavas, which rule besides receives, in this immediate neighbourhood, so unquestionable a support from

* See Mr Scrope's Panorama from the Montagne d'Ours.

† In clear weather Mont Blanc is visible from hence. *Bertrand, Description du Puy, &c.*, p. 124.

the fragments of trachyte found, as already mentioned, completely imbedded in the basalt, near the church at Fay-le-froid.

That the Mountain of Mezenc is situated near what was once a centre of volcanic action is highly probable, not only from being the most elevated summit of the whole country, and itself composed of volcanic rocks, but from the manner in which the basaltic *plateaux* which have since been broken up, and in great part removed by denuding causes, may be traced from the very environs of Le Puy up to this chain, and also from the peculiar evidences of local fire which the torrefied materials exhibit in the section just presented. I am, however, disposed to agree with those geologists who consider that since the production (perhaps in a good measure by *elevation*) of the Mont Mezenc, the *contour* of the ground has been so completely altered and disfigured as to leave no ground for inferring that we are to trace in its lineaments the actual point of ejection, still less that the *Cirque de Clusels* or *Boutières* is really a "crater of elevation," for it appears to want the essential characters of a crater at all. It would be difficult to prove that the strata (ill-defined even where they exist) dip away universally from a common centre; for only the northern and western portions of the supposed circus can be found; and the argument which has been drawn* from the precipitousness of the rocks in this place compared to their gentle slope in other directions is worth little, since the same reasoning would apply to the whole eastern flank of the range, between Mont Mezenc and the Gerbier des Joncs, compared to the western, which depends upon some general, but probably posterior cause of denudation of which I can give no account. That the expansive action of the basalt, whilst fluid, elevated the previously existing beds of trachyte, and thus contributed to give to the Mezenc its present height, I think there can be no reasonable doubt; and the dikes of basalt occurring here and elsewhere appear to confirm this opinion. It is, however, worthy of note, that if this be cited as an argument in favour of "elevation craters," and as confirming the usual chronology of volcanic rocks of different mineral characters, in the Cantal at least, we find a mountain of phonolite, the Puy Griou, subjacent to the basalts which it is supposed to have elevated.

Whilst I admit, with M. Bertrand, that immense lava streams proceeded from *some point* or fissure near the Mont Mezenc, and flooded the fresh-water formations of a great part of the basin of Le Puy, I should hesitate before ascribing, with Mr Scrope, the immense basaltic plateaux of the Coyrons in the lower Vivarais, with the least degree of certainty, or even of probability, to an eruption of the Mezenc.† Amongst other arguments, the numerous *dikes* of basalt, traversing granite, and other formations in this country, seem to shew that the out-

* *Burat* Terrains Volcaniques de la France Centrale, p. 230.

† Mr Scrope's General Map gives an altogether erroneous idea of the proximity and mutual dependence of the basaltic formations of the Mezenc and those of the Coyrons.

lets of melted matter were far more widely spread than this idea of a central volcano would permit. Perhaps it may added that the close proximity of the granite to the surface, wherever the volcanic materials have been wasted sufficiently to expose it, and *that* particularly in the valley of the Salliouse, as shewn in M. Bertrand's Geological Map, almost close to the *Cirque de Clusels*, seems at variance with the supposition that Mont Mezenc is to be regarded as the sole volcanic centre which gave rise to such widely-spread phenomena.

I must still more emphatically dissent from the theory that the hills of phonolite, forming the north-east barrier of the basin of Le Puy, are the relics of a felspathic eruption proceeding from the Mezenc.* Not to multiply arguments against so very bold, and, in itself, I must think, so improbable a supposition, I will only observe, *first*, the rarity of any appearances, in trachytic mountains, indicating that the matter of which they are composed has been sufficiently liquid to *flow*, after the manner of a current, over any extent of country. *Secondly*, the absence of any appearance of a current in the chain of detached sugar-loaf shaped eminences here referred to. *Thirdly*, that if it were a current, it would not have occupied the *axis* of a granitic elevation, constituting one of the orographic features of the country; and, *lastly*, that the direction of the chain, coincident with that of other important chains, and especially of the chain separating the Allier and the Loire, and the chain of La Margeride, beyond the former, evidently points out an axis of elevation which, in other instances, in this singular country, is the fertile source of local explosions and eruptions, to which these insulated phonolitic peaks may, in my opinion, be, with far more probability, ascribed.

The route, southwards from the Mezenc, presents some singular features. We follow a ridge, sometimes composed of trachyte or phonolite, sometimes of basalt, which separates the gentle slopes towards the basin of the Loire and the more precipitous ones towards that of the Rhone. The views, in the latter direction, are eminently singular, and even romantic; a country intersected with deep ravines, and divided by deep hilly ranges, often capped by fantastic summits, stretches away for many miles in the direction of Chalançon and La Voulte. Some idea of the scenery may be formed from the sketch in Plate V., fig. 5, taken from a spot commanding a view *towards* the Mezenc, *across* the country now referred to. The form of the phonolitic summits (those whose names are given on the drawing) marks their composition in a manner scarcely to be mistaken; but the basis of the whole is granite or gneiss, as pointed out in M. Bertrand's map, and in the new Geological Map of France. It is pro-

* "The uniformly progressive declination of this series of phonolitic summits from the Mezenc to the bed of the river where they terminate, proves them, in my opinion, to be the remains of a single enormous lava current prior in date to the excavation of the actual channel of the Loire, and far the most considerable in bulk and extent of any which I have had occasion to observe in the phlegræan fields of France."—Scrope's *Geology of Central France*, p. 129.

bable that this district has been but imperfectly explored; the whole circumference of the Mezenc presents a degree of sterility and desertion almost repulsive. About an hour's walk from the Mezenc is La Clêde, a small public-house where refreshment may be had, and the neighbouring old monastery of Bonnefoi, which belonged to the Chartreux, but is now the remote habitation of a private gentleman, would afford an invaluable centre for excursions to a geologist proposing to examine the neighbourhood. Two hours after leaving the Croix de Boutières, I arrived at the foot of the phonolitic peak of the *Gerbier des Joncs*, which resembles the Pierre de Bar, near Le Puy. The Gerbier des Joncs is best known as the point where the river Loire is understood to take its rise, and where it at once commences its long and tortuous course, a course so involved, that between this point and the defile of Chamalières, by which it issues from the basin of Le Puy, it traverses a length of 250 kilomètres, or 170 English miles, whilst the *direct* distance is not above *one-fifth* part so great.* The height of the source of the Loire is 4505 English feet, which is about 2900 above the defile just mentioned.

After passing the villages of St Eulalie and Usclades, I ascended through the forest of Bauzon, at the foot of the volcanic cone of the same name,† which I afterwards examined more particularly. A moderate descent brought me to the head of the valley of the Fontaulier, and to the singular volcanic crater of Pal, which forms a cup in the midst of granite mountains, never having raised a cone of ashes. The road then rises slightly to the col or passage separating the sources of the Fontaulier from those of the Pourseuille, which descends to the valley of Montpezat. From this point a magnificent view opens. A steep descent of 2000 feet leads to the village of Montpezat, surrounded with verdure, and placed at the entrance of the Bas Vivarais, a perfect contrast to the cheerless highlands of the Velay. It was almost like a peep from the Alps into the warm valleys of Italy, or like some of the pleasant scenes in the Pyrenees. In fine contrast with the deep green of the chestnut-clad slopes, rose the warm reddish-brown summit of the Gravenne of Montpezat, a volcano so fresh in its appearance as to seem as if scarcely yet extinct. Immediately on the left, also, appeared volcanic relics still fresh and cindery, which contrasted with the sombre hue of the granite rocks on which they were spread. From Montpezat almost every part of the volcanic district of the Vivarais can be conveniently reached; and, in point of accommodation, there is not much choice elsewhere, and probably it may be long before it is improved.

But it will assist the clearness of my descriptions if I commence my account of the volcanoes and the surrounding scenery, not from the *centre* of the district but from one extremity; and, as these all lie on or near some of the numerous streams which rise amidst the heights of the Cevennes, and which unite a good

* Burat, p. 158.

† From this point of the description, the map, Plate I., may be consulted.

many miles to the south of Montpezat to form one considerable river,—the Ardêche, which gives its name to the department,—it will be most convenient to suppose the traveller ascending the Ardêche from the town of Aubenas, one of the most considerable in the province, and arriving at a point called Pont de la Beaume (see the map), where the river Fontaulier, whose rise we have taken notice of, joins the Ardêche, having previously passed close to the village of Montpezat, and immediately under the volcano of the same name, subsequently receiving two minor tributaries, which we shall presently have to describe.

From the neighbourhood of the Pont de la Beaume, the greater part of the valleys of which we are to speak diverge almost like the rays of a fan. As we look *up* the course of the Ardêche, Montpezat occupies nearly the centre of the fan. Now, in all these valleys there is a remarkable uniformity of constitution, and, in some respects, of general appearance. The substratum of the whole is a primitive rock, granite, or in some places gneiss. The distinction is not very important in connection with the phenomena which we are to describe; and I have not attempted to determine the limits of the more crystalline granites, as distinguished from those whose slaty structure may allow them to be considered as having a regular cleavage and direction of beds, subordinate to which hornblende slate also occurs. When I speak, therefore, of granite forming the predominant rock (*uncoloured* on the map), I would not be understood to do so always with precise mineralogical accuracy. So far as I know, there is no peculiarity in the volcanic action in the granitic districts, compared with that in gneiss. A small patch of the coal-formation appears near Jaujac on the Alignon, but it is surrounded by granite or gneiss, which again is succeeded by the lias or oolite formations near Aubenas. The coal-formation occurs in patches nearly all round the great primitive *plateau* of central France. It is extensively worked on the east side at St Ettienne; and, in some places at least, its strata lie horizontally against the granite, shewing the anterior date of the elevation of the latter.* The valleys we have to describe farther agree in this extraordinary particular,—that, *as surely as they contain water they contain a stream of lava or basalt, or the remains of one*, which stream has accommodated itself perfectly to the sinuosities of the channel of primitive rock in which it has run, the possession of which it contests yard by yard with the water; these lava streams are sometimes attenuated to a surprising degree, leaving but small relics for the space of miles; in other places they accumulate to an astonishing thickness and breadth, altering the configuration of the valley, the stagnant pool of lava having, in the first instance, created a lake of water, and compelling the river to alter its course and to excavate a new channel. The tributary of each valley commonly unites with others, accumulating near the points of junction; but the

* Burat, p. 4.

heat being gradually spent, the currents have lost their mobility, and do not at all extend themselves into the plains. When we trace these lava currents to their sources, the result is uniformly the discovery of a crater, often formed in the breast of a mound of cinders, whose fiery-red colour will bear a comparison, in point of apparent freshness, with any of those which stud the flanks of Vesuvius, or the more prolific Etna; and, in very many instances, the precise point of ejection of the lava may be ascertained with the most extreme nicety, and all the accidents of its subsequent course chronologically traced. Thus, every individual eruption has written, as it were, its own history, although the relative dates cannot always be determined. It is an inquiry not a little interesting (at least upon the spot) to collect these rude hieroglyphics, which form a chapter of the ancient records of our globe, and register events amongst the most recent of *geological* change, yet of which no trace or tradition is to be found amongst the histories of the Gallic nations.

The hamlet of La Beaume lies on the right bank of the Ardêche, almost under the shadow of a basaltic colonnade, which stretches parallel to the course of the river, but leaves a level space between the foot of the cliff and the water, along which the public road passes. The section (Plate II., fig. 3) is sufficiently remarkable to have attracted attention ever since the days of Faujas St Fond, who has given a view of it, but with great inaccuracy and exaggeration. Our figure shews the lava stream (*a*) invested with a coating of soil, on which is abundant vegetation. The lava rests on a mass of scoriæ *b*, which again reposes upon the debris and vegetable mould *c*, thus marking strongly the comparative recency of the eruption which produced the lava, and the perfectly *natural* and *modern* condition of the valley into which it flowed. The debris and soil rest upon the primitive rock (granite or gneiss) which is exposed in the bed of the river Ardêche.

The contact of the lava and scoriæ (beds *a* and *b*) presents some interesting considerations; and, *first*, to account for the so frequent phenomenon of the superposition of lava to the scoriæ, which, being its scum and refuse, we should rather expect to find upon its surface, we must recall the peculiar manner of progression of those highly viscid lavas, which most abound in scoriæ (for, whilst very fluid, there is little or no scoria deposited). The progression is exceedingly slow, and, according to the usual laws of a tenacious fluid, moving over a rough surface with great friction, the surface moves faster than the bottom of the stream, and the *front* of the wave of lava (that which would be presented to a spectator towards whom it is in the act of descending) being hard and scoriaceous, in consequence of long exposure to the cooling action of the air, is continually thrust *under* the liquid as it slowly struggles on, and its place is supplied by freshly floating scum from the surface, which finally descends the front of

the lava wave, and thus, as it were, unrolls a continuous carpet, over which the more liquid stream flows; here and there breaking through the tenacious but partially yielding and crackling crust by which it is imperfectly confined.* This perfectly explains why we should have such a substratum of scoriæ in this place, which is often wanting below the basalt in the higher and steeper valleys where the lava flowed more rapidly, and it also accounts for the arched forms of the contact-surface, which fig. 3 remarkably displays; these vaults being due to the actual rolling of the lava over the more or less ponderous masses of scoriæ deposited from its own surface. The brick-red colour common in scoriæ beneath lava is to be attributed to the intense heat communicated to them by the lava, *after* they have been covered by it. This heat must have been retained for a prodigious space of time. Brick-red tints are usually produced upon minerals subjected to close or confined heat. The casual removal of the scoriæ has, in some places, left grottoes beneath the vaults. *Secondly*, These vaults also present this remarkable peculiarity, that the columnar structure of the lava (which here, as elsewhere, is best developed near its lower surface) conforms so nicely to the contact-surface with the scoriæ, to which it is always nearly perpendicular, as to give quite the appearance of vaulting stones to the covering of the grottoes. This is a fact so general amongst the lavas of the Vivarais, as to deserve almost to be called universal. It is interesting, as illustrating the development of the *prismatic structure* which was so long supposed to distinguish ancient basalts from true lavas, but which is now universally admitted to characterize both, when the circumstances of cooling are favourable to their production. The fact of the perpendicularity of the columns to the surface of cooling admits of this general illustration,—that if A (Plate II., fig. 4) represents a cold mass of rock overflowed by lava, which gradually loses its heat by contact and conduction, all the points equidistant from the rock, as a, a, a, or b, b, b, or c, c, c, may be conceived to be, at the same moment, in the same condition as regards a tendency to consolidate or to crystallize. Any peculiar action, which depends upon a particular stage of cooling, will therefore affect similarly all the points a, a, a, and so of the rest; that peculiar state of tension which produces the columnar division, will therefore prevail uniformly over any one of these isothermal surfaces (or surfaces equally cooled) at a given time, and will tend to produce its effect everywhere on that surface, and the lines or planes of separation will therefore seem to proceed uniformly from the surface of cooling in a direction perpendicular to it. Or on the less probable hypothesis of the columns being due to real crystallization, the crystals will naturally begin to form at the surface of earliest consolidation,

* That such is the mode of progression of lava streams at a great distance from their origin, or after they have been running for a long time, appears from the descriptions of the best writers on volcanoes. Compare Scrope's description of the lava of Etna of 1819, in his work on Volcanoes, p. 102; and Auldjo's figure of the descending lava waves of 1831, in his Description of Vesuvius, p. 92.

and will shoot outwards, so as to form simultaneously at points equidistant from that surface. The accuracy of the empirical law is greater than any theoretical view could possibly lead us to expect. Varied examples will be quoted in the following pages.

A little way above La Beaume, near the junction of the rivers of Ardêche and Fontaulier, is the picturesque Castle of Mayras, which, however, offers in itself nothing of much interest. Opposite to this castle (on the other side of the Ardêche) the lava cliff continues from the point already described, so that the colonnade of La Beaume owes its origin to the western, and not to the eastern valleys which unite there. There is little doubt, however, that the lava stream of the eastern valley (or that of Montpezat) may be traced at the sharp turn of the road to Thuez, opposite the Chateau de Mayras, where there is evidently an older and inferior stratum of lava below the greater colonnade. A section at this point, of a cliff about 100 feet in height, is given in fig. 2. The bed No. 3, I take to be the lava of the valley of Montpezat. It contains a remarkable quantity of olivine, and is here amorphous: it is completely detached from the superior mass, which is beautifully columnar where it rests upon the older bed, the columns being vertical, since they rest upon a horizontal base. The beds Nos. 1 and 2 might appear also to belong to distinct currents, the upper part shewing but imperfect columns; a close examination shews, however, that the beds (1) and (2) inosculate in such a manner as to leave no doubt of their being due to one and the same eruption, and that the distinction is caused by the accidental manner of their consolidation.

Valley of the Alignon—Jaujac.

A short distance higher up, the Ardêche divides from its tributary the Alignon, the former descending from Thuez, the latter from Jaujac. The great mass of the lava which we are tracing evidently descends from the latter valley, and here we gain the first view of one of the volcanic vents which has furnished the lavas of the lower valleys, but presents from this point no trace of a crater. It is situated on the ridge separating the valleys of the Ardêche and Alignon, and is sometimes called the Volcano of Neyrac, from a village of the former, or Souillols, from one of the latter. And here occurs an interesting question, whether the great *single* lava-flow, extending to the Pont de La Beaume, is due to this volcano, or to that of Jaujac, higher up the Alignon; for it undoubtedly did not come from the valley of Thuez (on the Ardêche). On my first visit in 1839, I was of opinion that the stream might be traced uninterruptedly from the volcano of Jaujac, and that there was no sufficient evidence that the volcano of Neyrac had yielded a considerable stream into the valley of the Alignon, notwithstanding the presumption afforded by its crater (a considerably decayed one) being broken

down (which is commonly due to the pressure of lava) on the south-west side. A very careful examination in 1841 changed my opinion; and, notwithstanding the seeming improbability of the conclusion, we must, I believe, admit that the lava of Jaujac terminates almost exactly where the lava of Neyrac commences, filling the valley to almost the same depth, and with similar matter. And this must be conceded for the following amongst other reasons:—*First*, There is a break in the continuity of the lava stream a little above, and opposite to the village of Souillols, and in the cliff formed by the river. The granite of the country may be traced in the interval. *Secondly*, Up to this point the character of the lava, as displayed continually in the cliff on the right bank of the stream, is remarkably uniform upwards from the point of section (fig. 2) opposite the Castle of Mayras. Only a small portion of the lower part is columnar, surrounded in the greater part of its thickness by basalt nearly amorphous or slightly columnar. The columnar part diminishes in thickness as we ascend the course of the river, and opposite Souillols it is only three or four feet in height. But when the lava cliff reappears after the break alluded to (a break, however, so slight, that it might easily escape notice), it presents a very different front to the river. The cliff is now 130 feet in height,* of which not less than two-thirds, and in some places nearly the whole is composed of a single range of perfectly continuous basaltic pillars; and the level of the prismatic boundary is again gradually depressed, as we approach the undoubted origin of *this* part of the stream, namely, the crater of Jaujac. *Thirdly*, The volcano of Neyrac *does* exhibit a streak of ashes and slag down its southern face. Now the ashes may be seen to pass into the slag, and the slag into the lava of the Alignon near Souillols,—a convincing argument.

We now come to describe more particularly the lava of Jaujac, which extends along the bottom of the valley, from a short distance above the village of that name, to nearly opposite Souillols, at least two miles farther down. All this space has been raised from the natural bed of the stream to a vertical height of perhaps 100 feet on an average, throughout the entire breadth of the valley, and now presents a cultivated and wooded plateau, whose extraneous origin would hardly be suspected but for the deep incision made by the river near the foot of the hills which bound it to the north. This section displays the wonderful colonnade already referred to, of which so correct a representation has been given in Mr Scrope's beautiful work on Central France.

That representation exhibits well the remarkable fact of the gradual *rise* of the lower perfectly-columnar stratum into the higher or imperfect one, in proportion as we follow the section down the valley, or farther from the point of

* Measured by the fall of a stone, and confirmed by the authority of a person who told me that he had measured it with a string.

emission (the *Coupe de Jaujac*, near the village of that name). Near Jaujac, where the cliff may be, perhaps, from 60 to 70 feet high, only one-fourth of the height is occupied by the columnar range; down the course of the valley it rises to one-third and one-half of the height of the cliff (which is also increasing), it then divides into two columnar beds, with an amorphous portion between, and finally presents the magnificent display of unbroken columns in almost its entire height, already referred to.

We have seen, that, sharp as the separation appears of the columns from the amorphous or slightly-columnar part, it is impossible to deny that both are the production of a single eruption. The cause of the abruptness of the separation appears, in the present state of our knowledge, altogether inexplicable. Mr Scrope, indeed, gives the following elucidation, but I doubt whether it can be considered as, in any degree, satisfactory. "[This] singular difference of structure may be accounted for, I conceive, by supposing the elastic vapour, contained in the lava, to have escaped from the upper part, through fissures of retreat, formed in irregular directions as the mass cooled; while the immense pressure, acting on the lower part, would, at the same time, prevent the escape of the contained vapour, and effect its *condensation*, thereby allowing the mass to consolidate in a regular and tranquil manner, such as would facilitate the establishment of straight and vertical axes of contraction, and the formation of very regular hexahedral columnar concretions."* Whatever be the cause, it would appear to become more energetic after the lava has flowed to a considerable distance; and from the regularity with which the limit of the columns follows the sinuosities of the base (see Plate VI., fig. 3), it would seem to depend more upon the rapidity of the cooling action below than upon the superincumbent load of lava.

It is a very curious circumstance that this sharp limit of columnar formations occurs also in circumstances which would appear very different from the volcanic flood of Jaujac, which undoubtedly occurred in the open air exactly like any modern eruption. A recent visit to the Island of Staffa has satisfied me that the basaltic bed in which Fingal's Cave occurs, is constituted precisely like the lavas of the Vivarais, and that the sharp cessation of the columns, and the abrupt transition to a nearly shapeless bed of basalt, is not due to a superposition of other matter, but simply to the limited sphere of action of the crystallizing or concretionary forces; we have there, as at Jaujac, perfect evidence of the contemporaneous formation of both beds; and, indeed, the apparent section of the two would be perfectly identical, the compact part commonly projecting over the columnar part, as in Plate II., fig. 6. Dr MacCulloch, who makes no allusion to the contemporaneity of the beds, or to the cause of their distinction of character, minutely specifies the exact perpendicularity of the principal range of the columns of Staffa

* Geology of Central France, p. 152.

to the plane of the stratum which they occupy, which is inclined about 9° to the horizon.* Some of the pillars are a little twisted in their upper parts, although quite straight below, a phenomenon which gives them an appearance of having yielded under the pressure of the load above them; this occurs a little to the east of Fingal's Cave, and is correctly represented in one of Dr MacCulloch's views of the island. If we compare the lava-cliff of Jaujac with Staffa as a mere columnar display, we must give the former a decided preference. The columns are more numerous, more extended, higher, slenderer, better pointed, and, in every respect, more perfect. It is probable that this, as well as their incomparable superiority to any known product of altogether recent volcanoes, is due to the more perfect composition and fusion of the material of which they are formed.

But this grand phenomenon might have been lost for ever to human sight had not the excavating action of the stream made the *section* which we have been contemplating; and, is it not interesting to inquire in what manner the degradation or destruction of so large a mass of intensely hard rock was effected, and whether it is going on still? Now, as to the present action of the elements, there are abundant proofs that it is going on with great energy. Just below Jaujac, a small stream, named Rioclat, joins the Alignon, and, opposite to their junction, a great mass of the principal lava-cliff fell about three weeks before my visit in 1841. The scale of this operation, which is the removal of about 2000 cubic yards (40 yards in front, 3 from back to front, and 16 high), makes it very interesting, and its recent occurrence, as well as that of another great *éboulement* opposite Souillols, throws light upon the natural mode of proceeding. In both these cases the atmosphere seems to have acted alone. At Jaujac (the case represented in Plate VI., fig. 3), the river did not touch the base of the lava at all; we must, therefore, distinguish two methods of disintegration. 1*st*, The atmospheric water penetrating freely amongst the countless fissures which the imperfect columnar structure of the upper bed presents, detaches it gradually by the usual effect of liquid pressure, more rarely perhaps, by congelation. The principal tendency to fracture being exactly in a vertical direction, the cliff has a continual tendency to instability. A few hundred yards from the spot last described, there was, at the time of my visit, another huge mass in the process of separation from the cliff by a gap already two feet wide at the top. I passed below it during a tremendous thunder-storm, when the rain-water was gushing in torrents from the joints of the pillars, threatening instant precipitation. 2*d*, The *lower* part of the lava being always regularly columnar, and having, therefore, very little lateral cohesion, and probably, like a table with many legs, all the pillars not bearing equal shares of the load, the erosive action of the water must necessarily detach them; and, we almost always, if not invariably, find that the upper part of the cliff pro-

* A description of the Western Islands of Scotland, vol. ii.

jects, as in Plate II., fig. 6, and gravity thus assists the atmospheric causes of disintegration.

To these I would add one consideration as to the *commencement* of erosion. When the form of a lava-bed confines the running water to the centre of a stream, as in fig. 7, we find that it acts extremely slowly;* but if the river takes *one side* of a lava-bed, as in fig. 8, there being a crevice between the granite and the lava, the water must penetrate, and, by its pressure, tend to separate the columns, and to wash them out, so that I know of no *existing* case of water running under the condition last described. But *portions* of the lava often adhere to the granite even on the side of the valley most eroded. This is the case on the bank of the Alignon.

The often excessive fragility of the lava also assists its division and removal. At the *éboulement* of Jaujac the basalt is singularly brittle, almost the whole fallen mass is shattered into bits of a few pounds weight. In texture it slightly resembles pitch-stone, and the lustre is that of animal glue. *The fragments include many pieces of granite.* At the junction of the Alignon and Rioclat is a considerable mass of pure feldspathic granite, not like that of the country, surrounded by, and cased in basaltic columns, which have formed almost as regularly as if it had not been there. This, though very interesting, is conformable to the illustration which we gave of the law of direction of the columns. If it depended upon the contact of the lava with the granite otherwise than as the cooling is thereby affected, the columns might be expected to radiate from the enclosed mass; but as any mass, not enormous, enveloped in such a stream, might acquire the temperature of the melted matter, the whole would cool with reference only to the conditions extraneous to it. The lava extends but little way up the Rioclat, which, however, presents a curious deposit of volcanic ashes, which no doubt must have fallen from the crater of Jaujac.

The *Coupe* de Jaujac (being an exact translation of the word *crater*, by which the ancients denoted a volcanic orifice) is distant only half-a-mile or a mile from the village, in a natural opening or cavity between two primitive mountains, filled with coal-formation sandstone, whose character is well marked by abundant impressions of fossil plants. This formation (in which coal was at one time worked)† is interesting on several accounts, although the occurrence of the volcano in the midst of it cannot but be regarded as entirely accidental. Its extent is small, being nearly confined to the valley of Prades, adjoining that of the Alignon, at least if the Geological Map of France be correct. It is, as already observed, part of a widely-spread but often-interrupted ring of the coal-formation, surrounding the primitive *plateau* of Central France. I examined, very carefully,

* *Examples:*—The tributary on the left of Ardêche near Thuez; lava of Burzet at the cascade near the village.

† On the authority of Faujas St Fond.

the whole exterior of the volcanic cone, and found the sandstone *everywhere*, except in the narrow space occupied by the issue of the lava-stream from the crater (see Plate II., fig. 5), which abuts against a hillock of sandstone, marked C on the plan. It does not, however, appear to reach the banks of the Alignon, and terminates abruptly against the two hills of gneiss, A and B, which form part of the boundary of the valley, which is everywhere of primitive rock. Near A, the junction of the coal-formation and the gneiss is well seen. The strata of sandstone and gneiss resemble each other so much, that, being nearly conformable, it was some time before I could assure myself that the fine granite veins which occur there were not in the coal-formation.

The crater is probably the largest in the Vivarais. It is low, strong, and of an elliptical form, and has burst at one end of the longer axis, being that towards the village of Jaujac. From the firmness and dimension of the lava walls, I presume that it must have been lofty, as is indeed probable, if it contained any large part of the prodigious flow of lava which proceeds from it, and which is evidently the result of one eruption, and probably the only one which this volcano has experienced. The interior of the crater is filled with clay and ashes, which sustain, however, a magnificent growth of chestnut trees. The open lip is narrow, and just gives vent to a stream of fine compact lava with little slag, which then fills the valley; the town of Jaujac stands on the extreme upper part of it. Near the mouth of the crater, and 1531 feet above the sea, is a spring plentifully charged with carbonic acid, whose temperature was 53°·2. A spring, issuing from below the basalt, at the junction of the Alignon and Rioclat, had a temperature of 54°·7 (23d June 1841). These temperatures are both above the mean of the place. The extreme height of the *Coupe* (by the mean of barometrical observations in 1839 and 1841) is 1923 English feet above the sea. The level of the surface of the basaltic formation at Jaujac is about 1400 feet; and the top of the *coulée* of Neyrac, at the junction of the Ardêche and Alignon, is 1117 feet.

Valley of the Ardêche—Thuez.

If, instead of pursuing the course of the Alignon to Jaujac, we follow the principal stream of the Ardêche to Thuez, we first of all seem to leave all traces of basalts in the valley. Very soon, however, patches of volcanic formation appear upon the right of the road; whilst, on our left beyond the river, we have the volcano of Neyrac or Souillols, formerly mentioned as occupying the ridge between the two valleys, and as having unquestionably thrown out its principal stream into that of the Alignon. The summit of the volcano (which is at a height of 2178 feet above the sea, barometrically determined in 1839) commands a very fine view of the upper district of the Vivarais, and of the volcanoes which

occur there, as well as of the valley of Thuez. It has a distinct, though degraded crater, which has given way on the south-eastern side. The sides of the ridge are wooded; but I traced two distinct streams, though of no great dimensions, into the valley of the Ardêche, which evidently came from the crater of Neyrac. They include between them the hamlet which bears that name; and one at least of them (the most westerly) may be traced down the bed of an insignificant watercourse in the granite, down to the valley, where it has formed columnar basalt for a short distance. The other patches of basalt, between this point and the junction of the Alignon, are probably due to this stream.

Opposite to the village of Neyrac, the following section occurs (Plate III., fig. 1), which attracted my attention from the complete state of aggregation of the sand and gravel intervening between the basalt and the granite soil. It is evidently formed by the concreting action of calcareous matter, held in solution by the carbonic acid which occurs abundantly in the neighbourhood upon the detritus of the valley, which it has compacted to such a degree as to form a kind of sandstone. The carbonic acid occurs in a spring rising in a neighbouring meadow, and having a temperature of no less than 78°·5, whilst a small spring, between it and the river, marked only 51°. The elevation of the mineral spring above the sea is 1359 feet. In the side of a granite hill, close to the hamlet of Neyrac, is a dry discharge of carbonic acid gas, producing a suffocating atmosphere, similar to that of the *Grotto del Cane* near Naples, the effects of which have been elaborately described by Faujas and other older writers on the Vivarais.

Continuing to ascend the valley from Neyrac, either by the road or by the river, objects of interest multiply. The slopes on the right are occupied by an immense mass of red cinders, and slaggy lava and ejected bombs, indicating the close proximity of a crater, round the outside of which, indeed, the road winds for some distance,—whilst the valley seems absolutely barred in advance by immense cliffs of basalt, which tower over rugged rocks of granite, amongst which the river struggles. Across it has been thrown an extremely slender and elegant arch, which bears the name of Pont du Diable, or Pont d'Enfer. The basaltic cliffs rise to a height of nearly 300 feet above the level of the stream, and upon the plateau which they form is the mean but charmingly-situated village of Thuez. For variety of outline, luxuriance of vegetation, rich colouring, and romantic forms of ground, few spots can be compared to it; and, on my last visit, disregarding the very indifferent accommodation which it affords, I made it the principal centre of my excursions. A short description will, I hope, tend to give a distinct idea of its situation, and the chief points of picturesque as well as of geological interest.

The reader must imagine the bed of a rapid stream (the Ardêche) to have been worked out through the lapse of ages, by natural operations, to a great depth in a soil of granite,—that near the junction with a tributary stream on the left, a powerful volcano suddenly opened, emitting a torrent of lava which filled

the greater part both of the lateral and principal valley, damming back the river Ardêche so as to form a lake, which was only drained when the unceasing and all-powerful action of water had excavated a channel in the hard basalts, and even in the granites beneath them, leaving a cliff, which has been already described, towering above the walls of primitive rock, between which the stream struggles, forming a scene far more varied and picturesque, though less regular, than the columnade of Jaujac which we have lately quitted.

Dire must have been the confusion which the element of fire wrought in this quiet valley. Scarcely less appalling the confusion and organic change produced by the subsequent action of the water upon the intruded masses which had for a while staid its course. But now all is again tranquil, and the progress of events is marked by almost imperceptible gradation; for now, the lava being nowhere in contact with the stream, the action of the river on the granite may be considered as inappreciable. The platform raised by the lava, and terminated by a tremendous chasm on the side next the valley, is covered by a most luxuriant vegetation. The village, surrounded by its vines and maize, is exactly opposite to the spot whence the volcano of Mouleyres or Thuez must have evacuated the most abundant part of the fiery flood which charged it; the crater itself, which rises to 2026 feet above the sea, is filled (as usual) with splendid chestnut trees, and its porous cone is planted, like the sides of Vesuvius, with productive vineyards; the site of the lake once formed by the waters of the Ardêche is now a fertile meadow; and the chasm once so ruined and bare, in which the river flows past Thuez, is now ornamented with luxuriant wood of walnut, chestnut, and beech, which give a great charm to the contrasted outlines of the jagged granites, and the alternately level and perpendicular basalts. In many places the *primitive* soil of the valley, the granite surface, cleared of the prodigious load of black rock which for ages covered it, is again brought into cultivation and yields abundant crops.

I have attempted to give (in Plate VI., fig. 5) a panoramic sketch of the position of Thuez, which may serve to illustrate this description. It is taken from the granitic heights on the opposite side of the Ardêche. In the centre of the view, in the middle distance, is the village of Thuez, resting, as has been explained, on the almost horizontal plateau of basalt, whose front or section is presented to the spectator, and which stretches for a great way to the left, or *up* the valley of the Ardêche, with gradually diminishing elevation. In the nearer part of the drawing, on the left, is seen the deep chasm *in the granite* rocks through which the river passes. Immediately behind Thuez, at the closed end of the little lateral valley which we have mentioned, rises the *Gravenne of Montpezat*, a volcano which has thrown by far the greater part, if not all, of its lava in a contrary direction, and which, therefore, has not produced at least any sensible portion of the basalts of Thuez. It is to the open and degraded crater on the right, whose vivid red colour contrasts splendidly in nature with the bright green of the trees around

and within it, that we are to look for the source of the great eruption which has at one time levelled the whole valley. A small ravine and bridge may be observed between the volcano and the village. This is called the *Gueule d'Enfer* by Faujas, and is figured by him; the Pont du Diable is concealed by the foreground of the drawing. The Gueule d'Enfer now affords passage to the stream belonging to the lateral valley. It is almost entirely formed of granite, and the contrast of the granite and basalt may be traced on the left of the bridge by which the ascending road from Aubenas reaches Thuez.

Now, in the first place, as to the *origin* of the lava there can be no doubt. The volcano of Mouleyres or Thuez has evidently produced at one prodigious throe all this mass, and may be said to have expired in the effort; for there is no appearance of any repetition of the action, and the crater is burst completely open on the side of Thuez, so as to represent, like many volcanic cones near Clermont, the figure of an arm-chair. A portion of the lava seems, however, to have escaped from the crater towards the south, and to have formed one of the basaltic plateaux in the lower part of the valley, and at a much lower level. The highest part of the crater of Mouleyres or Thuez is 2026 feet above the sea, or 500 feet above the general level of the lava-bed on which the village is placed.*

We shall call the *lateral valley* of Thuez that which is seen in the panoramic view extending towards the Gravenne of Montpezat. I have repeatedly examined the whole of this valley with the greatest care, in order to decide whether the last-named volcano had any share in producing the lava which fills it (as some authors have supposed). There is certainly an obscure appearance of a slaggy lava-stream having descended the Gravenne on the side of Thuez, but it is everywhere covered with loose cinders, whose boundary with the granite may be traced right and left. These cinders are in contact with the slag of the volcano of Mouleyres to the east, and they are lost in the bottom of the valley amongst the multitude of granite blocks by which it is entirely choked, and through which the rivulet (the same which passes through the Gueule d'Enfer) makes its way without leaving a trace of a section which should decide whether or not there is a lava-stream beneath. Even the undoubted lava of Mouleyres can be traced but a very little way above Thuez, in the bed of the stream. It is important to mention that amidst the granite blocks many are found evidently altered by heat; they are heavy, red, and friable, and have no doubt been ejected from the Gravenne. Similar blocks are likewise common at Jaujac and elsewhere.

If we would now trace more accurately the composition and dimension of the lava-stream, as shewn in the valley of the Ardêche, the best way is to descend

* The elevations in this part of the valley were deduced from barometrical observations chiefly made in 1841, and referred to Thuez as a standard height. This latter has been estimated at 1545 feet above the sea at Marseilles from six observations in 1839 and 1841, compared with those of M. Valz at Marseilles, and kindly communicated to me by that excellent observer.

by a rough footway beneath the bridge across the Gueule d'Enfer. Here we are on the exact boundary of the immense lava *plateau* and the granite to the east,—that is, the granite is under our feet and to our left in descending, the lava is above us and to the right. The remarkable section (Plate III., fig. 2) of the Gueule d'Enfer shews plainly that the lava *must* have had a support which piled it up, when fluid, to the level which it still retains, and the position of the barrier is conclusively shewn by the direction of the stratum of basaltic columns, whose axes are as usual perpendicular to its surface, and which point out with mathematical accuracy the figure of the retaining wall now removed, and replaced by the deep ravine on the right of the spectator, who looks *up* the defile as in fig. 2. We are therefore compelled not only to admit the excavation of the Gueule d'Enfer *since* the lava was consolidated, but we must suppose that a barrier of some kind stretched across the valley of the Ardêche itself, in order to retain the prodigious lava flow at the great elevation which it has attained, and which causes its bared cliffs now to overhang the valley to a height of 250 feet, reckoning from the bed of the stream. A careful examination of the panoramic view will clearly prove the surprising dilemma in which we are placed. The almost perfect horizontality of the whole remaining surface of the lava proves that it consolidated tranquilly at that level; and yet we find to the right nothing but a wide open valley, which presents no trace of a support, and from which the lava itself *has totally vanished;* for the most scrupulous examination of the bed of the Ardêche has shewn me that there is not a volcanic vestige in its neighbourhood so far down as the environs of Neyrac; and, though it is as plain as the truth of hydrostatics that the basalt must have once filled up the whole bed of the Ardêche at this place, and abutted against the granite-hill opposite (from whence the panoramic view in Plate VI. is taken), there is now not a trace of it on the southern side of the river. How astonishing, then, must have been the excavating power which has not merely disintegrated the mass of lava which has disappeared, but has destroyed the barrier, by means of which it was accumulated to the level which it retains! It is certainly conceivable that this barrier might have been partly composed of the dejections of the volcano which, when much higher than at present, may possibly have extended its cone so as partly to close the valley; yet the whole circumstances appear to shew that the forms of these volcanoes have not materially changed since the completion of their eruptions, and that certainly no vast or powerful streams of water, sweeping over the whole country after the manner of a *debâcle*, can be invoked in explanation of the last excavation of the valleys; for the loose texture of the ashes, which repose upon every volcanic cone, would have given way at once before the action of a flood, however gentle. This argument has been effectively used by Mr Scrope, to prove that the removal of the lava beds can be ascribed only to the action of water following the channels of the present rivers; and has been enforced by Sir C. Lyell

and Sir R. I. Murchison, in a paper published many years ago, on this very subject,* in which the excavation of the granite, as well as of the lava of Thuez, is cited in additional confirmation. But none of these authors, so far as I recollect, have referred to the singular manner in which this lake of lava has been, as it were, suspended in the middle of a valley which presents so great a declivity. The section just given in the Gueule d'Enfer seems to shew that that ravine must have been entirely excavated since the lava was consolidated! There are few phenomena, geologically so recent, which appear more unaccountable, more disproportioned to the means by which apparently they must have been produced. The facts before us recal, in a striking manner, the *parellel roads of Glen Roy* in Scotland; lake terraces apparently of a similar age to the basalts of the Vivarais (that is, posterior to many of the ordinary river alluvia), and which must have required barriers far exceeding in dimension those which dammed up the lava of Thuez. But in Glen Roy, whatever were the barriers, they were certainly not composed of *solid rock*. Here, on the contrary, they would appear at least in a great measure to have been so.

As we issue from the *Gueule d'Enfer*, we find a tolerably wide and cultivated ravine, entirely based on the primitive rock; whilst the mural precipice of lava, in some places 200 feet high, extends for about a mile parallel to the river on the right. The contact of the lava with the ancient soil may almost everywhere be traced. The lower part is usually composed of vertical columns; the upper part is (as at Jaujac) only very imperfectly prismatic; but the whole is evidently the result of one eruption. The lower part of the stream, where it touches the soil, has in some places a very singular appearance, glistening and coaly; probably composed of nearly pure augite mixed with carbonaceous matter, of which I found a singular proof in a portion of a vegetable stem, of which a *cast* has been made by the lava.†

The general phenomenon of the perpendicularity of the lava prisms to the surface of cooling is everywhere exemplified; but nowhere so beautifully as near the part of the cliff called the *échelle du Roi* (from a narrow steep passage formed by a dyke or vein in the lava, and by which the cliff may be ascended). Here there occurs beneath the prismatic lava a shapeless mass, apparently of old lava, slaggy, and not at all columnar; of which the new lava has formed an exact cast, and fringed it all round with columns perpendicular to its own most irregular surface. This is represented in Plate III. fig. 3, from a rather careful drawing made on the spot. I have often been inclined to think that the old lava, which must evidently have been cold when it was overflowed by the other, may be derived from the Gravenne of Montpezat; and this idea is confirmed by the consideration, that

* Jameson's Edinburgh Philosophical Journal, 1829.

† The specimen illustrating this curious fact, and others referred to in this paper, are now placed in the Museum of the Royal Society of Edinburgh.

this point was undoubtedly the ancient issue of the stream of the lateral valley which now escapes at the Gueule d'Enfer. I have ascertained by barometrical observations that this is really the lowest point of contact of the lava with the ancient surface of the granite valley; and was therefore the *thalweg* or water-way until it was choked by the lava of Mouleyres. If, then, the Gravenne gave birth to any stream of lava, however small, it might be expected to have flowed in this direction. If this change of the water-way be admitted (and it appears to be unquestionable), there is another proof, amounting almost to demonstration, that the Gueule d'Enfer has been excavated in the granite or gneiss since the lava of Mouleyres flowed; for before that time no water could have run through it.

The following are the results of my barometrical observations:—

	Eng. Ft. above Sea.
Summit of Lava Cliff at Echelle du Roi, ...	1476
Foot of	1283
Summit of Lava Cliff at Gueule d'Enfer, . . .	1514
Foot of	1343
Height of Cliff at Echelle du Roi (A), ...	193
... ... Gueule d'Enfer (B), ...	171
Surface of Lava at A *below* Surface at B, . . .	38
Contact of Lava with Ground at A *below* contact at B,	60

The lava of Thuez gradually thins out as we ascend the valley; continuing, however, to present a basaltic cliff towards the stream. Where the lava ceases, the valley expands; and the river has a wide bed formed of detritus, preserving a uniform level, evidently occasioned by a lake which once existed, due to the obstruction caused by the lava to the waters of the Ardêche.*

Montpezat.

A short walk from Thuez leads to the top of the volcano called *La Gravenne de Montpezat;* but the ascent is rapid, the strata of pozzuolana constituting the southern face of the Gravenne dip at an angle of about 30°, the superficial debris 25°. The principal discharge of lava being towards the north, filling the valley of Montpezat, we shall next in order consider the phenomena which that district, watered by the Fontaulier, presents. The Gravenne itself rises to a height of 2727 feet. The view from it is extensive and striking. It has an exceedingly well-formed crater, which, however, is seen from but few points. Plate III., fig. 4, shews its appearance, as seen from a hill to the north, on a ridge between Mont-

* About 5 hours' walk across the hills to the south-west of Mayres, or 7 hours' from Thuez (passing le Chambon and Bornes), are the remote mineral-springs of St Laurent les Bains, having a temperature of 125° Fahr. They rise from mica slate, in the neighbourhood of granite, and traversed by granite veins. They contain salts of soda almost exclusively, and particularly the carbonate.

pezat and Burzet. It is a real *cup*—the bottom being lower than the lowest edge of the crater by which the lava has poured out into the valley. A high and solid conoïdal mass of lava and ashes commands the crater to the SW. so as to give the character of an unbroken top to the hill in that direction. The highest parts are composed of partly solid lava, and rise 100 feet above the bottom of the crater.* The principal lava-flow descends in the direction of Montpezat, at an angle (by estimation) of little less than 35°; yet it is tolerably continuous. At its base, near the bridge across the Fontaulier (which is 1534 feet above the sea, and 1200 below the summit of the Gravenne), we have a remarkable section, Plate III., fig. 5, which is made *transversely* to the direction of the lava stream of the Gravenne, which forms the upper layer. The layer of basalt forming the base of the section, and which is separated from the other by a layer of granite boulders, cemented like that of Neyrac by the action of the acidulous water, is evidently the product of an altogether anterior eruption. The more modern eruption has bathed the whole valley with lava, which has formed a tolerably level prismatic bed, stretching some way up the tributary stream of the Pourseuille, as well as up the valley of the Fontaulier, but not far in either case, and stopping considerably short of the town or village of Montpezat, which is built upon a mass of granite boulders. The junction of the Pourseuille and Fontaulier affords an excellent section below the picturesque site of the Castle of Pourchirol, Plate III., fig. 6. Here but one bed of basalt appears (I mean due to one eruption).

The plateau formed in the valley of the Fontaulier by the lava of the Gravenne is justly described by Mr Scrope as having "its upper surface bristled with rocky and scoriform projections; which, however, by decomposition, resolve themselves into a rich soil, affording nourishment to very productive chestnut forests." Many scenes of great beauty occur on this plateau. There is some appearance of a lake having been formed at the contraction of the valley at the Pont de la Motte, several miles farther down; and both in this valley and that of Burzet (which we shall afterwards describe) there occur deposits of pozzuolana apparently stratified by water. At the junction of the Fontaulier with the tributary valley of Burzet, about half-way between the Gravenne and the Pont de la Beaume, the lava of Montpezat joins that of Burzet, or rather, overlies it,—the plan and section, Plate IV., fig. 1, marking plainly the more recent date of the eruption of the Gravenne. These streams remain superposed for some distance; but at the Pont de la Motte (at Mayras) there is only a single stream, which is columnar, abounds in olivine, and reposes on a conglomerate. I am unable to state positively to which of the two valleys this may be traced (although the determination would be easy); but from its composition and other circumstances, I am inclined to think that it derived from the valley of Burzet. It continues all the

* Messrs Lyell & Murchison (Edin. Phil. Journal, 1829, p. 27) speak of angular blocks of unaltered gneiss as occurring near the summit of the Gravenne. These appear to have escaped my notice.

way to the *Pont de la Beaume*, and is no doubt the same which we have already described as underlying the great *coulée* of the valley of the Ardêche.

Having now traced the course of the lava of the Gravenne, we shall point out some other volcanic appearances in the valley of Montpezat. And, first, we have a dyke of basalt, in the granite of the left bank of the Fontaulier, near the hamlet of Les Plantas, about half-way between the Castle of Pourchirol and the entrance of the valley of Burzet. This is a phenomenon which we should naturally expect to meet with very frequently in a country which, with a soil entirely primitive, has been pierced at so many points within a short space by volcanic ducts, which can hardly have been formed without an intense pressure from below, which might have been expected to rend the strata of gneiss in all directions, and then to have filled the rents with melted matter, constituting true dykes, such as those which occur in Monte Somma, near Naples. The reverse is, however, the case: and the dyke of *Les Plantas* is the *only* one which I have met with in *this part* of the Vivarais (amongst the older volcanic formations of Le Puy and the Coyrons they are numerous). No doubt, many may have escaped me; and I should never have known of this one, owing to its remote and concealed position, had I not been fortunately directed to it by a resident at Montpezat. It occurs in a small ravine (see the map), running in a direction nearly north and south, and is said to have been traced for a mile. Its plane is vertical. Its breadth varies from 1 to 4 feet; and it sends forth small veins into the rock, and includes portions of granite in its substance. In composition, it resembles the lava of the district; black in colour, with concretions of olivine.

The village of Montpezat, charmingly situated on the rivulet Pourseuille, at the height of 1857 feet above the sea, does not stand upon the lava of the Gravenne, but about half-a-mile beyond where it terminates at the parish church of St Pierre. The valley is filled by a prodigious multitude of rolled blocks of granite; which here, as in the lateral valley of Thuez, appear to be out of all proportion to the extent of the ravine from which they must have been derived, and of which the countless multitude contrasts with the comparative rarity of basaltic blocks. At the head of the valley, near the source of the Pourseuille, rises a mass of volcanic cinders, well distinguished at a distance by its colour; and which is figured by Mr Scrope, in his view of the valley of Montpezat (Plate XV. of his work), who describes it as an anonymous volcanic cone which has not produced any lava stream. He had evidently not visited the place; for the true description is very different. I shall now detail the chief results of a patient examination of this locality, both in 1839 and 1841; there is no point in the Vivarais which I have more narrowly investigated.

Montpezat, we have said, is placed upon an immense bed of granite boulders, which extend upwards, occupying the whole bed of the valley, to a great thickness; but near the village of Le Fau we find a mass of lava, which, with other

volcanic products, fills the bed of the stream for a very considerable distance, rising with a very steep acclivity, which shews that its viscidity must have been considerable to allow it to harden at so great an inclination. M. Serrouil, watchmaker at Montpezat, showed me a sapphire and zircon which he had picked up near this place, which is interesting from the occurrence of these minerals in the lavas of Croustet, near Le Puy. The lava of Le Fau is surmounted by beds of volcanic conglomerate, of which the river has made a section in passing between it and the granite on the right bank of the valley; the thickness of the conglomerate is here at least 100 feet, and the inclination may amount to 25°. The volcanic stream presents a very massive and striking appearance, and leads to the belief that we are in the immediate neighbourhood of a volcanic focus; and such might be supposed to exist in the steep slopes covered with slag and volcanic conglomerate (the conglomerate still uppermost), which rise to a great height on the left bank of the stream, not far from its source. Nothing of the kind, however, appears; the beds become thinner, and are in absolute contact at no great depth with the granite which constitutes the neighbouring slopes, or with granitic boulders covering the rock at D, Plate IV., fig. 2. The lava in contact with the granite has evidently consolidated in its present position, from the nicety of its adaptation to the rock, and from the *shortness* and fragility of its structure. The position of this volcanic *curtain* will be best understood from the plan in the figure just referred to, which shews the uppermost part of the valley of Montpezat, as traversed by the very steep paved road leading by the hamlet of Le Pal from Usclades and Le Puy. This road has since been superseded by a new one, which was nearly completed at the time of my last visit. It will be seen that the highly-inclined beds D, are those now described, and are divided by a streamlet which issues from a kind little circular valley, in which a cottage is marked.* This circus might well pass, upon a superficial examination, for the source of the lava or a true crater. It has, however, no such pretension, being completely excavated in granite; and the aforesaid curtain of lava and breccia forming a thin exterior facing on the side exposed to the high road. The height which the volcanic formations attain on this slope is no less than 4134 feet above the sea, and nearly 2300 feet above Montpezat, though so little distant. It will be understood, therefore, that the slope is extremely rapid.

The place where these remains occur is called *Le Chambon*: we shall therefore denominate the deposit by the name of the lava of Chambon. It is hoped that it has been made evident that no eruption took place in this spot. The excavation of the ravine behind D, and the numerous contacts with the granitic soil, shew that it is entirely foreign to the place. Whence then is it derived? There can be but one answer, even although that conclusion be attended with some diffi-

* There is also another cottage more to the left, in a place where, on Cassini's Map, is marked "Lac de Ferrand;" that little lake in reality lying higher up in the direction of the volcano of Bauzon.

culties. It must be a stream from the *Crater of Pal*, already adverted to in describing the route by the Bauzon from Le Puy to Montpezat. This well-formed, nearly circular crater occurs a little behind the granite rocks and cliffs which compose the back-ground of the ravine of Chambon; and so far it would appear to be a very natural origin of the lava. But the singular circumstance is, that the crater in question belongs to a different system of valleys; inasmuch as the water issuing from it forms the source of the Fontaulier, and reaches the vicinity of Montpezat after a long circuit (see the General Map, Plate I.). It is also pretty open to the north; but it seems completely cut off from the ravine of Chambon by a considerable granite ridge running NW. and SE., forming the *Col* or passage o. Le Pal, and rising to a height of 4537 feet, exactly between the crater of Pal and lava of Chambon, being 644 feet higher than the former, and 403 feet above the extreme point of the latter. There are therefore two difficulties to be accounted for; *first*, the passage of a great mass of lava and other volcanic matters over this barrier; *secondly*, the excavation of the ravine of Chambon, and the removal of all the upper part of the lava stream.

With regard to the *first* difficulty, it is important to remark that most of the granite heights between Le Pal and Chambon are covered more or less thickly with a volcanic conglomerate forming horizontal beds (one granite top which has escaped, is uncoloured on the map). This conglomerate descends to the crater of Pal, and stops short abruptly on the face nearest Le Chambon (see section, Plate IV., fig. 2). Consequently, the hills about C (fig. 2, *Plan*) did not probably always form the highest part of the edge of the crater; for the conglomerate is of too compact a character to have been formed by a dejection of volcanic materials merely; and the same cause which permitted the deposition of these conglomerates (such as the heightening of the crater by the formation of a cone which has since disappeared), would permit equally the passage of a part of the fused materials across the ridge into the neighbouring valley. And the fact of the occurrence of abundant conglomerates in both positions, confirms, if it does not render absolutely necessary, this supposition; although I am aware that there is some mineralogical difference in their composition,—that of Chambon being more friable, and more generally composed of volcanic ingredients, containing, however, granitic masses,—that of Pal being *chiefly* of granite boulders, cemented by a very hard volcanic basis. The posteriority of the conglomerate eruption may account (together with the great declivity) for the absence of any trace of the *lava* flow on the granite heights of Le Pal.

The *second* difficulty, that of the excavation of the ravine of Chambon since the lava flowed, must remain, I fear, unanswered, upon any theory. Of the fact there appears to be no reasonable doubt. This (geologically) recent excavation of a perfect mountain of hard granite, at the head of a ravine which possesses no drainage sufficient to procure a powerful current of water, and which is near the culminating point of three pretty extensive mountain-ridges, is merely one of a

thousand cases of excavation which most hilly countries exhibit, but of which no geological hypothesis has yet given any satisfactory account. In this case, the difficulty of the explanation is increased by the circumstance, that the excavation has occurred since the deposition of a very recent lava, a lava which has itself covered the boulders due to a previous denudation.

I need hardly say one word of M. Burat's assertion, that the lava of Chambon is an "*épanchement lateral*" from the granite wall of the crater of Pal; in other words, that it has issued by a subterranean orifice from the same internal focus. Such an explanation would be highly satisfactory were it founded on fact. But we have seen that no such orifice exists. Did it exist, it must be at a higher level than the highest point of the volcanic accumulations D (fig. 2); but the lava has there only a few feet of thickness, and beyond, above, and around it, the bare granite is everywhere exposed, the circus extending from E to D, being throughout entire, generally precipitous, and *presenting no fissure*. To this may be added the fact, that the highest part of D is higher than the present level of the interior surface of the crater of Pal, as the following barometrical heights attest.

			Barom. mill.	Ht. above Sea.
1839, June 3.		—Lowest part of Crater of Pal, . . .	661·6	3893 feet.
...	...	Highest Boundary of the Crater to NE. (conglomerate), ...	646·9	4537 ...
...	...	Highest point Lava of Chambon, . .	656·1	4134 ...

The whole relations of this singular formation will, I hope, be made plainer by the elevation, or distant perspective view in Plate IV., fig. 3, carefully drawn from the top of the Gravenne of Montpezat. The same letters of reference are used as far as possible in figs. 2 and 3.

The crater of Le Pal has acquired some celebrity by the discussions to which it has given rise in the Geological Society of France;* having been by some regarded as the type of a *crater of elevation*, as composed entirely of granitic strata elevated so as to present a volcano composed of primitive rock. I cannot at all partake of this view; and indeed can affirm, from the most careful and repeated examination, that it presents no peculiarity which can cause it to be considered as exceptional, unless the insignificant quantity of scoriæ ejected by it (or, at least, which are now visible), which is insufficient to form a true cone like that of the neighbouring volcano of Bauzon. The primitive rocks appeared to me here as elsewhere to be undisturbed;—to have no exterior configuration whatever, indicating that they have been moulded by the pressure of lava in the interior; and certainly not to possess the character of "internal precipices (forming the crater) with gentle external slopes:" the section in fig. 2 shews just the reverse. In fact, the granitic eminences by which the crater of Pal is partly surrounded, are (as already mentioned) the culmina-

* Bulletin de la Société Géologique de France. Tom. III. and IV.

tion of several ridges which have no approximation to a circular arrangement; and they do *not* form the entire circumference of the crater,* but only about one-half of it at its internal base, without allowing for the inequalities which even in that part are often filled up with volcanic slag, and the enormously thick coating of conglomerate above the granite on the eastern slope of the crater. Were we simply to project the points where the granite appears, it would make but a sorry circus. In fact, it may be regarded as a granitic valley choked with scoriæ, and plastered with Roman cement till it forms a nearly circular cavity. The occurrence of a crater in such a position only affords, in my opinion, a fresh proof of the singular perforation of non-volcanic strata (including granite or gneiss in that term), by subterranean explosions which have been so incredibly sudden and violent as to have occurred without any visible general disturbance (like a pistol-shot penetrating a board, to use a comparison which I have somewhere read), which recall the extraordinary crater lakes of the Eyffel, penetrating slaty rocks without deranging their strata, and seldom giving birth to any considerable volcanic stream.

Fig. 4 of Plate IV. is taken from a rude eye-sketch of the crater of Pal. The chasm at the western entrance gives exit to the river Fontaulier, which takes its rise here. The temperature of the copious spring is 42°·2, the height above the sea being 3900 feet.† The existence of a spring of the natural temperature (for such it appears to be)‡ in such a position leads to an interesting reflection. Its great bulk, and probably uniform temperature, shew that it rises from a considerable depth; and it cannot be doubted that it must follow the course, or nearly so, by which the lavas amidst which it rises have made their way from the interior of the earth. The source of the spring is therefore in the direction of the volcanic focus. It is evident, then, that the mass of rock at that depth, which was once incandescent, has had time to cool to (sensibly) the temperature of the air. How old, then, must be the date of this geological recent eruption? It would not be difficult to submit it to calculation; but the uncertainty of the date would render numerical results of little value. Every one comprehends that a mountain of granite, with a nucleus of incandescent lava, could not have been *completely* cooled unless in a period of time of very great length.

* I am sorry to say, that the view in M. Burat's interesting work on Central France (Plate VII.), is altogether exaggerated and inexact.

† 1839, June 3. Spring 42°·0 (Therm. marked A. 1.). 1841, June 25. Spring 42°·2 (Therm. marked A. 3.). Now the correction of A. 3 is +0°·2. That of A. 1 was 0°·0 in 1838. By a singular coincidence the barometer on these two occasions marked *the same tenth of a millimetre*. The temperature of the air was also within 1° Fahr. of being the same both days.

‡ The mean temperature of Viviers on the Rhone, which is in the same Department, and only 57 mètres above the sea, is 55°·25 according to Cotte, as given in Dove's Tables. The crater of Pal is at 1186 mètres above the sea, or 1129 above Viviers. Now, in France generally (according to Martins), the decrease of temperature with height is 1° cent. for 180 mètres, or 1° Fahr. for 100 mètres exactly; or 11°·29 for 1129 mètres, whence the mean temperature of Pal should be 55°·25 − 11°·29 = 43°·96. The temperature of the spring was somewhat lower in the month of June, but the approximation is a fair one.

There are two portions of slaggy lava near the artificial dam, at the exit of the stream from the crater. But I could in no way identify them with the *dykes* so prominently alluded to by BURAT; but these may possibly have been concealed by the artificial erection. Indeed, I saw nowhere any appearance of injected lavas into fissures in the granite. The crater is stated by BURAT to be 1200 metres (two-thirds of an English mile) in its longer diameter. It is sensibly oval and tolerably flat, presenting three mounds of scoriæ in the centre, similar to those which occur in the craters of recent volcanoes.

Only a few hundred yards in a north-west direction from the crater of Pal is the little *Lac de Ferrand;* erroneously placed in CASSINI's map at the head of the valley of Montpezat. Adjacent to it is a small lava stream (indicated in the map), which might lead us to conclude that this lake had been a small volcanic orifice; this is, however, uncertain. Farther on in the same direction lies the volcano of Bauzon, which has ejected a vast quantity of scoriæ, and is the last of the recent volcanoes of the Vivarais on this side. Otherwise it is without interest. Its height above the sea, by my observations in 1841, is 4922 feet.

Valley of Burzet—Crater of Fiollonge—Valleys of Antraigues and La Bastide—Pic de l'Etoile—Coupe d'Aysac.

We now return to the valley of Burzet, which is a tributary of the main valley of Montpezat, but which extends itself to a great distance in one of the least frequented parts of the country. We have seen (p. 23) that its bed is filled with lava, which was certainly older than that of the Gravenne (see Plate IV., fig. 1). This lava may be traced, with little intermission, as far as the village of Burzet, between 4 and 5 miles from its outlet. The valley is beautifully varied in scenery, and in some places richly wooded. At Burzet it widens, receiving a tributary on the right bank of the principal stream. This tributary takes its origin near the crater of Pal; but its course presents nothing very interesting. The lava widens at the junction of the two streams, and the village is partly built upon it. It would be difficult to select a more exquisite picture of rich and peaceful scenery than the neighbourhood of Burzet. The stream is small, and its bed generally narrow; yet though the lava masses become more insulated and smaller as we ascend, nothing indicates that we have arrived at their commencement. We fancy them to have ceased entirely, when in some re-entering angle of the valley their black abrupt faces reappear, clinging to the granitic soil, moulded upon it, and the columns always perpendicular to the surface of cooling, as in Plate IV., fig. 5. The slaggy traces of the current often rose so high upon the banks, that I repeatedly thought that I had obtained the volcanic orifice. This, and the *concavity* of the surface of the basalt in many places, as in Plate II., fig. 7,

shews the extreme liquidity of the stream, which must have shot through this narrow and tortuous valley with a rapidity truly astonishing, leaving its scum and slag to mark the height which it attained; but the main flood of lava not having time to solidify, was propelled ever onwards, leaving often but a narrow thread in the channel of the stream, by which to trace its passage.

One circumstance struck me very forcibly as regards the mineral character of this lava. It contains most abundant nodules of olivine, which have not in the least the appearance of being formed *in* the lava by a slow crystallizing process; but, on the contrary, the irregularity of their forms is *decidedly fragmentary*, often as if rolled; and from the manifest impurities which they present, I am of opinion that they are nothing but fragments of granite in a peculiar metamorphic condition. This idea first struck me several years before I visited the Vivarais, in an examination of the lavas of Clermont; where I found olivine masses near La Barraque, having at first sight the aspect of granite fragments, but when examined, they appear to be only olivine, with some adhering reddish micaceous and clayey matter.* The olivine of Burzet presents two varieties of a pale yellow green, and of a peculiar orange colour. They have no concretionary structure depending on their form. Similar phenomena are common in different parts of the Vivarais, but less, I think, in lavas which are decidedly columnar. The frequency of apparently ejected masses of almost pure olivine in many volcanic countries, and most especially in Upper Eyffel (as at the crater of Dreiser-Weyer) gives an additional probability to the hypothesis that they are foreign substances expelled in an altered condition. Long after making these remarks, I noticed a passage in Mr Scrope's work on Central France, tending to the same conclusion; namely, that these olivine nodules are altered masses of pinite from the granite.†

In 1839 I pursued the traces of the lava of Burzet about 4 miles beyond the village of that name, or at least 8 from the opening of the valley, without seeing a trace of a crater; and my time did not then allow me to prosecute the search. But it was one of the objects of my second visit in 1841 to resume it; and, accordingly, accompanied by my friend Mr John Mackintosh, I slept at Burzet in very uncomfortable quarters, in order to have the whole day for our excursion, resolving to ascend as high as the great ridge which separates the waters of the Ardêche and of the Erieux. We then readily found the point which I had before reached, where the lava temporarily disappears from the valley, though there is no appearance of a crater; and this disappearance con-

* M. de Buch in his early writings (Journal des Mines, XIII., p. 251) notices the olivine of La Barraque, without saying anything as to its origin, except that he considers it an exclusive character of old basalt, as contrasted with lavas, at least in that country.

† Scrope, p. 150. Faujas St Fond speaks of the lava at Beaume containing "petits éclats de granit *en chrysolite*," which seems to indicate a similar opinion. (Recherches, p. 300.) He also describes masses of olivine existing in the basalt of the valley of Burzet, in the meadows below the village of St Pierre Colombier, weighing as much as 30 pounds. This remarkable statement deserves verification. See *Recherches sur les Volcans eteints*, pp. 249 and 312.

tinued longer than I expected, as we scrambled with some difficulty along the bank of the river. I carefully marked here, as elsewhere, every patch of lava upon CASSINI'S map. The disappearance of the lava in this place is no doubt due to the steepness and narrowness of the channel, which left few points of lodgment for the lava, and which gave an increased force to the eroding power of water.

We passed successively the hamlets of Lespereyres and Chabron; and after the last we kept in the hollow of the valley, and soon got a sight of a great lava cliff, betokening our approach to the crater. Clambering high above the left bank of the stream (which we crossed above Chabron) to avoid precipices, we at length, after a fatiguing walk of three hours from Burzet, obtained a view of a very remarkable cascade, which descends from near the crater which formed the object of our search, which, from a cottage occupying its centre, I called the Crater of Fiollonge. The cascade is called Raïpis; and the water falls by several steps over a mass of beautifully columnar lava several hundred feet high. These interesting objects more than rewarded our perseverance. They are described, so far as I am aware, by no previous writer, and were probably seen by a geologist then for the first time. Plate VI., fig. 4, gives a view of the cascade.

The crater presents, as usual, an insulated cone of scoriæ, touching the granite in its whole circumference, except on the side next the valley, where a flood of lava has made its escape, damming the lateral stream, and forming the great cascade. In the slag adjoining the cascade are immense masses of imbedded granite; which give additional countenance to the notion of the metamorphic origin of the olivine nodules which so remarkably characterize this lava in all its extent. The columnar formation at the cascade, though far from the most extensive, is probably the most curious and complex in its forms of aggregation of any in the Vivarais.

If we now consider the operations of this volcano, we shall find the extreme length of its lava-produce to be really surprising. Although, by the map, the distance of the crater from Burzet may not appear to be more than 4 or 5 miles, the contortions of the bed of the stream are such, that it is probably double that distance; and, indeed, can hardly be less than 8 miles, since we were 3 hours on the way (steep and rough as it was). To the junction with the lava of the Gravenne, in the valley of Montpezat, is therefore more than 12 miles; and if it be the same lava (as Mr SCROPE supposes, and as there is some reason to think) which continues to the Pont de la Beaume, the distance will be 16 miles. Yet the whole valley of Burzet is a ravine so narrow and crooked, that the lava appears like a thread winding through it; and we are amazed that it should not have become solidified by the contact with the cold granite, before it had performed one-fourth of the distance. I apprehend that there is no lava known, ancient or modern, which has formed so attenuated a stream. This is only to be accounted for by its excessive fluidity, of which we have additional evidence, (1.)

in the *washing* of the sides of the valley by the scoriæ to a considerable height on either hand; (2.) from the nicety with which it has filled every angle of this irregular channel; (3.) from its almost total disappearance for several miles from its course, shewing (I apprehend) that it ran over the ground almost without consolidating.

	Feet.
The Height of the Pont de la Beaume above the Sea is . .	1073
... ... Village of Burzet*	1815
Rise of the Valley in 8 miles, ...	742

Mean Slope (1 in 57, or) 1° 0′

At the crater of Fiollonge the barometer stood, on the 26th June 1841, at 670·7 millimetres. Its height is 3678 English feet. If we deduct from this 300 feet, as a rough estimate of the height of the crater above the bed of the river of Burzet, at the foot of the cascade of Raïpis, we have for the rise in the upper 8 miles of the valley from the village of Burzet, 1563 feet.

Mean Slope (1 in 27, or) 2° 7′

Even on my first visit in 1839, I was so much struck by the fluidity which this lava stream must have possessed, that I made some experiments during the following winter, with the kind permission and aid of the late Mr Edington of Glasgow, upon the flow of cast-iron in very small and tortuous channels at different slopes. The results are scarcely worthy of minute detail; but they left the impression on my mind that the temperature and the liquidity of the lava of Burzet must have at least equalled that of pure cast-iron, a result which strikingly contrasts with the extreme viscidity of most modern lavas, which only attain a long course, aided by very steep inclinations, or by their great volume, which generates a vast moving power as well as retains the high temperature. I found that in tortuous channels of half-an-inch broad, and under slopes of about 1 in 120, the hottest iron used in ordinary casting solidified before it had run a course of many feet. The form of the current, too, was the same as in the lava; the surface was concave, except near the lower termination of the stream, where it became convex, owing to the pressure from behind, and the accumulation due to the increasing friction occasioned by the crystallization of the iron which appeared to be rather suddenly effected. Near the source a mere *trail* of slag was left (often hollow), and the slag bathed the sides of the channel to some height, as described in the case of the lava.

In making these experiments, I little thought that in a few years I should have occasion to return to the subject of the motion of viscous fluids in narrow channels, in connection with the seemingly opposite subject of glaciers; but, in fact, the forms of my cast-iron models (which I still possess), recall strikingly

* Barom. 715·6 mm. 26th June 1841.

those of glaciers, both in their higher concave parts, and in the abruptness of their lower terminations.

Figs. 3 and 4 of Plate V. shew the position of the volcanic formation of Fiollonge, relatively to the neighbouring valleys, which are not correctly laid down in Cassini's map.* The way to the village of La † Champ Raphael is by the saw-mill of Bacconnier. The pastures here are filled with charming alpine flowers, and surrounded with extensive woods (*Bois de Cuze*); but La Champ is a miserable exposed village, situated near the heights which overlook St Andeol and the valley of the Erieux, with the chain of the Mezenc in the distance. The elevation above the sea is 4409 feet. In the neighbourhood are old basalts. A few miles to the eastward we reached, by a bleak exposed tract, the village of Mesilhac, where we passed the night in very wretched quarters. The view from hence to the north is similar to that from La Champ Raphael; and I thought the mountain forms sufficiently remarkable to deserve a record, which I have given in Plate V., fig. 5.

Mesilhac is a dirty hamlet of but a few houses, on the *col* connecting the vallies of the Ardêche and Erieux, and is at a height of 3791 feet above the sea. A pleasant walk of only three hours took us next day through a comparatively open and accessible valley to the beautifully situated village of Antraigues, where we found ourselves amidst a wholly new series of modern volcanic formations. Opposite La Viole, a hamlet about five miles above Antraigues, there is a volcanic summit on the right hand of the River Volane; but as its lava is chiefly thrown in a different direction, we shall not at present describe it.

Antraigues (*inter aquas*) is seated, as it name imports, at the junction of several mountain streams in a romantic hollow. It is built on a tongue of granite, formed by the junction of the Volane and Rivière du Mas, and is almost surrounded by patches of a flood of lava, which must once have levelled the whole of this mountain basin. The source of this lava is the beautiful volcano of Aysac. One of the most picturesque remnants of lava bed is shewn in Plate VI., fig. 1.

La Coupe d'Aysac, one of the best-known craters of the Vivarais, is situated almost upon a ridge of granite near the head of a small lateral valley connected with that of the Volane at Antraigues, and pours its lava first into the small valley, and through it into that of the Volane, which has made a remarkable section of it exactly opposite to the village, as accurately figured by Mr Scrope.‡ The lower part, which is basaltic, has, as usual, given way, and left a cavern beneath the little cascade which descends from the lateral valley. The cone of

* In the View, fig. 4, the hill behind the cottage of La Fiollonge is of granite, but covered with cinders nearly to its summit. The rocks in front on the left are of granite, but those between the spectator and the cottage in the centre, are of lava, as well as in the lower part of the ravine on the right.

† So spelt in the Maps.

‡ Plate XVI. The volume of the River Volane is, however, by mistake, altogether exaggerated.

Aysac is very perfect, with a nearly circular crater, which rises to a height of 2640 feet above the sea, and 1257 above the bridge across the Volane at Antraigues. The crater is extremely well formed, and I estimated its circumference at about a mile. It is broken down at one point only, on the NW. side, by which the lava stream has run down the steep face of the hill opposite to the hamlet of Aysac, where it enters the bed of the torrent, forming a prismatic mass exactly at the foot of the cone. It is from this point that the view of La Coupe, in Faujas's work, is taken, and the general features are exact enough. It is therefore without reason that Mr Scrope has charged him with a gross and absurd blunder in representing the crater in a direction whence it could not possibly be seen. Mr Scrope supposes the basalts in the foreground to be those which he has himself figured in the ravine opposite Antraigues; whereas the view of Faujas is entirely confined to the little lateral valley leading to the Collet d'Aysac, as clearly appears from his ample, and generally accurate description.*

The lava of Aysac, after filling up in great part the basin of Antraigues, has followed the course of the Volane for several miles, as the numerous basaltic patches between that village and Vals sufficiently attest. These basalts are perhaps equalled by none in the Vivarais, or in any other part of Europe, as regards the exquisite perfection of the pillars of which they are composed; which, though they attain no great height, as at Jaujac and Thuez, are symmetrically polygonal, small in diameter, and beautifully jointed, affording beautiful cabinet specimens, particularly at the bridge across the Volane, two miles above Vals. This lava, like that of Burzet, appears to have been very fluid, and to have accumulated in the gorges to a greater height than it could afterwards retain. We find it, accordingly, applied in thin sheets upon the steep granite walls of the valley, forming, as usual, pillars perpendicular to the cooling surface, as in fig. 2 of Plate V.

Before finally quitting Antraigues, I must mention that the summit of the hill, a little to the ESE. of the village, is occupied by a formation of basalt (which is indicated in the map as a lava), but which appears rather to belong to the ancient basalts of the Coyrans and Mesilhac. It is composed chiefly of loose masses, excessively hard and heavy, containing olivine, chalcedony, and other substances, and decomposing into an earthy matter. It gives no indication of having flowed into the valley, and was probably formed before the valleys existed.

The village of Vals is pleasantly situated on the river Volane, below the last traces of the lava of Aysac, and not far from the junction of the gneiss with the secondary rocks. Two mineral springs issue from the gneiss at the river's side, having temperatures of 59·5 and 61°. They are charged with carbonic acid, and contain a little iron and soda. They are frequented in summer by visitors even from a considerable distance, and, in consequence, the inn or hôtel at Vals is the only tolerable one in the whole district, and, as such, will be duly estimated by

* Recherches, p. 296–298.

every traveller who has spent some time in the Vivarais. It is also a good centre for making excursions.

One valley remains to be described, which I shall call the valley of La Bastide, from its principal village. It is intermediate between the valleys of Burzet and Antraigues, and may easily be reached from either of them; but by ascending from Vals (which is but a little way below where it unites with the valley of the Volant) it may be conveniently examined in its whole length. The first portion for several miles presents no trace of lava; but when we get opposite to the position of the Coupe d'Aysac, the valley opens and becomes extremely beautiful. A spring from the granite rising here, which discharges carbonic acid, has a temperature of 53°·2. A mile or two above this, we came at once upon an extensive lava deposit, excavated by the torrent, and commanding a pleasing view of the hamlet of La Bastide, with its water-mills, and the ruins of its chateau, which belonged to the Comte d'Antraigues, an extensive proprietor in this country, including nearly all the Bois de Cuze, who was expelled by the populace at the first French revolution, his house destroyed, and his lands divided.

The lava continues with but very little interruption for several miles, from La Bastide up the valley, indicating clearly a peculiar source of its own. I had previously visited it in 1839, and (more fortunate than in the case of the Burzet lava) I had fairly hunted it to cover, tracing it in an irregular stream up the face of the ridge of lofty hills separating the valleys of La Bastide and Antraigues, about three miles above the former village (Plate VI., fig. 2). The summit from which it is derived, I then learnt, was called the Pic de l'Etoile. My time did not allow me to ascend farther; but on a subsequent day I made an expedition expressly by the valley of Antraigues, and ascended the *right* bank of the Volane for four or five miles above that village, when I judged that I must be nearly opposite the volcano (of which, however, there is no trace visible from below on the east side). I commenced the ascent from the hamlet of La Viole (see the map), which proved to be excessively steep, and for a height of nearly 2000 feet, the latter part of the way being through tangled and nearly pathless brushwood, frequented only by a few charcoal burners. At less than one-third of the ascent I found, in a little ravine, a flow of lava, which proved that I had not mistaken my point, and from under it issued a fine spring, having a temperature of 45°, affording another proof (see p. 28) of the complete refrigeration of the lavas. This stream does not reach the bed of the Volane. As I followed it up, it constantly occupied the bed of the water-course, and soon led me amongst slag and cinders, which constitute the steep part of the ascent. At the top I found the summit of the chain occupied for some distance by volcanic products. The granite, however, resumes the higher position. On examining from the top the course of the stream of lava descending towards La Bastide, I perceived that it originated still farther beyond the granite; and, proceeding northwards, I welcomed the sight of a crater, of which hitherto I had perceived no trace. It is rather singularly

situated, occupying the whole breadth of the ridge, in a sort of depression or *col*, at a point where the ridge makes, at the same time, a bend, as sketched in Plate V., fig. 1 (which was not drawn on the spot, but immediately after from memory).

Whatever may be thought of the crater of Pal near Montpezat, *this* one is undoubtedly blown out of granite, and entirely formed in it; for I do not think that the scoriæ constitute a considerable mass upon any side, and there is no outlet or imperfection upon any side except that to the westward, by which the lava has run into the valley of La Bastide. The crater is of a beautiful elliptical form, and lies exactly between two ravines on either side of the ridge; the imperfect lava stream towards the last no doubt flowed first. The elevation above the sea is 4204 feet, or somewhat higher than the crater of Pal. It commands a fine view, and here I first caught a glimpse of the probable origin of the lava of Burzet, which two years later I was enabled to confirm. This volcano, like that of La Fiollonge, appears to have been undiscovered by any previous geologist, and as an example of a crater in granite it is undoubtedly remarkable. I was forced to abridge my observations on account of a very violent thunderstorm, which covered every thing with mist, and compelled my retreat. The lava on the side of La Bastide accumulated to a considerable thickness, but it is not remarkably columnar. In some places it approaches almost to the character of obsidian. The character of the valley is cultivated and pleasing. Groups of well-built houses are studded over the slopes, amidst groves of chestnut, with neat and well-watered gardens adjoining. In all this country, as well as in the *Haute Loire*, masonry is, or has been, much attended to.

From the village of La Bastide, a pleasing route may be made into the lower part of the valley of Burzet, by crossing the Col of Juvinas, which commands an excellent view in both directions; and, in particular, of the Coupe d'Aysac. Juvinas is known as being near the spot where a large meteoric stone fell from the sky not many years ago, in broad daylight, and in the presence of several witnesses. I did not lose the opportunity of making every inquiry relative to so rare and interesting an occurrence, and conversing with those who had seen it. The exact spot is a hamlet called Le Creux de Libounez, between Juvinas and St Pierre Colombier. At the time of my inquiry (1839), every one spoke of it as a recent occurrence, and one never to be forgotten. The field was immediately shewn to me, a small enclosure just below the village. I inquired for the actual spectators of the fall. Dolmaas, who had been mentioned to me, was dead; but, with some difficulty, I found two brothers named Serre, who were working with some others in their potato field when the stone fell amongst them. One of these men gave me in his *patois* a most animated account of the scene, and of their terror. With the aid of an interpreter,* I extracted the following particulars, agreed upon

* The native language of this country, as well as of the Haute Loire, is an almost unintelligible *patois*. It is more Italian or Latin than French, and is probably the remains of the old language of Provence. The following Italian phrases struck my ear, "un' ora e mezzo,"—"Aspett' un poc,"—"non ho mai stato,"—"Perchè."

by those present whom I questioned. The stone fell on the 15th June 1821, whilst the sky was clear and the wind north; the hour was half-past four in the afternoon. A long rolling noise was heard, then an explosion like a cannon, which occurred five minutes before the stone fell. It touched the ground within a few feet of them, perforating it to a depth of 7 palms (about $5\frac{1}{2}$ feet) *in a vertical direction*. It burnt the ground to a cindery state. No lightning accompanied the fall. The men were frightened, but not stunned; the noise was heard at a great distance; a man present said that he had heard it at Argentière (five French leagues' distant in a straight line). The people of Libounez thought it was *the devil which had fallen*, and did not venture to dig up the stone for seven days, when it was sprinkled with holy water by the priest. The hole was exactly the size of the stone; there was no scattering of earth. The course of the stone was from the NW.; (this is difficult to reconcile with its having penetrated *vertically*, perhaps the direction in which the noise was heard is meant.) The stone had wedged itself between two others, and could not be removed without breaking it. It weighed 220 pounds, as I was told by the man who had weighed it. It was sold for six francs; but the fragments have been so dispersed, that I with difficulty obtained one or two morsels, although I inquired for them in all the surrounding valleys.

Such was the circumstantial, and apparently authentic narrative, which I gathered from the spectators of this most curious occurrence; and they are entirely corroborated by a manuscript account (or *procès-verbal*), drawn up by the Maire of the Commune of Juvinas, and forwarded by him to the Prefecture of Privas, where I subsequently discovered and copied it. At Privas I also found, amongst a mass of mineral rubbish, preserved at the Prefecture, a small but characteristic specimen of the meteorite itself, which resembles the more ordinary varieties, and is coated with a superficial glossy black vitrification. The meteor of Juvinas is one of a very few which have fallen so near to intelligent spectators as actually to endanger their lives.

The following Table contains a summary of the altitudes referred to in the preceding paper. They were all obtained by myself by means of the barometer. Owing to the distance of the fundamental station, Marseilles, they have no pretension to more than a moderate approximation to the truth.

TABLE of HEIGHTS in the VIVARAIS *above the Level of the Mediterranean, barometrically determined in* 1839 *and* 1841 (*the latter are marked thus* *).

	Metres.	Eng. Feet.
* Alignon and Ardêche, Surface of Lava at Junction of,	341	1117
Antraigues, Bridge,	421	1383
Aubenas, Hotel, second floor,	316	1035
* ... ... first floor,	311	1019
Aysac, Coupe d', Summit of Crater,	805	2640
* Bauzon Volcano, Summit,	1500	4922
* Burzet, Village,	553	1815
Chambon, Lava of, highest part,	1260	4134
* Champ Raphael, Church,	1344	4409
* Fiollonge, Crater,	1121	3678
* Jaujac, Carbonic Acid Spring,	467	1531
... Surface of Lava at Village of,	431	1413
*	413	1354
... Top of Crater,	587	1923
La Bastide, Village,	592	1942
Libounez, Village,	523	1716
* Mesilhac Church,	1155	3791
Montpezat, Bridge of Fontaulier,	468	1534
... Gravenne, highest part,	831	2727
... ... lowest part,	800	2626
... Village (6 obs.),	566	1857
* Neyrac, Carbonic Acid Spring at Village of,	407	1359
... Volcano of,	664	2178
Pal, Crater of, Water Drainage,	1186	3893
... ... highest part,	1383	4537
Pic de l'Etoile,	1282	4204
Pont de la Beaume,	327	1073
* Thuez, Base of Lava Cliff at,	409	1343
... Inn, second Floor,	464	1522
*	478	1569
... Mean adopted,	471	1545
* ... Level of the Ardêche at Pont du Diable,	377	1237
* ... Surface of Lava at,	462	1514
* ... Volcano of Mouleyres Summit,	618	2026
* Vals, Inn,	235	773

II.—*On a Process in the Differential Calculus, and its application to the Solution of certain Differential Equations.* By the Rev. P. KELLAND, *M.A., F.R.SS.L. & E., F.C.P.S., late Fellow of Queen's College, Cambridge; Professor of Mathematics, &c., in the University of Edinburgh.*

(Read 17th December 1849.)

The facilities which are afforded by the introduction of the function $\lfloor\overline{\ }$ into certain classes of Differential expressions are well known. This function has effected the combination and generalization of Problems, which, although found to be capable of solution in particular cases, were regarded rather as isolated and exceptional forms, than as integral parts of some comprehensive expression. But the subject is far from exhausted. Some of the most important Differential Equations have never as yet been solved by a general method. The present Memoir is intended to supply this defect. The process employed differs little from that which I have previously exhibited; but the range of Problems which it embraces is much more extensive, and the Problems themselves are of a more important character.

SECTION I. PRELIMINARY THEOREMS.

1. Let $$y=e^{-\alpha x^r},$$

then $$\left(\frac{1}{x^{r-1}}\frac{d}{dx}\right)y=-\alpha r y,$$

from which it follows, since the operation reproduces the function itself, that $\left(\frac{1}{x^{r-1}}\frac{d}{dx}\right)^{\mu} y=(-\alpha r)^{\mu} y$ (1.), whatever μ may be.

It is necessary to observe, that if μ be negative, the above equation takes no cognizance of functions of integration, which would be introduced by means of the added arbitrary constants; and this remark applies to all our processes.

In the equation $\int_o^{\infty} e^{-\theta}\theta^{n-1}d\theta=\lfloor\overline{n},$

let $$\theta=\alpha x^r,$$

then $$\int_o^{\infty} e^{-\alpha x^r}\alpha^{n-1}x^{r(n-1)}x^r\,d\alpha=\lfloor\overline{n}$$

or $$\frac{\lfloor\overline{n}}{x^{rn}}=\int_o^{\infty}\alpha^{n-1}e^{-\alpha x^r}d\alpha,$$

or, which is the same thing,

$$\frac{\left|\overline{\frac{m}{r}}\right.}{x^m} = \int_o^\infty a^{\frac{m}{r}-1} e^{-\alpha x^r} d\alpha;$$ from which it follows, by equation (1.), that

$$\left|\overline{\frac{m}{r}}\right. \left(\frac{1}{x^{r-1}}\frac{d}{dx}\right)^\mu \cdot \frac{1}{x^m} = \int_o^\infty a^{\frac{m}{r}-1} (-\alpha r)^\mu e^{-\alpha x^r} d\alpha$$

$$= (-r)^\mu \frac{1}{x^{m+r\mu}} \int \theta^{\frac{m+r\mu}{r}-1} e^{-\theta} d\theta$$

$$= (-r)^\mu \frac{\left|\overline{\frac{m+r\mu}{r}}\right.}{x^{m+r\mu}}$$

Consequently,
$$\left(\frac{1}{x^{r-1}}\frac{d}{dx}\right)^\mu \frac{1}{x^m} = (-r)^\mu \frac{\left|\overline{\frac{m+r\mu}{r}}\right.}{\left|\overline{\frac{m}{r}}\right.} \frac{1}{x^{m+r\mu}} \quad \ldots \quad (2.)$$

Let u be a function, supposed to be capable of being represented under the form of a series of functions, such as $e^{-\alpha x^r}$; then we shall have

$$x^{\mu r}\left(\frac{1}{x^{r-1}}\frac{d}{dx}\right)^\mu u = (-r)^\mu \frac{\left|\overline{-\frac{D}{r}+\mu}\right.}{\left|\overline{-\frac{D}{r}}\right.} u \quad \ldots \quad (3.)$$

2. Since
$$\frac{1}{x^{r-1}}\frac{d}{dx}u = \frac{du}{x^{r-1}dx} = \frac{r\,du}{dx^r}$$

it follows that
$$\left(\frac{1}{x^{r-1}}\frac{d}{dx}\right)^\mu u = r^\mu \left(\frac{d}{dx^r}\right)^\mu u$$

$$\therefore \quad x^{\mu r}\left(\frac{d}{d\,.\,x^r}\right)^\mu u = (-1)^\mu \frac{\left|\overline{-\frac{D}{r}+\mu}\right.}{\left|\overline{-\frac{D}{r}}\right.} u \quad \ldots \quad (4.)$$

3. If we write $d_r u$ for $\frac{1}{x^{r-1}}\frac{d}{dx}u$; then from the last Art. it will be evident, without demonstration, that

$$d_r^\mu (uv) = v\,d_r^\mu u + \mu\, d_r v\, d_r^{\mu-1} u + \frac{\mu(\mu-1)}{1\,.\,2} d_r^2 v\, d_r^{\mu-2} u + \&c.$$

4. Let $\phi(d_r)$ be any function of d_r capable of expansion in the form $\phi(d_r) = \Sigma a d_r^\mu$; then, by the last Article

$$\phi(d_r)(uv) = \Sigma a \{v a_r^\mu u + \mu d_r v\, d_r^{\mu-1} u + \&c.\}$$

$$= v(\Sigma a d_r^\mu) u + d_r v (\Sigma a \mu d_r^{\mu-1}) u + \&c.$$

$$=v\,\phi\,(d_r)\,u+d_r\,v\,\phi'(d_r)\,u+\frac{1}{1\,.\,2}\,d^2_r\,v\,\phi''(d_r)\,u+\&c.$$

where $\phi'(d_r)$ is the differential coefficient of $\phi\,(d_r)$ with respect to d_r.

5. In Art. 3, let $v=e^{\beta x^r}$, then

$$d^\mu_r\,(e^{\beta x^r}u)=e^{\beta x^r}\{d^\mu_r u+\mu\,\beta\,r\,d^{\mu-1}_r u$$
$$+\frac{\mu\,(\mu-1)}{1\,.\,2}(\beta r)^2\,d^{\mu-2}_r u+\&c.\}$$
$$=e^{\beta x^r}\,(d_r+\beta\,r)^\mu\,u;$$

and hence, generally, as in Art. 1.,

$$\phi\,(d_r)\,\{e^{\beta x^r}u\}=e^{\beta x^r}\,\phi\,(d_r+\beta\,r)\,u\;.\;.\;.\;.\;.\;(5.)$$

6. Let $\phi(x)=\Sigma\,\mathrm{B}\,e^{\beta x^r}$, then

$$\phi\,(x)\,\psi(d_r)u=\Sigma\,\mathrm{B}\,e^{\beta x^r}\,\psi(d_r)\,u$$
$$=\Sigma\,\mathrm{B}\,\psi(d_r-\beta r)\,e^{\beta x^r}u \text{ by (5)}$$
$$=\Sigma\,\mathrm{B}\,\{\psi(d_r)-\psi(d_r)\,\beta\,r+\psi''(d_r)\frac{(\beta r)^2}{1\,.\,2}-\&c.\}\,e^{\beta x^r}u$$
$$=\psi(d_r)\,\{\Sigma\,\mathrm{B}\,e^{\beta x^r}u\}-\psi'(d_r)\,\{d_r(\Sigma\,\mathrm{B}\,e^{\beta x^r})u\}+\&c.$$
$$=\psi(d_r)\,\{\phi\,(x)\,u\}-\psi'(d_r)\,\{d_r\,\phi\,(x)\,.\,u\}$$
$$+\frac{1}{1\,.\,2}\psi''(d_r)\,\{d^2_r\,\phi\,(x)\,.\,u\}-\&c.$$

7. It is easily seen that, if m be a whole number,

$$d^{-m}_r\log x=\frac{r^{-m}x^{rm}}{\lfloor 1+m}\left\{\log x-\frac{1}{r}\left(\frac{1}{1}+\frac{1}{2}+\&c+\frac{1}{m}\right)\right\};$$

and that in other cases,

$$d^\mu_r\log x=(-r)^{\mu-1}\lfloor\mu\,x^{-r\mu}$$

8. The operation denoted by d^{-m}_r must not be confounded with $\left(x^{r-1}\int dx\right)^m$: it is, in reality, $\left(\int x^{r-1}dx\right)^m$. The one expression can be deduced from the other, thus:—

$$\left(x^{r-1}\int dx\right)^m\log x=x^{r-1}d^{-m}_r x^{-(r-1)}\log x=x^{r-1}d^{-m}_r\frac{1}{p}\,(x^{p-r+1}-x^{q-r+1})$$
$$=\frac{(-r)^{-m}}{p}\left\{\frac{\sqrt{-\left(\frac{p-r+1}{r}+m\right)}}{\sqrt{-\frac{p-r+1}{r}}}\,x^p-\frac{\sqrt{-\left(\frac{q-r+1}{r}+m\right)}}{\sqrt{-\frac{q-r+1}{r}}}\,x^q\right\}x^{rm}$$

where p, q, and $\frac{q}{p}$ are indefinitely small.

Hence $$\left(x^{r-1}\int dx\right)^m \log x = \frac{x^{rm}}{p\,r^m}\left\{\frac{\sqrt{\frac{1+p}{r}}}{\sqrt{\frac{1+p}{r}+m}}x^p - \frac{\sqrt{\frac{1+q}{r}}}{\sqrt{\frac{1+q}{r}+m}}x^q\right\}$$

$$= x^{rm} r^{-m} \frac{\sqrt{\frac{1}{r}}}{\sqrt{\frac{1}{r}+m}}\left\{\log x - \left(\frac{1}{1}+\frac{1}{1+r}+\frac{1}{1+2r}+\&c.+\frac{1}{1+(m-1)r}\right)\right\}$$

9. *To find the differential equation, on the solution of which depends the value of* $d_x^\mu e^{ax}$.

Since $e^{ax}=1+ax+\frac{a^2x^2}{1\,.\,2}+\&c.$

$$d_x^\mu e^{ax} = (-r)^\mu x^{-r\mu}\left\{ax\frac{\sqrt{\frac{-1+r\mu}{r}}}{\sqrt{-\frac{1}{r}}}+\frac{a^2x^2}{1\,.\,2}\frac{\sqrt{\frac{-2+r\mu}{r}}}{\sqrt{-\frac{2}{r}}}+\&c.\right\}$$

This equation can be made to depend on the solution of a differential equation, when r is a whole number; for, in that case, the terms will recur after the rth. To reduce the equation, we observe that

$$\frac{\sqrt{\frac{-(r+1)+r\mu}{r}}}{\sqrt{-\frac{r+1}{r}}} = \frac{r+1}{r+1-r\mu}\frac{\sqrt{\frac{-1+r\mu}{r}}}{\sqrt{-\frac{1}{r}}}, \text{ and so on.}$$

Hence $$d_x^\mu e^{ax} = (-r)^\mu x^{-r\mu}\left[\frac{\sqrt{\frac{-1+r\mu}{r}}}{\sqrt{-\frac{1}{r}}}\left(ax+\frac{r+1}{r+1-r\mu}\frac{a^{r+1}r^{r+1}}{\underline{r+1}}+\&c.\right)\right.$$

$$\left.+\frac{\sqrt{\frac{-2+r\mu}{r}}}{\sqrt{-\frac{2}{r}}}\left(\frac{a^2x^2}{1\,.\,2}+\frac{r+2}{r+2-r\mu}\frac{a^{r+2}x^{r+2}}{\underline{r+2}}+\&c.\right)+\&c.,\&c.,\right]\ldots\ldots(A)$$

Let $$y = x^{1-r\mu}+\frac{a^r x^{r+1-r\mu}}{\underline{r+1}}\frac{r+1}{r+1-r\mu}$$

$$+\frac{a^{2r}x^{2r+1-r\mu}}{\underline{2r+1}}\frac{(r+1)(2r+1)}{(r+1-r\mu)(2r+1-r\mu)}+\&c.\ldots\ldots\ldots;\text{ then}$$

$$\frac{d^{r-1}}{dx^{r-1}}\left(x^{r\mu}\frac{dy}{dx}\right) = a^r\left\{\frac{x}{1}+\frac{a^r x^{r+1}(r+1)}{\underline{r+1}(r+1-r\mu)}+\&c.\right\}$$

$$= a^r x^{r\mu} y$$

Again, if $y=\frac{1}{1\,.\,2}x^{2-r\mu}\frac{\sqrt{\frac{-2+r\mu}{r}}}{\sqrt{-\frac{2}{r}}}+\frac{a^r x^{r+2-r\mu}}{\underline{r+2}}\frac{\sqrt{\frac{-(r+2)+r\mu}{r}}}{\sqrt{-\frac{r+2}{r}}}+\&c.$

$$= \frac{\lfloor \frac{-2+r\mu}{r}}{\lfloor -\frac{2}{r}} \left\{ \frac{1}{1.2} x^{2-r\mu} + \frac{r+2}{r+2-r\mu} \frac{a^r x^{r+2-r\mu}}{\lfloor r+2} \right.$$

$$\left. + \frac{a^{2r} x^{2r+2-r\mu}}{\lfloor 2r+2} \frac{(r+2)(2r+2)}{(r+2-r\mu)(2r+2-r\mu)} + \text{&c.} \right\}$$

$$\therefore \quad \frac{d^{r-1}}{dx^{r-1}} \left(x^{r\mu} \frac{dy}{dx} \right) = \frac{\lfloor \frac{-2+r\mu}{r}}{\lfloor -\frac{2}{r}} a^r \left\{ \frac{x^2}{1.2} + \frac{a^r x^{r+2}}{\lfloor r+2} \frac{r+2}{r+2-r\mu} + \text{&c.} \right\}$$

$$= a^r x^{r\mu} y \ldots . \text{(B)}.$$

The same process will give the same result for all the terms of this expansion except the last. But as $\frac{\lfloor -1+\mu}{\lfloor -1}$ is a factor of that term, it is evidently 0.

Consequently, the equation $\frac{d^{r-1}}{dx^{r-1}} \left(x^{r\mu} \frac{dy}{dx} \right) = a^r x^{r\mu} y$ gives the complete value of $y = d_r^{\mu} e^{ax}$, it being observed that one of the constants of integration is 0. The other constants must be determined by means of equation (A).

10. To find $d_r^{\mu} \cos ax$.

Since $$\cos ax = 1 - \frac{a^2}{\lfloor 2} x^2 + \frac{a^4}{\lfloor 4} x^4 - \text{&c.} \Big\}$$

if $-a^2 = b$, we shall have

$$\left(\frac{1}{x^{r-1}} \frac{d}{dx} \right)^{\mu} \cos ax = (-r)^{\mu} \left\{ \frac{b}{\lfloor 2} \frac{\lfloor \frac{-2+r\mu}{r}}{\lfloor -\frac{2}{r}} x^{2-r\mu} \right.$$

$$\left. + \frac{b^2}{\lfloor 4} \frac{\lfloor \frac{-4+r\mu}{r}}{\lfloor -\frac{4}{r}} x^{4-r\mu} + \frac{b^3}{\lfloor 6} \frac{\lfloor \frac{-6+r\mu}{r}}{\lfloor -\frac{6}{r}} x^{6-r\mu} + \text{&c.} \right\}$$

It remains to express this series in the form of a differential equation when r is a whole number.

Let s be any whole number less than $r+1$, $S = (-r)^{\mu} b^s \frac{\lfloor \frac{-2s+r\mu}{r}}{\lfloor -\frac{2s}{r}}$; then the series may be written

$$y = S \left\{ \frac{x^{2s-r\mu}}{\lfloor 2s} + \frac{b^r}{\lfloor 2r+2s} \frac{(r+2s)(2r+2s)}{(r+2s-r\mu)(2r+2s-r\mu)} x^{2r+2s-r\mu} + \text{&c.} \right\}$$

$$\therefore \frac{d^{r-1}}{dx^{r-1}} \left(x^{r\mu} \frac{dy}{dx} \right) = S \left\{ \frac{2s-r\mu}{2s} \frac{x^{2s-r}}{\lfloor 2s-r} + \frac{b^r}{\lfloor 2s+r-1} \frac{x^{2s+r}}{r+2\,s-r\mu} \right.$$

$$+\frac{b^{2r}}{\underline{2s+3r-1}}\frac{(r+2s)(2r+2s)x^{2s+3r}}{(r+2s-r\mu)(2r+2s-r\mu)(3r+2s-r\mu)}+\&c.\Big\}$$

$$\frac{d}{dx}x^{-r\mu}\frac{d^{r-1}}{dx^{r-1}}x^{r\mu}\frac{dy}{dx}=S\left\{\frac{(2s-r\mu)(2s-r-r\mu)}{2s(2s-r)}\frac{x^{2s-r-1-r\mu}}{\underline{2s-r-1}}\right.$$

$$\left.+\frac{b^r}{\underline{2s+r-1}}x^{2s+r-1-r\mu}+\frac{b^{2r}}{\underline{2s+3r-1}}\frac{(r+2s)(2r+2s)x^{2s+3r-1-r\mu}}{(r+2s-r\mu)(2r+2s-r\mu)}+\&c.\right\}$$

$$\frac{d^{r-1}}{dx^{r-1}}x^{r\mu}\frac{d}{dx}x^{-r\mu}\frac{d^{r-1}}{dx^{r-1}}x^{r\mu}\frac{dy}{dx}=S\left\{\frac{(2s-r\mu)(2s-r-r\mu)}{2s(2s-r)}\frac{x^{2s-2r}}{\underline{2s-2r}}\right.$$

$$\left.+\frac{b^r}{\underline{2s}}x^{2s}+\frac{b^{2r}}{\underline{2r+2s}}\frac{(r+2s)(2r+2s)}{(r+2s-r\mu)(2r+2s-r\mu)}x^{2s+2r}+\&c.\right\}$$

$$=S\frac{(2s-r\mu)(2s-r-r\mu)}{2s(2s-r)\ \underline{2s-2r}}x^{2s-2r}+b^r x^{r\mu}y.$$

If $s<r$, $\dfrac{1}{\underline{2s-2r}}=0$

If $s=r$, $S\dfrac{(2s-r\mu)(2s-r-r\mu)}{2s(2s-r)\underline{2s-2r}}x^{2s-2r}=(-r)^{\mu}\dfrac{\underline{-\frac{2s}{r}+\mu+2}}{\underline{-\frac{2s}{r}+2}}$

$$=(-r)^{\mu}\frac{\underline{\mu}}{\underline{0}}=0;$$

Therefore, generally, if $y=d_r^{\mu}\cos ax$, we have

$$\frac{d^{r-1}}{dx^{r-1}}x^{r\mu}\frac{d}{dx}x^{-r\mu}\frac{d^{r-1}}{dx^{r-1}}x^{r\mu}\frac{dy}{dx}=(-a^2)^r x^{r\mu}y;$$

or, if $x^{r\mu}y=z$, the equation may be written

$$\left(\frac{d^{r-1}}{dx^{r-1}}x^{r\mu}\frac{d}{dx}x^{-r\mu}\right)^2 z=(-a^2)^r z,\ \ldots\ (C)$$

a form which exhibits its relation with the differential equation resulting from e^{ax}.

11. There exist numerous relations between the differential coefficients of a function; such as the following:—

Since $$x^{\mu r}\left(\frac{1}{x^{r-1}}\frac{d}{dx}\right)^{\mu}u=(-r)^{\mu}\frac{\underline{-\frac{D}{r}+\mu}}{\underline{-\frac{D}{r}}}u;$$

let $$\phi\left(\frac{D}{r}\right)=\frac{\underline{-\frac{D}{r}+\mu}}{\underline{-\frac{D}{r}}};\ \text{then}$$

$$\phi\left(\frac{\mathrm{D}}{r}+1\right)=\frac{\sqrt{-\frac{\mathrm{D}}{r}-1+\mu}}{\sqrt{-\frac{\mathrm{D}}{r}-1}}=\frac{\frac{\mathrm{D}}{r}+1}{\frac{\mathrm{D}}{r}-\mu+1}\,\phi\left(\frac{\mathrm{D}}{r}\right)$$

Hence
$$\phi\left(\frac{\mathrm{D}}{r}\right)=\frac{\sqrt{\frac{\mathrm{D}}{r}+1}}{\sqrt{\frac{\mathrm{D}}{r}-\mu+1}}$$

$$=x^{(\mu-1)r}\frac{\sqrt{\frac{\mathrm{D}}{r}+\mu}}{\sqrt{\frac{\mathrm{D}}{r}}}\,x^{-(\mu-1)r}$$

$$=x^{(\mu-1)r}\frac{\sqrt{\frac{-\mathrm{D}}{-r}+\mu}}{\sqrt{\frac{-\mathrm{D}}{-r}}}\,x^{-(\mu-1)r}$$

Hence
$$x^{\mu r}\left(\frac{1}{x^{r-1}}\frac{d}{dx}\right)^{\mu}u=(-1)^{\mu}\,x^{-r}\left(x^{r+1}\,\frac{d}{dx}\right)^{\mu}x^{-(\mu-1)r}\,u,$$

a relation between differentials with positive and negative values of r.

Section II. Application of the preceding Theory to the Solution of Differential Equations.

12. The first example which I propose to give is the solution of equation (B), Art. 9.

Ex. 1. $\frac{d^{r-1}}{dx^{r-1}}\left(x^{r\mu}\frac{dy}{dx}\right)=a^r x^{r\mu}y$, where μ may be anything whatever.

Let $x^{r\mu}y=z$,

$\therefore$ $$\frac{d^{r-1}}{dx^{r-1}}x^{r\mu}\frac{d}{dx}x^{-r\mu}z=a^r z$$

or $$x^{r-1}\frac{d^{r-1}}{dx^{r-1}}x^{r\mu-1}\mathrm{D}\,x^{-r\mu}z=a^r x^{r-1}z$$

or $$\mathrm{D}(\mathrm{D}-1)\ldots.(\mathrm{D}-r+2)\times(\mathrm{D}-r\mu+1)\,e^{-\theta}z=a^r e^{r\theta}e^{-\theta}z$$

Let $\frac{z}{x}$ or $e^{-\theta}z=f\left(-\frac{\mathrm{D}}{r}\right)v$; then

$$\mathrm{D}(\mathrm{D}-1)\ldots.(\mathrm{D}-r+2)\times(\mathrm{D}-r\mu+1)f\left(-\frac{\mathrm{D}}{r}\right)v=$$

$$a^r e^{r\theta}f\left(-\frac{\mathrm{D}}{r}\right)v=a^r f\left(-\frac{\mathrm{D}}{r}+1\right)e^{r\theta}v.$$

Which equation is reduced to

$$\mathrm{D}(\mathrm{D}-1)\ldots\ldots(\mathrm{D}-r+1)\,v=a^r e^{r\theta}\,v \qquad (1.)$$

by the condition $f\left(-\frac{\mathrm{D}}{r}+1\right)=\frac{\mathrm{D}-r\mu+1}{\mathrm{D}-r+1}\,f\left(-\frac{\mathrm{D}}{r}\right)$ (2.)

$$=\frac{-\frac{\mathrm{D}}{r}-\frac{1}{r}+\mu}{-\frac{\mathrm{D}}{r}-\frac{1}{r}+1}\,f\left(-\frac{\mathrm{D}}{r}\right)$$

the solution of which equation is

$$f\left(-\frac{\mathrm{D}}{r}\right)=\frac{\left|-\frac{\mathrm{D}}{r}-\frac{1}{r}+\mu\right.}{\left|-\frac{\mathrm{D}}{r}-\frac{1}{r}+1\right.}$$

whence $\frac{z}{x}$ or $x^{r\mu-1}y=\frac{\left|-\frac{\mathrm{D}}{r}-\frac{1}{r}+\mu\right.}{\left|-\frac{\mathrm{D}}{r}-\frac{1}{r}+1\right.}\,v$

$$=x^{r-1}\,\frac{\left|-\frac{\mathrm{D}}{r}+\mu-1\right.}{\left|-\frac{\mathrm{D}}{r}\right.}\,x^{-r+1}v$$

$$=(-r)^{-\mu+1}\,x^{\mu r-1}\,d_r^{\mu-1}\,x^{-r+1}v$$

Now v is the solution of equation (1); or, which is the same thing, of

$$\frac{d^r v}{dx^r}=a^r\,v$$

$\therefore$ $v=\mathrm{A}\,e^{ax}$ is a value of v; and hence, finally,

$$y=(-r)^{-\mu+1}\,\mathrm{A}\,d_r^{\mu-1}\,\frac{e^{ax}}{x^{r-1}}$$

$$=\mathrm{B}\,d_r^{\mu}\,e^{ax}, \text{ where B is an arbitrary constant.}$$

It is evident that any other of the r roots of the equation $\frac{d^r v}{dx^r}=a^r\,v$ would satisfy the conditions, or that any one of the roots of a^r will produce the same result as a itself.

This example affords an excellent illustration of the process.

Let us take, in the next place, the equation which occurs in the theory of the Figure of the Earth; an equation which has been solved by Professor Boole, in his admirable Memoir in the *Philosophical Transactions* for 1844, p. 251, but whose process is necessarily restricted to integral values of i. The equation is—

Ex. 2. $$\frac{d^2u}{dx^2}-q^2u-\frac{i(i+1)\,u}{x^2}=0.$$

By the usual process, this equation becomes

$$\mathrm{D}(\mathrm{D}-1)u-q^2e^{2\theta}u-i(i+1)u=0$$

or
$$(\mathrm{D}+i)(\mathrm{D}-i-1)u-q^2e^{2\theta}u=0.$$

Let $u=e^{-i\theta}f\left(-\frac{\mathrm{D}}{2}\right)v$, then

$$(\mathrm{D}+i)(\mathrm{D}-i-1)e^{-i\theta}f\left(-\frac{\mathrm{D}}{2}\right)v-q^2e^{-i\theta}f\left(-\frac{\mathrm{D}}{2}+1\right)e^{2\theta}v=0$$

or
$$\mathrm{D}(\mathrm{D}-2i-1)f\left(-\frac{\mathrm{D}}{2}\right)v-q^2f\left(-\frac{\mathrm{D}}{2}+1\right)e^{2\theta}v=0$$

an equation which, by omitting the function of 0, is reduced to

$$\mathrm{D}(\mathrm{D}-1)v-q^2e^{2\theta}v=0 \qquad (1.)$$

by the condition

$$f\left(-\frac{\mathrm{D}}{2}+1\right)=\frac{\mathrm{D}-2i-1}{\mathrm{D}-1}f\left(-\frac{\mathrm{D}}{2}\right) \qquad (2.)$$

$$=\frac{-\frac{\mathrm{D}}{2}+i+\frac{1}{2}}{-\frac{\mathrm{D}}{2}+\frac{1}{2}}f\left(-\frac{\mathrm{D}}{2}\right)$$

$$\therefore \quad f\left(-\frac{\mathrm{D}}{2}\right)=\frac{\sqrt{-\frac{\mathrm{D}}{2}+i+\frac{1}{2}}}{\sqrt{-\frac{\mathrm{D}}{2}+\frac{1}{2}}}$$

and
$$u=e^{-i\theta}\frac{\sqrt{-\frac{\mathrm{D}}{2}+i+\frac{1}{2}}}{\sqrt{-\frac{\mathrm{D}}{2}+\frac{1}{2}}}v$$

$$=e^{-(i-1)\theta}\frac{\sqrt{-\frac{\mathrm{D}}{2}+i}}{\sqrt{-\frac{\mathrm{D}}{2}}}e^{-\theta}v$$

$$=(-2)^{-i}x^{i+1}\left(\frac{1}{x}\frac{d}{dx}\right)^i\frac{v}{x}$$

But the solution of Equation (1.) is

$$v=\mathrm{A}e^{qx}+\mathrm{B}e^{-qx}:$$

Hence the complete solution of the given equation is

$$u=(-2)^{-i}x^{i+1}\left(\frac{1}{x}\frac{d}{dx}\right)^i\frac{1}{x}(\mathrm{A}e^{qx}+\mathrm{B}e^{-qx})$$

This solution is susceptible of another form by Art. 11; for by that Article,

$$\left(\frac{1}{x}\frac{d}{dx}\right)^i\frac{v}{x}=(-1)^ix^{-(2i+2)}\left(x^3\frac{d}{dx}\right)^i\frac{v}{x^{2i-1}}$$

$$\therefore \quad u=2^{-i}\frac{1}{x^{i+1}}\left(x^3\frac{d}{dx}\right)^i\frac{\mathrm{A}e^{qx}+\mathrm{B}e^{-qx}}{x^{2i-1}},$$

in which form the solution has been given by Professor BOOLE, when i is a whole number.

Ex. 3. *Solution of the Equation for Laplace's Functions.*

This equation has been reduced by Professor BOOLE, in the *Cambridge Mathematical Journal*, New Series, vol. i., p. 15, to the form

$$(1-\mu^2)\frac{d^2v}{d\mu^2}+2(a-1)\mu\frac{dv}{d\mu}+\{n(n+1)-a(a-1)\}v=0 \quad \ldots (1)$$

where u, the quantity sought, is connected with v by the equation

$$u=(1-\mu^2)^{-\frac{a}{2}}v \quad (2).$$

Putting $\mu=e^{\theta}$ and writing D for $\frac{d}{d\theta}$, we have

$$\{(e^{-2\theta}-1)\,\mathrm{D}\,(\mathrm{D}-1)+2(a-1)\,\mathrm{D}+n(n+1)-a(a-1)\}v=0$$

or $$e^{-2\theta}\,\mathrm{D}\,(\mathrm{D}-1)\,v-(\mathrm{D}-\overline{a+n})\,(\mathrm{D}-\overline{a-n-1})\,v=0$$

or $$\mathrm{D}\,(\mathrm{D}-1)\,v-(\mathrm{D}-a-n-2)\,(\mathrm{D}-a+n-1)\,e^{2\theta}v=0$$

Let, therefore, $v=f\left(-\frac{\mathrm{D}}{2}\right)w$ (3), and this equation becomes

$$\mathrm{D}\,(\mathrm{D}-1)f\left(-\frac{\mathrm{D}}{2}\right)w-(\mathrm{D}-a-n-2)\,(\mathrm{D}-a+n-1)f\left(-\frac{\mathrm{D}}{2}+1\right)e^{2\theta}w=0$$

or $$\mathrm{D}\,(\mathrm{D}-1)\,w-(\mathrm{D}-a-n-1)\,(\mathrm{D}-a-n-2)\,e^{2\theta}w=0 \quad (4),$$ provided

$$f\left(-\frac{\mathrm{D}}{2}+1\right)=\frac{\mathrm{D}-a-n-1}{\mathrm{D}-a+n-1}f\left(-\frac{\mathrm{D}}{2}\right)$$

$$=\frac{-\frac{\mathrm{D}}{2}+\frac{a}{2}+\frac{n}{2}+\frac{1}{2}}{-\frac{\mathrm{D}}{2}+\frac{a}{2}-\frac{n}{2}+\frac{1}{2}}f\left(-\frac{\mathrm{D}}{2}\right)$$

an equation of which the solution is evidently

$$f\left(-\frac{\mathrm{D}}{2}\right)=\frac{\sqrt{-\frac{\mathrm{D}}{2}+\frac{a}{2}+\frac{n}{2}+\frac{1}{2}}}{\sqrt{-\frac{\mathrm{D}}{2}+\frac{a}{2}-\frac{n}{2}+\frac{1}{2}}} \quad (5.)$$

and by (3)

$$v=f\left(-\frac{\mathrm{D}}{2}\right)w$$

$$=\frac{\sqrt{-\frac{\mathrm{D}}{2}+\frac{a}{2}+\frac{n}{2}+\frac{1}{2}}}{\sqrt{-\frac{\mathrm{D}}{2}+\frac{a}{2}-\frac{n}{2}+\frac{1}{2}}}w$$

$$=e^{-(n-a-1)\theta}\frac{\sqrt{-\frac{\mathrm{D}}{2}+n}}{\sqrt{-\frac{\mathrm{D}}{2}}}e^{(n-a-1)\theta}w$$

$$= (-2)^{-n} \mu^{n+\alpha+1} \left(\frac{1}{\mu}\frac{d}{d\mu}\right)^n \mu^{n-\alpha-1} w \quad (6)$$

a result which is true, whether n be integral or fractional.

The value of w from Equation (4.) is easily found, and is given by Mr BOOLE in the form

$$w = (1+\mu)^{n+\alpha} \downarrow(\phi) + (1-\mu)^{n+\alpha} \chi(\phi)$$

whence that of u is found.

Let us take, as our next example, the equation which has been discussed by M. POISSON in the *Journal de l'Ecole Polytechnique*, cah. 17, p. 614; and by Professor BOOLE in the *Philosophical Transactions* for 1844, p. 254.

Ex. 4. $$(1-x^2)\frac{d^2u}{dx^2} + \left\{\frac{p+1}{x} - (4n-p+1)\,x\right\}\frac{du}{dx} - 2n(2n-p)\,u = 0. \quad \ldots \quad (1.)$$

The symbolical form of this equation is

$$u - \frac{(D+2n-2)(D+2n-2-p)}{D(D+p)}\, e^{2\theta} u = 0. \quad \ldots \quad (2.)$$

Let $u = f\left(-\frac{D}{2}\right) v \quad \ldots \quad (3.)$; then

$$f\left(-\frac{D}{2}\right) v - \frac{(D+2n-2)(D+2n-2-p)}{D(D+p)} f\left(-\frac{D}{2}+1\right) e^{2\theta} v = 0 \quad \ldots \quad (4.)$$

This last equation may be reduced to an integrable form in various ways:

1. By making $$f\left(-\frac{D}{2}+1\right) = \frac{D+p}{D-1} \cdot \frac{D+2n-3}{D+2n-2-p} f\left(-\frac{D}{2}\right)$$

or
$$f\left(-\frac{D}{2}\right) = \frac{\left|-\frac{D}{2}-\frac{p}{2}\right|}{\left|-\frac{D}{2}+\frac{1}{2}\right|} \cdot \frac{\left|-\frac{D}{2}-n+\frac{3}{2}\right|}{\left|-\frac{D}{2}-n+1+\frac{p}{2}\right|}$$

$$= e^{\theta} \frac{\left|-\frac{D}{2}-\frac{p}{2}-\frac{1}{2}\right|}{\left|-\frac{D}{2}\right|} e^{-(2n-p-1)\theta} \frac{\left|-\frac{D}{2}-\frac{p}{2}+\frac{1}{2}\right|}{\left|-\frac{D}{2}\right|} e^{(2n-p-2)\theta}$$

or
$$u = (-2)^p x^{-p} \left(\frac{1}{x}\frac{d}{dx}\right)^{-\frac{p+1}{2}} x^{-2n+2} \left(\frac{1}{x}\frac{d}{dx}\right)^{-\frac{p-1}{2}} x^{2n-p-2} v$$

and equation (4.) becomes

$$v - \frac{(D+2n-2)(D+2n-3)}{D(D-1)}\, e^{2\theta} v = 0 \quad \ldots \quad (5.)$$

which is a known form, and thus the given equation is completely solved.

It is evident that our solution reduces the operation to that of ordinary differentiation or integration, when p is an odd integer, positive or negative. By varying the process, however, we can obtain other forms of the solution of this

equation, which are reduced to ordinary differentiation or integration in other cases. The other forms are as follow:

2. By making $f\left(-\frac{D}{2}+1\right)=\frac{D}{D+2n-2}f\left(-\frac{D}{2}\right)$

or
$$f\left(-\frac{D}{2}\right)=\frac{\sqrt{-\frac{D}{2}}}{\sqrt{-\frac{D}{2}-n+1}}$$
$$=e^{-(2n-2)\theta}\frac{\sqrt{-\frac{D}{2}+n-1}}{\sqrt{-\frac{D}{2}}}e^{(2n-2)\theta}$$

and, consequently, $u=(-2)^{-(n-1)}\left(\frac{1}{x}\frac{d}{dx}\right)^{n-1}x^{2n-2}r$; where v is found from the equation of the first order $v-\frac{D+2n-2-p}{D+p}e^{2\theta}v=\left(f\left(-\frac{D}{2}\right)\right)^{-1}0$.

This solution is of the ordinary form, whenever n is a positive or negative integer.

By making $f(-\frac{D}{2}+1)=\frac{D}{D+2n-2-p}f\left(-\frac{D}{2}\right)$

or
$$f\left(-\frac{D}{2}\right)=\frac{\sqrt{-\frac{D}{2}}}{\sqrt{-\frac{D}{2}-n+1+\frac{p}{2}}}$$
$$=e^{-(2n-2-p)}\frac{\sqrt{-\frac{D}{2}+n-1-\frac{p}{2}}}{\sqrt{-\frac{D}{2}}}e^{2n-2-p}$$

and
$$u=(-2)^{-(n-\frac{p}{2}-1)}\left(\frac{1}{x}\frac{d}{dx}\right)^{n-\frac{p}{2}-1}x^{2n-p-2}v;$$

where r is found from the solution of the equation of the first order,
$$v-\frac{D+2n-2}{D+p}e^{2\theta}v=f\left(-\frac{D}{2}\right)^{-1}.0.$$

This solution is of the ordinary form when $n-\frac{p}{2}$ is an integer.

4. By making $f(-\frac{D}{2}+1)=\frac{D+p}{D+2n-p-2}f\left(-\frac{D}{2}\right)$,

or
$$f\left(-\frac{D}{2}\right)=\frac{\sqrt{-\frac{D}{2}-\frac{p}{2}}}{\sqrt{-\frac{D}{2}-n+\frac{p}{2}+1}}$$
$$=e^{-(2n-p-2)\theta}\frac{\sqrt{-\frac{D}{2}+n-p-1}}{\sqrt{-\frac{D}{2}}}e^{(2n-p-2)\theta}$$

and $$u=(-2)^{-(n-p-1)}x^{-p}\left(\frac{1}{x}\frac{d}{dx}\right)^{n-p-1}x^{2n-p-2}v,$$

where v is found from the solution of the equation of the first order,

$$v-\frac{D+2n-2}{D}e^{2\theta}v=f\left(-\frac{D}{2}\right)^{-1}0.$$

This solution is of the ordinary form when $n-p$ is an integer.

This last form appears to have been overlooked by Professor BOOLE.

It may be remarked, that we have omitted $f\left(-\frac{D}{2}\right)^{-1}0$ in the first solution. The reason is, that equation (5) being of the second order, will contain two arbitrary constants, and will thus render the solution of the given equation complete, without the introduction of any other terms.

Ex. 5. $$(1-x^2)\frac{d^2u}{dx^2}-(2m+1)x\frac{du}{dx}-(m^2-q^2)u=0.$$

This equation has been solved by Professor BOOLE; but it is requisite to shew that his solution is not confined to integral values of m.

The symbolical form of the equation is

$$u-\frac{(D+m-2)^2-q^2}{D(D-1)}e^{2\theta}u=0.$$

Let $u=e^{-m\theta}f(-D)v$; then

$$f(-D)v-\frac{D-2)^2-q^2}{(D-m)(D-m-1)}f(-D+2)e^{2\theta}v=0.$$

By making $$f(-D)=\frac{D(D-1)}{(D-m)(D-m-1)}f(-D+2);$$

or $$f(-D)=\frac{\sqrt{-D+m}}{\sqrt{-D}}$$

this equation is reduced to

$$v-\frac{(D-2)^2-q^2}{D(D-1)}e^{2\theta}v=0 \quad (1)$$

and $$u=e^{-m\theta}\frac{\sqrt{-D+m}}{\sqrt{-D}}v=(-1)^{-m}\left(\frac{d}{dx}\right)^m v$$

$$=\left(\frac{d}{dx}\right)^m\left\{A\cos(q\sin^{-1}x)+B\sin(q\sin^{-1}x)\right\}$$

This equation is, however, susceptible of a solution in a different form from that which we have just exhibited.

Let $u=f\left(-\frac{D}{2}\right)v$, then

$$f\left(-\frac{D}{2}\right)v-\frac{(D+m-2-q)(D+m-2+q)}{D(D-1)}f\left(-\frac{D}{2}+1\right)e^{2\theta}v=0$$

By making $$f\left(-\frac{D}{2}+1\right) = \frac{(D-1)}{D+m-2+q} f\left(-\frac{D}{2}\right) \quad \ldots (1.)$$

this equation is reduced to the equation of the first order.

$$v - \frac{D+m-2-q}{D} e^{2\theta} v = f\left(-\frac{D}{2}\right)^{-1} 0 \quad \ldots (2.)$$

Also $$f\left(-\frac{D}{2}\right) = \frac{\sqrt{-\frac{D}{2}+\frac{1}{2}}}{\sqrt{-\frac{D}{2}-\frac{m}{2}+1-\frac{q}{2}}}$$

$$= e^{-(m-2+q)\theta} \frac{\sqrt{-\frac{D}{2}+\frac{m}{2}-\frac{1}{2}+\frac{q}{2}}}{\sqrt{-\frac{D}{2}}} e^{(m-2+q)\theta}$$

$$u = (-2)^{-(m+q-1)} x \left(\frac{1}{x}\frac{d}{dx}\right)^{m+q-1} x^{m+q-2} v$$

which is of the ordinary form when $m+q$ is a whole number.

The same is true if $-q$ be substituted for q.

Section III. Comparison of Processes.

We shall occupy this Section in the comparison of the different methods which may be employed in the solution of a given Differential Equation.

13. *To find the value of* $\sqrt{-D} \cdot v$.

Suppose v expressed in the form

$$v = \frac{A}{x^n} + \frac{B}{x^{n+r}} + \&c.$$

$$= A e^{-n\theta} + B e^{-(n+r)\theta} + \&c.$$

then $$\sqrt{-D} \cdot v = A\sqrt{n}\, e^{-n\theta} + B\sqrt{n+r}\, e^{-(n+r)\theta} + \&c.$$

$$= A\frac{\sqrt{n}}{x^n} + B\frac{\sqrt{n+r}}{x^{n+r}} + \&c.$$

$$= \int_0^\infty e^{-\alpha x} d\alpha \, (A\alpha^{n-1} + B\alpha^{n+r-1} + \ldots)$$

$$= \int_0^\infty e^{-\alpha x} \frac{d\alpha}{\alpha} (A\alpha^n + B\alpha^{n+r} + \&c.$$

Hence, if $$v = f(x)$$

$$\sqrt{-D} f(x) = \int_0^\infty e^{-\alpha x} \frac{d\alpha}{\alpha} f\left(\frac{1}{\alpha}\right)$$

14. All equations of the form $y-mx^r\frac{d^n y}{dx^n}=X$ can be converted into equations in $\frac{d^r v}{dx^r}$.

For $$y-mx^{r-n}(-1)^n\frac{\sqrt{-D+n}}{\sqrt{-D}}y=X$$

Let $y=\sqrt{-D}\,.\,v$, then

$$\sqrt{-D}\,.\,v-mx^{r-n}(-1)^n\sqrt{-D+n}\,v=X$$

or $$v-m(-1)^n\frac{\sqrt{-D+r}}{\sqrt{-D}}e^{(r-n)\theta}v=(\sqrt{-D})^{-1}X$$

or $$v-m(-1)^{-(r-n)}x^r\left(\frac{d}{dx}\right)^r x^{r-n}v=(\sqrt{-D})^{-1}X$$

or, if $x^{r-n}v=z$,

$$z-m(-1)^{-(r-n)}x^{2r-n}\frac{d^r z}{dx^r}=x^{r-n}(\sqrt{-D})^{-1}X$$

wherefore the above equation is reduced to an ordinary differential equation whatever be x, provided r is an integer.

As an example, let us take the simplest case, of which the solution will be found at pp. 257, 258 of my Memoir on General Differentiation, Part III.

Ex. 1. $$y-a\sqrt{-1}\,x\frac{d^{\frac{1}{2}}y}{dx^{\frac{1}{2}}}=0$$

Suppose $y=\sqrt{-D}\,v$,

then $$v\sqrt{x}+a\sqrt{x}\frac{\sqrt{-D+1}}{\sqrt{-D}}(v\sqrt{x})=C\sqrt{x}$$

or $$z-ax^{\frac{3}{2}}\frac{dz}{dx}=C\sqrt{x},\text{ where } z=v\sqrt{x};$$

$$\therefore\quad z=e^{-\frac{2}{a\sqrt{x}}}\left\{-\frac{A}{a}-\frac{C}{a}\int\frac{dx}{x}e^{\frac{2}{a\sqrt{x}}}\right\}$$

and $$y=\sqrt{-D}\,\frac{e^{-\frac{2}{a\sqrt{x}}}}{\sqrt{x}}\left\{-\frac{A}{a}-\frac{C}{a}\int\frac{dx}{x}e^{\frac{2}{a\sqrt{x}}}\right\}\quad\ldots\ (1.)$$

$$=\int_0^\infty e^{-px}\frac{dp}{\sqrt{p}}e^{-\frac{2\sqrt{p}}{a}}\left\{-\frac{A}{a}+\frac{C}{a}\int\frac{dp}{p}e^{-\frac{2\sqrt{p}}{a}}\right\}\quad\ldots\ (2.)$$

The constants A and C are not both of them arbitrary. In fact, it will be seen that $C=\frac{A}{\sqrt{0}}$, so that the final form of equation (1.) is

$$y=-\frac{A}{a}\sqrt{-D}\,\frac{e^{-\frac{2}{a\sqrt{x}}}}{\sqrt{x}}\left\{1+\frac{1}{\sqrt{0}}\int\frac{dx}{x}e^{\frac{2}{a\sqrt{x}}}\right\}$$

Ex. 2. To solve the equation

$$\frac{dy}{dx} - a\frac{d^{\frac{1}{2}}y}{dx^{\frac{1}{2}}} - \frac{1}{2}\frac{y}{x} = X.$$

1. By the method employed at p. 269 of my previous Memoir, this equation becomes

$$(-D+\tfrac{1}{2})y + a(-1)^{\frac{1}{2}}\frac{\sqrt{-D+1}}{\sqrt{-D+\frac{1}{2}}}e^{\frac{\ell}{2}}y = -Xx$$

or

$$\frac{y}{x} - a\frac{d^{-\frac{1}{2}}}{dx^{-\frac{1}{2}}}\cdot\frac{y}{x} = \frac{1}{x}\cdot\frac{Xx}{D-\frac{1}{2}}$$

$$= \frac{1}{\sqrt{x}}\int\frac{X\,dx}{x^{\frac{1}{2}}}$$

$$= P, \text{ suppose};$$

then

$$\frac{y}{x} = \frac{1}{\frac{d}{dx} - a^2}\left(\frac{dP}{dx} + a\frac{d^{\frac{1}{2}}P}{dx^{\frac{1}{2}}}\right)$$

$$y = Axe^{a^2x} + xe^{a^2x}\int e^{-a^2x}dx\left(\frac{dP}{dx} + a\frac{d^{\frac{1}{2}}P}{dx^{\frac{1}{2}}}\right)$$

2. By Professor Boole's method, given in the *Philosophical Magazine* for February 1847. The given equation when written

$$x(d - a\,d^{\frac{1}{2}})y - \tfrac{1}{2}y = Xx;$$

may be thrown into the form

$$f(d)\left(x\,F(d)\right)y = Xx; \text{ provided}$$

$$f(d)\,F(d) = d - a\,d^{\frac{1}{2}}, \text{ and}$$

$$f'(d)\,F(d) = -\tfrac{1}{2}.$$

We have, therefore,

$$f(d) = (d^{\frac{1}{2}} - a)^{-1}$$

$$F(d) = (d^{\frac{1}{2}} - a)^2\,d^{\frac{1}{2}}$$

and

$$y = d^{-\frac{1}{2}}(d^{\frac{1}{2}} - a)^{-2}\{x^{-1}(d^{\frac{1}{2}} - a)\,Xx\}$$

3. By Mr Hargreave's method, given in the *Philosophical Transactions* for 1848, p. 31.

By changing d into x, and x into $-d$ in the equation

$$x(d - a\,d^{\frac{1}{2}})y - \tfrac{1}{2}y = Xx \quad \ldots\ (1.)$$

it becomes

$$d(x - a\,x^{\frac{1}{2}})y + \tfrac{1}{2}y = d\,X \quad \ldots\ (2.)$$

which, being an ordinary linear equation, gives, as its solution, if we write d^{-1} for $\int d\,x$,

$$y = x^{-\frac{1}{2}}(x^{\frac{1}{2}} - a)^{-2}\,d^{-1}\{(x^{\frac{1}{2}} - a)\,dX\} \quad \ldots\ (3.)$$

Now, since equation (2.) has been derived from equation (1.) by the change of d into x, and x into $-d$, equation (1.) may be derived from equation (2.) by changing x into d and d into $-x$. Consequently (in some cases, at least, of which this form is an instance) the *solution* of equation (1.) may be derived from the *solution* of equation (2.) by the same change. Hence the solution of the given equation is

$$y=d^{-\frac{1}{2}}\,(d^{\frac{1}{2}}-a)^{-2}\,\{x^{-1}\,(d^{\frac{1}{2}}-a)\,x\,\mathrm{X}\},$$

which is precisely the solution given by Professor Boole's process.

We shall conclude the present Memoir by comparing the methods here exhibited in two particular cases of the second example.

Case 1. Let $\mathrm{X}=0$; then the first method gives $y=\mathrm{A}\,x\,e^{a^2x}$: whilst the second and third methods give

$$y=d^{-\frac{1}{2}}\,(d^{\frac{1}{2}}-a)^{-2}\ .\ 0.$$

Now, in a former paper (*Transactions of the Royal Society of Edinburgh*, vol. xiv., p. 252), I have shewn, in Cor. 1. to Example 3, that

$$(d^{\frac{1}{2}}-a)^{-2}\ .\ 0=\mathrm{A}\,e^{a^2x}+\mathrm{B}\,x\,e^{a^2x}$$

Hence

$$y=d^{-\frac{1}{2}}\{\mathrm{A}\,e^{a^2x}+\mathrm{B}\,x\,e^{a^2x}\}$$

$$=\frac{\mathrm{A}}{a}\,e^{a^2x}+\frac{\mathrm{B}\,x}{a}\,e^{a^2x}-\frac{\mathrm{B}}{2\,a^3}\,e^{a^2x}$$

The condition $\mathrm{B}=2\,\mathrm{A}\,a^2$ reduces this solution to the former.

Case 2. Let

$$\mathrm{X}=b\left(\frac{1}{x^{\frac{3}{2}}}+\frac{a\sqrt{-1}}{\sqrt{\pi}}\,\frac{1}{x}\right);$$

then, by the first method,

$$\mathrm{P}=\frac{1}{\sqrt{x}}\int\frac{\mathrm{X}\,dx}{\sqrt{x}}=\frac{b}{\sqrt{x}}\left\{-\frac{1}{x}-\frac{2\,a\sqrt{-1}}{\sqrt{\pi}}\,\frac{1}{\sqrt{x}}\right\}$$

$$=-\frac{b}{x^{\frac{3}{2}}}-\frac{2\,ab\sqrt{-1}}{\sqrt{\pi}}\,\frac{1}{x}$$

$$\frac{d\,\mathrm{P}}{dx}+a\,\frac{d^{\frac{1}{2}}\mathrm{P}}{dx^{\frac{1}{2}}}=b\left(\frac{3}{2}\,\frac{1}{x^{\frac{5}{2}}}+\frac{a^2}{x^{\frac{3}{2}}}\right)$$

$$y=\mathrm{A}\,x\,e^{a^2x}+b\,x\,e^{a^2x}\int e^{-a^2x}\,dx\,\left(\frac{3}{2}\,\frac{1}{x^{\frac{5}{2}}}+\frac{a^2}{x^{\frac{3}{2}}}\right)$$

$$=\mathrm{A}\,x\,e^{a^2x}-\frac{b}{\sqrt{x}}$$

By the second and third methods,

$$\frac{1}{x}\,(d^{\frac{1}{2}}-a)\,\mathrm{X}\,x=b\left(\frac{\sqrt{-1}}{\sqrt{\pi}}\,\frac{1}{x^2}-\frac{a}{x^{\frac{3}{2}}}-\frac{a^2\sqrt{-1}}{\sqrt{\pi}\,x}\right)$$

Also

$$d^{-\frac{1}{2}}\,(d^{\frac{1}{2}}-a)^{-2}=\frac{d^{\frac{1}{2}}+2\,a+d^{-\frac{1}{2}}}{(d-a^2)^2}$$

$$\therefore\quad y=(d-a^2)^{-2}\,(d^{\frac{1}{2}}+2\,a+d^{-\frac{1}{2}})\,b\left(\frac{\sqrt{-1}}{\sqrt{\pi}}\,\frac{1}{x^2}-\frac{a}{x^{\frac{3}{2}}}-\frac{a^2\sqrt{-1}}{\sqrt{\pi}\,x}\right)$$

$$=(d-a^2)^{-2}\,b\left(-\frac{3}{4\,x^{\frac{5}{2}}}-\frac{a^2}{x^{\frac{3}{2}}}-\frac{a^4}{x^{\frac{1}{2}}}\right)$$

an ordinary linear differential equation, of which the solution is

$$y=\mathrm{A}\,x\,e^{a^2x}+\mathrm{B}\,e^{a^2x}-\frac{b}{\sqrt{x}}.$$

This agrees with the former solution by making $\mathrm{B}=0$.

III.—*On the Constitution of Codeine and its Products of Decomposition.* By Thomas Anderson, M.D.

(Read 15th April 1850.)

During the last few years, great progress has been made in the study of the organic alkalies, and the discovery of methods by which these substances can be artificially produced, and the long train of investigations by which it has been followed, has greatly extended our previous information, and afforded us some definite ideas regarding their constitution. The advance made has, however, related entirely to the volatile bases produced by artificial processes, and our knowledge of the natural fixed alkaloids stands very much where it did some years since, and is still very imperfect, and in regard to many entirely fragmentary; so much so, indeed, that of all the alkaloids of this class described in chemical works, there are not perhaps a dozen of which the constitution can be considered as definitely fixed, and not half that number of which we know the products of decomposition. The fact is, that the interest attaching to the artificial bases has altogether diverted the attention of chemists from the natural alkalies, which have not hitherto proved a very productive field of inquiry; at least the researches to which many of them were subjected ten or fifteen years since, proved comparatively unfruitful in their results. The want of success which attended their investigation at that time, however, is attributable, partly to the imperfections of the method of analysis of such compounds, and partly to our entire ignorance of the constitution of the nitrogenous substances generally. Neither of these difficulties can now be said to exist; and the investigation of the volatile bases has so far elucidated the constitution of these substances generally, that we are now in the condition to return to the examination of the far more complex natural bases with some prospect of ultimate success. Chemists are, accordingly, beginning to turn their attention to this field of inquiry, and during the last few months, several investigations have been published, by which the constitution and products of decomposition of several important bases have been established; and in the present paper I propose communicating to the Royal Society the results of a series of investigations of codeine and its compounds, which has enabled me to add it to the number of those of which the constitution is definitely fixed.

It will be unnecessary for me to premise any observations regarding the history of codeine and its discovery, which are sufficiently well known, further than to refer to the analyses and formulæ given for it by the different chemists by

whom it has been examined. Codeine has been analysed by its discoverer, Robiquet, and by Couerbe, Regnault, Will, Gregory, and Gerhardt. All the analyses of these observers I have brought together in the following table, in which, however, the *per centage* results are not those found in the original papers, but have been calculated from the analytical numbers according to the new equivalent of carbon.*

Anhydrous Codeine.

	Robiquet.†	Couerbe.‡		Regnault.§		Gregory.‖	Will.¶
Carbon,	70·363	71·59	72·10	73·31	72·93	73·18	73·27
Hydrogen,	7·585	7·12	7·17	7·19	7·23	7·23	7·25
Nitrogen,	5·353	5·23	...	4·89	4·89	4·82	...
Oxygen,	16·699	16·06	...	14·61	14·95	14·77	...
	100·00	100·00		100·00	100·00	100·00	

Crystallised Codeine.

	Gerhardt.**	
Carbon,	67·77	67·87
Hydrogen,	7·59	7·33
Nitrogen,	...	...
Oxygen,	...	...

From these analyses, four different formulæ have been deduced. Two of these, however, those of Robiquet and Couerbe, do not require particular mention, as they were unsupported by any accurate determination of the atomic weight of the substance, and are now certainly known not to represent its true constitution. That which has been hitherto most generally adopted by chemists is the one founded by Regnault upon his analysis, and represents codeine as $C_{35}\,H_{20}\,NO_5$, and the crystallised base as $C_{35}\,H_{20}\,NO_5 + 2\,HO$; the calculation of which gives

	Anhydrous.	Crystallised.
Carbon,	73·94	69·53
Hydrogen,	7·04	7·28
Nitrogen,	4·92	4·63
Oxygen,	14·10	18·50
	100·00	100·00

The analyses of Will and Gregory have usually been quoted in confirmation of this formula. It is clear, however, that the agreement between the calculated and experimental results is by no means satisfactory, either in them or in

* In the case of Robiquet and Will's analyses, the details of the experiment are not given. I have, therefore, been obliged to convert the per centage of carbon into carbonic acid, according to the old equivalent of carbon, and recalculate it into carbon according to the new equivalent.

† Annales de Chimie et de Physique, vol. li., p 265.

‡ Ibid., vol. lix., p. 158. § Ibid., vol. lxviii., p. 136.

‖ Annalen der Chimie und Pharmacie, vol. xxvi., p. 44. ¶ Ibid.

** Revue Scientifique, vol. x., p. 203.

REGNAULT'S own results; the highest amount obtained for the carbon being 0·63 per cent. below the calculation, while the lowest differs by more than one per cent., and the mean of the whole four gives 0·77 too little carbon, involving a loss which could not possibly have occurred in carefully made analyses.

Partly on account of this difference, and partly guided by his views regarding the divisibility of formulæ, GERHARDT was induced to doubt the exactitude of REGNAULT'S formula, which presents three different deviations from his law; the number of equivalents of carbon and of oxygen being uneven, and the sum of the equivalents of hydrogen and nitrogen also indivisible by two. He therefore repeated its analysis, using the crystallised codeine, and obtained the results contained in the table, and deduced from them the formula $C_{36} H_{21} NO_6$ for the anhydrous base, which gives the calculated results:

	Anhydrous.	Crystallised.
Carbon, . . .	72·24	68·13
Hydrogen, . .	7·02	7·25
Nitrogen, . . .	4·68	4·41
Oxygen, . . .	16·06	20·11
	100·00	100·00

and tallies extremely well with his analysis. This formula has, however, been again called in question by DOLLFUS,* who has endeavoured to determine the constitution and atomic weight of the alkaloids by the analysis of their hydrosulphocyanates, and obtained from the codeine salt of that acid; results agreeing with the formula $C_{34} H_{19} NO_5$. Considering the known accuracy of REGNAULT, and of the chemists by whom his formula has been confirmed, I considered it an essential preliminary to my investigation to repeat its analysis with all possible care, so as to determine which of the two represents its true constitution.

I. *Preparation and Analysis of Codeine.*

I have little to add to the information we already possess regarding the preparation of codeine. I have obtained it, as usual, from the mother liquor from which morphia has been precipitated by ammonia. As the codeine forms only from a sixteenth to a thirtieth of the morphia, it is, of course, mixed in this fluid with a corresponding quantity of muriate of ammonia, which must be decomposed by potash, in order to obtain it. Much advantage is gained, however, by first evaporating the fluid to crystallisation, and expressing the crystals deposited, as in this way the greater part of the muriate of ammonia, which is the more soluble salt of the two, is left in solution; and by repeating the crystallisation many

* Annalen der Chimie und Pharmacie, vol. lxv., p. 218.

times, it may be entirely removed, and crystals obtained which are pure hydrochlorate of codeine. For the preparation of codeine, however, it would be worse than useless to carry the process thus far, as the solubility of hydrochlorates of codeine and ammonia differs so little that much of the former salt would be lost; but by carrying it a certain length, the greater part of the sal-ammoniac may be separated without any material loss of codeine, and the subsequent steps of the process much facilitated. The crystals so obtained being dissolved in boiling water, strong solution of caustic potash is added in excess, when codeine is in part precipitated as an oil, which by-and-by concretes into a solid mass, and is partly deposited in crystals as the solution cools. By evaporating the fluid, another crop of crystals is obtained; and, finally, when the mother-liquor has been concentrated to a very small bulk, it becomes filled on cooling with long silky needles of morphia, which has been retained in solution by the excess of potash. A certain quantity of morphia appears always to remain in solution along with the codeine; at least I have found it in all the mother-liquors I have examined, although its quantity appears to vary considerably. Its presence in this solution has been observed before, and it has been stated that it exists in the form of a double salt with codeine; this, however, is not consistent with my own experience, at least the salt separated from the muriate of ammonia by successive crystallisations contained no morphia, but, as has been already stated, was pure hydrochlorate of codeine.

The crystals of codeine precipitated by potash, in the manner described, are always more or less coloured. They are purified by solution in hydrochloric acid, boiling with animal charcoal, and reprecipitation with a slight excess of potash, and the precipitate obtained finally dissolved in ether, to separate any morphia which may adhere to it. For this purpose hydrous ether is best adapted; and it ought to be free from alcohol, as if any be present, the ether evaporates, and a syrupy fluid is left behind, which refuses to crystallise. When the ether is anhydrous, it dissolves codeine with much greater difficulty, and by evaporation small crystals are deposited, which are anhydrous.

The codeine employed for analysis was dried at 212°. The three first were made with codeine crystallised from hydrous ether, which lost two equivalents of water at 212°; the last was anhydrous codeine in small colourless crystals.

I. { 6·120 grains of codeine, with oxide of copper, gave
16·135 ... of carbonic acid, and
3·888 ... of water.

II. { 5·896 grains of codeine, with oxide of copper, gave
15·616 ... of carbonic acid, and
3·737 ... of water.

III. { 4·688 grains of codeine, with chromate of lead, gave
12·392 ... of carbonic acid, and
3·015 ... of water.

IV. { 5·858 grains of codeine, with chromate of lead, gave
15·485 ... of carbonic acid, and
3·780 ... of water.

5·395 grains of codeine gave, by VARRENTRAP and WILL'S method, 3·79 grains of ammonio-chloride of platinum.

5·898 grains gave, by the same method, 4·32 grains of ammonio-chloride of platinum.

	I.	II.	III.	IV.
Carbon, . . .	71·91	72·02	72·09	72·09
Hydrogen, . .	7·05	7·04	7·14	7·16
Nitrogen, . .	4·41	4·60	4·50	...
Oxygen, . .	16·63	16·34	16·27	...
	100·00	100·00	100·00	

These results confirm, in all respects, the formula $C_{36} H_{21} NO_9$, the calculated results of which are given on a former page. The determination of the atomic weight of codeine by the analysis of its platinum salt, presented considerable difficulties, and at first gave extremely discordant results, the per centage of platinum varying from 18·51 to 20·30. I found, however, that by precipitating in the cold, a salt was obtained, to be afterwards described, which gave sufficiently uniform results. This salt, dried at 212°, retained an equivalent of water. It gave, as the mean of seven experiments, the details of which will be afterwards given, 19·25 per cent. of platinum, while the calculation, according to the above formula, requires 19·19 per cent. These determinations leave no doubt as to the formula of codeine; and they are fully confirmed by the result of the analyses of the substances to be described in the sequel of this paper.

Codeine crystallised from water or hydrous ether is obtained in crystals, often of considerable size, belonging to the right-prismatic system, and presenting a considerable number of modifications. These crystals contain two equivalents of water of crystallisation, as determined by this experiment:—

7·126 grains crystallised codeine lost, at 212°, 0·454=5·66 per cent. water.

The calculated result gives 5·67.

Codeine is an extremely powerful base, rapidly restoring the blue of reddened litmus, and precipitating oxides of lead, copper, iron, cobalt, nickel, and other metals, from their solutions. It is precipitated by potash from its salts; and is generally stated to be insoluble in that alkali, but this is true only of very highly concentrated solutions, as a considerable quantity of strong potash may be added to a saturated solution of codeine in water without producing precipitation; and even when a very large amount of potash is added, a certain quantity of the base is still retained in solution. Codeine is soluble in ammonia, but not more so than in water. 100 parts of a moderately strong solution of ammonia dissolved, at 60°, 1·46 parts of codeine; and according to ROBIQUET, 100 parts of water, at 59°, dissolve 1·26 parts. Contrary to what is usually stated, I have found that codeine

is precipitated from all its salts by ammonia; it does not, however, fall immediately, but is slowly deposited in small transparent crystals.

II. *Salts of Codeine.*

Hydrochlorate of Codeine.—This salt is readily obtained by saturating hot dilute hydrochloric acid with pure codeine. If the solution has been sufficiently concentrated, it becomes nearly solid on cooling, but if more dilute, the salt is deposited in radiated groups of short needles, which, under the microscope, are found to be four-sided prisms terminated by dihedral summits. It is never obtained in large crystals, even when considerable quantities are crystallised. These crystals are soluble in 20 times their weight of water at 60°, and in less than their weight of water at 212°. Codeine is precipitated from the saturated cold solution immediately by potash; ammonia gives no precipitate, but after some time colourless crystals are deposited. The crystallised hydrochlorate of codeine contains water of crystallisation, and presents some curious anomalies in its relations to that fluid. When dried in the air, it retains four equivalents of water, one of which escapes at 212°, but the remaining three are only expelled at 250°, and at the same time the salt loses acid, and acquires an alkaline reaction. It would appear, also, that under certain circumstances, the salt is deposited in anhydrous crystals, as one analysis of it dried at 212°, gave numbers corresponding to the anhydrous salt. I could not, however, again succeed in obtaining it in this condition; but many analyses were made which gave results lying between those of the anhydrous and crystallised salts, and the only means of explaining the discrepancy is by supposing that the two sorts of crystals had been deposited simultaneously and in variable proportions. The following is the analysis of the salt dried at 212°:—

6·035 grains hydrochlorate of codeine gave
13·208 ... of carbonic acid,
3·830 ... of water.

	Experiment.	Calculation.		
Carbon, . .	59·68	59·58	C_{36}	216
Hydrogen, . .	7·08	6.89	H_{25}	25
Nitrogen, . .	...	3·86	N	14
Oxygen, . .	...	19·88	O_9	72
Chlorine, . .	...	9·79	Cl	35·5
		100·00		362·5

10·735 grains of the salt lost, at 212°, 0·31 grains of water=2·88 per cent. One equivalent of water gives by calculation 2·42 per cent. The formula of the air-dried salt is therefore $C_{36}H_{21}NO_6 H Cl + 4 HO$.

The anhydrous salt gave the following results. Of these, No. I. is the salt obtained by direct crystallisation from the morphia mother-liquor; No. II. is that

which was got anhydrous at 212°; and No. III. is a portion dried at 250°; it had become strongly alkaline, which accounts for the excess of carbon obtained.

I. { 6·171 grains dried at 250° gave
14·565 ... carbonic acid, and
3·795 ... water.

II. { 4·286 grains dried at 212° gave
10·014 ... carbonic acid, and
2·603 ... water.

III. { 5·740 grains dried at 250° gave
13·667 ... carbonic acid, and
3·467 ... water.

	Experiment.			Calculation.		
	I.	II.	III.			
Carbon,	64·37	64·56	64·93	64·38	C_{36}	216
Hydrogen,	6·83	6·74	6·71	6·55	H_{22}	22
Nitrogen,	...	...	...	4·17	N	14
Oxygen,	...	...	...	14·32	O_6	48
Chlorine,	...	...	...	10·58	Cl	35·5
				100·00		335·5

These results correspond to the formula $C_{36}H_{21}NO_6 H Cl$.

Hydriodate of Codeine is obtained by dissolving codeine in hot hydriodic acid, and allowing the solution to cool. It is deposited in long slender needles, which fill the whole fluid, if it have been sufficiently concentrated. It is of rather sparing solubility in cold water, requiring about 60 times its weight, but is much more soluble in boiling water. Its saturated cold solution is precipitated by ammonia on standing. No difficulty was experienced in its analysis.

I. { 6·336 grains hydriodate, dried at 212°, gave
11·190 ... of carbonic acid, and
3·247 ... of water.

II. { 5·801 grains dried at 212°, gave
10·347 ... of carbonic acid, and
2·977 ... of water.

5·733 grains of hydriodate of codeine gave 2·994 grains of iodide of silver.

	Experiment.		Calculation.		
	I.	II.			
Carbon, . .	48·16	48·64	48·60	C_{36}	216
Hydrogen, . .	5·69	5·70	5·40	H_{24}	24
Nitrogen, .	...	...	3·15	N	14
Oxygen, . .	...	...	14·45	O_8	64
Iodine, . .	...	28·22	28·40	I	126·36
			100·00		444·36

The formula of the salt, dried at 212°, is, therefore, $C_{36}H_{21}NO_6 HI + 2 HO$.

Sulphate of Codeine.—Crystallises in radiated groups of long needles, or by spontaneous evaporation in flattened four-sided prisms. It requires for solution 30 times its weight of cold water, but it is very soluble in the heat. When pure, it is neutral to test paper, but it is very liable to retain a small quantity of acid, which can be got rid of by repeated crystallisations. The first analysis was made with the salt which had been only once crystallised, and has therefore given an excess of sulphuric acid.

Analysis of the salt, dried at 212°, gave the following results:—

I. { 5·564 grains sulphate of codeine gave
12·536 ... of carbonic acid, and
3·270 ... of water.

II. { 5·677 grains of sulphate of codeine gave
12·831 ... of carbonic acid,
3·324 ... of water.

I. { 9·540 grains of sulphate of codeine gave
3·265 ... of sulphate of baryta.

II. { 10·826 grains of sulphate of codeine gave
3·650 ... of sulphate of baryta.

	Experiment.		Calculation.		
	I.	II.			
Carbon, . . .	61·44	61·64	62·07	C_{36}	216
Hydrogen, . . .	6·52	6·50	6·39	H_{22}	22
Nitrogen, . . .	...	...	4·03	N	14
Oxygen, . . .	...	...	16·03	O_7	48
Sulphuric Acid, . .	11·75	11·54	11·49	SO_3	40
			100·00		348

27·13 grains of the crystallised salt lost, at 212°, 3·068 grains of water = 11·30 per cent. This corresponds to 5 equivalents of water, the calculated result for which is 11·45.

The formula of the crystallised salt is therefore

$$C_{36}H_{21}NO_6\,HO\,SO_3 + 5\,HO.$$

Nitrate of Codeine.—Is obtained by slowly adding nitric acid, of specific gravity 1·060, to powdered codeine, an excess of nitric acid being carefully avoided, as the base is rapidly decomposed by it with the formation of a product of substitution to be afterwards described. The nitrate is readily soluble in boiling water, and is deposited on cooling in small prismatic crystals. Heated on platinum, it melts, and on cooling, concretes into a brown resinous mass; at a higher temperature it is rapidly decomposed, leaving a bulky coal, difficult of incineration.

{ 6·360 grains of nitrate of codeine, dried at 212°, gave
13·854 ... of carbonic acid,
3·746 ... of water.

These results correspond with the formula $C_{36}H_{21}NO_6\,HO\,NO_5$.

	Experiment.	Calculation.		
Carbon, ...	59·40	59·66	C_{36}	216
Hydrogen, . . .	6·54	6·07	H_{22}	22
Nitrogen, . . .	...	7·73	N_2	28
Oxygen, . . .	...	26·54	O_{12}	96
		100·00		362

Phosphate of Codeine.—Several phosphates of codeine appear to exist, but I have only examined that which is obtained by saturating tribasic phosphoric acid with codeine in powder. In this way a fluid is obtained, which, when concentrated to a small bulk, refuses to crystallise, but from which crystals are immediately precipitated by the addition of strong spirit. The salt is thus obtained in small scales, or in short thick prisms. It is readily soluble in water, and cannot be obtained in crystals from the solution. Its analysis gave the following results, corresponding with the formula $C_{36}H_{21}NO_6HO\,2HO\,PO_5$.

6·343 grains phosphate of codeine, dried at 212°, gave
12·618 ... of carbonic acid,
3·708 ... of water.

	Experiment.	Calculation.		
Carbon, ...	54·25	54·27	C_{36}	216
Hydrogen, . . .	6·49	6·03	H_{24}	24
Nitrogen, . . .	...	3·52	N	14
Oxygen, . . .	...	18·09	O_9	72
Phosphoric acid, . .	...	18·09	PO_5	72
		100·00		398

6·911 grains of the crystallised salt lost, at 212°, 0·434 grains of water = 6·27 per cent. Three equivalents of water correspond to 6·35 per cent.; and the formula of the crystallised salt is, consequently, $C_{36}H_{21}NO_6HO\,2HO\,PO + 3\,HO$.

Oxalate of Codeine.—This salt is deposited, on cooling its saturated hot solution, in short prisms, and sometimes in scales. It requires 30 times its weight of water at 60° for solution, and about half its weight at 212°. Heated to 212° it loses water of crystallisation; at 250° it becomes brown, and at a higher temperature it is entirely decomposed.

6·073 grains oxalate of codeine, dried at 212°, gave
14·739 ... of carbonic acid,
3·608 ... of water.

	Experiment.	Calculation.		
Carbon, ...	66·19	66·28	C_{38}	228
Hydrogen, . . .	6·60	6·39	H_{22}	22
Nitrogen, . . .	...	4·07	N	14
Oxygen, . . .	...	23·26	O_{10}	80
		100·00		344

10·050 grains of the crystallised oxalate lost, at 212°, 0·704 grains of water = 7·00 per cent., corresponding to three equivalents of water, which requires 7·27 per cent. The formula of the crystallised salt is, therefore, $C_{36}\,H_{21}\,NO_6\,HO\,C_2\,O_3 + 3\,HO$.

Hydrosulphocyanate of Codeine.—This salt has been already examined by Dollfus;* but I have prepared it, and repeated the analysis, with results differing somewhat from those obtained by him. It is readily obtained by mixing solutions of hydrochlorate of codeine and of sulphocyanide of potassium, and is slowly deposited in beautiful radiated needles.

6·164 grains of hydrosulphocyanate, dried at 212°, gave
14·285 ... carbonic acid, and
3·543 ... water.

7·444 grains, burnt with nitre and carbonate of soda, gave 4·899 grains of sulphate of baryta.

These results correspond with the formula $C_{36}\,H_{21}\,NO_6\,H\,C_2N\,S_2$, as is shewn by the following per centage calculation, to which I have added the results obtained by Dollfus:—

	Experiment.		Calculation.		
	Dollfus.				
Carbon, . . .	62·30	63·20	63·68	C_{38}	228
Hydrogen, . . .	6·13	6·38	6·14	H_{22}	22
Nitrogen, . . .	...	...	7·82	N_2	28
Oxygen, . . .	...	...	13·43	O_6	48
Sulphur, . . .	...	9·04	8·93	S_2	32
			100·00		358

11·613 grains of the crystallised salt, dried at 212°, lost 0·288 grains of water = 2·47 per cent., corresponding to one equivalent of water, the calculation of which gives 2·45 per cent.

In the analysis of Dollfus, there is manifestly a loss of carbon, as the results are quite incompatible with those of the base and its other salts. In the same paper Dollfus has also determined the amount of sulphocyanogen by precipitation with silver, and the results obtained agree better with the formula given above than with his own.

Chloride of Platinum and Codeine.—When bichloride of platinum is added to a moderately concentrated solution of hydrochlorate of codeine, a pale-yellow, pulverulent precipitate is deposited. If this be allowed to stand for some time in the solution, or still better, if it be collected on a filter and kept moist, it begins to change in its appearance; specks of darker colour appear in it, and it is gradually converted into a mass of crystalline grains of an orange-yellow colour. The fluid which filters off deposits, on standing, a small quantity of larger grains.

* Annalen der Chimie und Pharmacie, vol. lxv., p. 218.

The change which takes place in this manner is not always complete, and the granular crystals are often mixed with unchanged yellow powder. When the chloride of platinum is added to a more dilute solution of hydrochlorate of codeine, precipitation does not take place immediately, but in a short time the salt is deposited in minute tufts of silky needles. The salt is soluble in boiling water, and is deposited on cooling partly in grains, partly as a powder. By this process, however, it is partially changed; and I have ascertained that by ebullition, with excess of chloride of platinum, it is completely decomposed. I have not as yet, however, followed up this observation. I at first attempted to purify the salt by solution in water and alcohol, in which it is also soluble; and a number of analyses were made, which gave extremely contradictory results; but by precipitation in the cold, and without excess of platinum, sufficiently uniform results were obtained.

When dried at 212°, the salt retains an equivalent of water, which is expelled at 250°, but not without occasioning partial decomposition of the substance, which evolves acid, and acquires a brownish colour. The following are the results of analysis:—

I. 7·240 grains of platinum salt, dried at 212°, gave
11·072 ... of carbonic acid, and
2·925 ... water.

II. 9·394 grains of platinum salt gave
14·593 ... of carbonic acid, and
3·912 ... of water.

III. 7·648 grains of platinum salt gave
11·694 ... of carbonic acid, and
3·450 ... of water.

IV. 6·665 grains of platinum salt gave
10·230 ... of carbonic acid, and
2·835 ... of water.

V. 7·383 grains of platinum salt gave
11·372 ... of carbonic acid, and
3·304 ... of water.

7·247	grains platinum salt gave	1·400	grains platinum,	=19·31 per cent.
10·030	... ...	1·920	...	=19·14 ...
9·775	... ...	1·850	...	=18·92 ...
10·471	... ...	2·020	...	=19·32 ...
8·428	... ...	1·600	...	=18·98 ...
6·790	... ...	1·296	...	=19·08 ...
5·052	... ...	0·960	...	=19·00 ...

	I.	II.	III.	IV.	V.	VI.	VII.
Carbon, .	41·70	42·36	41·70	41·80	42·00	...	...
Hydrogen, .	4·49	4·62	5·01	4·72	4·97	...	...
Nitrogen, .	...	...	...	...	...	...	...
Oxygen, .	...	...	...	...	...	...	...
Chlorine, .	...	...	...	...	...	...	...
Platinum, .	19·31	19·14	18·92	19·32	18·98	19·08	19·00

These analyses correspond with the formula

$$C_{36}\,H_{21}\,NO_6\,HCl + Pt\,Cl_2 + HO.$$

of which the following is the calculated result compared with the mean of experiment:—

	Mean.	Calculation.		
Carbon, . . .	41·91	42·07	C_{36}	216·
Hydrogen, . . .	4·76	4·47	H_{23}	23·
Nitrogen, . . .	...	2·72	N	14·
Oxygen, . . .	...	10·94	O_7	56·
Chlorine, . . .	...	20·61	Cl_3	106·5
Platinum, . . .	19·25	19·19	Pt	98·7
		100·00		514·2

The air-dried salt gave the following results, when dried at 212° :—

14·845 grains lost 0·770 grains of water, =5·11 per cent.
14·546 ... 0·758 ... =5·20 ...

This corresponds to three equivalents of water, the calculated result for which gives 4·99 per cent. The crystallised salt is therefore represented by the formula

$$C_{36}\,H_{21}\,NO_6\,HCl + Pt\,Cl_2 + 4\,HO.$$

Codeine forms many other crystallisable salts, none of which, however, have been examined. The chromate is easily obtained in fine yellow needles. With solution of bichloride of mercury, codeine gives a white precipitate, soluble in boiling water and alcohol, and deposited on cooling in stellated groups of crystals. With chloride of palladium a yellow precipitate is obtained, which is decomposed by boiling, with separation of metallic palladium. Tartrate and hydrocyanate of codeine are uncrystallisable.

Products of Decomposition of Codeine.

III. *Action of Sulphuric Acid.*

Amorphous Codeine.—When codeine is dissolved in an excess of moderately-concentrated sulphuric acid, and the mixture digested on the sand-bath, the fluid gradually acquires a dark colour, and after some time gives a precipitate with carbonate of soda, which the salts of codeine are incapable of doing. The precipitate so obtained is codeine in a modified or amorphous condition, similar to that in which quinine is obtained by a similar treatment with excess of acid. By carefully regulating the temperature of the mixture of codeine and sulphuric acid, the amorphous codeine may be obtained in a state of purity; but it is neither so definite nor so stable a substance as quinoidine. After the action has been prolonged for some time, carbonate of soda is added to the fluid, and the gray precipitate obtained, collected on a filter, washed with water, dissolved in alcohol, and precipitated from the solution by means of water. As thus obtained, it is a gray powder, with a more or less green shade, insoluble in water, readily soluble in

alcohol, and precipitated by ether from the solution. It fuses at 212° into a black resinous mass. In acids it is readily soluble, with the formation of salts which are amorphous, and dry up by evaporation into brown resins.

Analysis gave the following results:—

I. { 5·400 grains amorphous codeine gave
14·240 ... of carbonic acid, and
3·663 ... of water.

II. { 4·532 grains amorphous codeine gave
12·054 ... of carbonic acid, and
2·781 ... of water

	Experiment.		Calculation.
	I.	II.	
Carbon, ...	71·92	72·53	72·24
Hydrogen, ...	7·53	6·84	7·02
Nitrogen, ...	...	...	6·68
Oxygen, ...	...	...	16·06
			100·00

These results correspond sufficiently closely with those of codeine to shew that this substance is represented by the same formula. At the same time it is to be observed, that the action does not stop at the point at which amorphous codeine is formed; for the excess of carbon and deficiency of hydrogen in the second analysis (which occurred also in another analysis from a different preparation), appear to me to shew that some farther change had taken place. Indeed, by continuing the action of sulphuric acid, a deep-green powder was obtained, which contained sulphur, and agreed in its general properties with the sulphomorphide described by Arppe, and the corresponding sulphonarcotide of Laurent and Gerhardt.

IV. *Action of Nitric Acid.*

Nitrocodeine.—When strong nitric acid is poured upon codeine, and heat applied, violent action takes place, nitrous fumes are abundantly evolved, and the solution acquires a red colour. If the fluid be evaporated on the water-bath, a yellow resinous acid is left, which dissolves in ammonia and potash solutions, with a red colour.* If the nitric acid be employed in a sufficiently dilute state, a different result is obtained, and a nitrobase is formed, to which I give the name of nitrocodeine.

The preparation of this substance is a matter of some nicety, as by the continued action even of very dilute nitric acid it is rapidly destroyed. The operation succeeds best when the acid employed is of a specific gravity of 1·060. Acid of this density is heated in a flask, but not to ebullition, and finely-powdered codeine is added, and a moderate heat is sustained. In the course of a few minutes a small quantity of the fluid is poured out into a glass, and an excess of ammonia

* The constitution and properties of this substance will be detailed in a future communication.

added; if no precipitate appears, the heat is kept up for a short time longer, and another quantity is then taken out and tested; and this is repeated until the precipitate, which makes its appearance when the acid is neutralised, ceases to increase. The fluid is then immediately saturated with ammonia, and stirred rapidly, when it becomes filled with a bulky precipitate of nitrocodeine. The action which takes place is extremely rapid, and the whole operation is complete in a few minutes; so that the experimenter requires to be carefully on the watch, in order to hit the right moment for precipitating the fluid. No red fumes are evolved; if they are seen, it is a sure sign that the action has gone too far, and that part of the codeine has been converted into the resinous acid already mentioned. On this account it is better to stop the action before the whole of the codeine is decomposed, the quantity left being easily recovered from the solution; but even with the greatest possible care, the formation of a small quantity of the resinous acid cannot be avoided, and its presence is always indicated by the dark colour which the fluid acquires when saturated by ammonia.

On the addition of ammonia, the nitrocodeine falls in the form of minute silvery plates, with a very slight shade of yellow. It is purified by solution in hydrochloric acid, boiling with animal charcoal, and a reprecipitation with ammonia, in order to separate colouring matter and any unchanged codeine which may have been precipitated along with the first crystals. The nitrocodeine is then crystallised by dissolving in dilute alcohol, or a mixture of alcohol and ether.

Nitrocodeine crystallised from alcohol is deposited in the form of slender silky needles of a pale fawn-colour, which, on drying, mat together into a silky mass. From alcohol and ether it is obtained by spontaneous evaporation in small yellowish crystals, which, under the microscope, are seen to be four-sided prisms, terminated by dihedral summits. Nitrocodeine is sparingly soluble in boiling water, from which it is deposited in minute crystals on cooling. It dissolves abundantly in boiling alcohol, and but sparingly in ether. It is soluble in acids, with the formation of salts which are neutral to test-paper, and from which potash and ammonia precipitate the base as a crystalline powder. When heated carefully, it melts into a yellow fluid, which concretes on cooling into a highly-crystalline mass. At a higher temperature, it suddenly decomposes without flame, leaving a bulky charcoal.

Its analysis yielded the following results, of which No. 1 is from the base crystallised from the first precipitate by ammonia, before I had observed its tendency to carry down codeine with it, and which has therefore given a slight excess; the others are from the pure base. Crystallised nitrocodeine is anhydrous.

I. { 5·748 grains of nitrocodeine, dried at 212°, gave
13·301 ... of carbonic acid, and
3·128 ... of water.

II. { 5·523 ... of nitrocodeine gave
12·724 ... of carbonic acid, and
2·887 ... water.

III. { 4·463 grains of nitrocodeine gave
10·226 ... of carbonic acid, and
2·377 ... of water.

	Experiment.			Calculation.		
	I.	II.	III.			
Carbon,	63·10	62·83	62·49	62·79	C_{36}	216
Hydrogen,	6·04	5·80	5·91	5·81	H_{20}	20
Nitrogen,	...	...	...	8·11	N_2	28
Oxygen,	...	...	...	23·29	O_{10}	80
				100·00		344

These results correspond with the formula $C_{36}H_{20}(NO_4)NO_6$, derived from that of codeine by the substitution of NO_4 in place of an equivalent of hydrogen. It is confirmed by the analysis of its platinum salt, which was found to contain 17·88 per cent. of platinum, giving for the atomic weight of the base 345·8: the calculated atomic weight is 344.

Hydrochlorate of Nitrocodeine.—Nitrocodeine dissolves readily in hydrochloric acid, and the solution on evaporation leaves the hydrochlorate in the form of a resinous mass, which cannot be made to crystallise.

Sulphate of Nitrocodeine is obtained in a radiated group of short-pointed needles, which are neutral to test-paper, and very soluble in boiling water.

4·687 grains of the sulphate, dried at 212°, gave 1·383 sulphate of baryta.

This corresponds to the formula $C_{36}H_{20}(NO_4)NO_6HOSO_3$, which requires

	Experiment.	Calculation.		
Nitrocodeine,	...	...	...	344
Water,	...	...	...	9
Sulphuric acid,	10·13	10·17	...	40
				393

Oxalate of Nitrocodeine.—Crystallises in beautiful yellow short prisms, readily soluble in water.

Platinochloride of Nitrocodeine.—This salt is precipitated from the solution of the hydrochlorate as a yellow powder, insoluble in water and alcohol. Its analysis gave the following results:—

{ 8·113 grains of platinochloride of nitrocodeine, dried at 212°, gave
11·635 ... of carbonic acid, and
2.987 ... of water.

9·392 grains, dried at 212°, gave 1·68 grains platinum.

	Experiment.	Calculation.		
Carbon,	39·11	39·25	C_{36}	215
Hydrogen,	4·09	3·81	H_{21}	21
Nitrogen,	...	5·08	N_2	28
Oxygen,	...	14·58	O_{10}	80
Chlorine,	...	19·35	Cl_3	106·5
Platinum,	17·88	17·93	Pt	98·7
		100·00		550·2

8·670 grains of the precipitated salt, dried by long exposure to the air, lost, at 212°, 0·569 grains of water = 6·56 per cent.

Four equivalents of water require 6·14 per cent. The formula of the salt is, therefore, $C_{36}H_{20}(NO_4)NO_6HCl+PtCl_2+4HO$.

When nitrocodeine dissolved in alcohol is treated with hydrosulphuret of ammonia in the water-bath, the solution gradually acquires a dark colour, and sulphur is deposited. When the action is complete, the filtered fluid gives with ammonia a brown amorphous precipitate, which, when dissolved in hydrochloric acid, and boiled with animal charcoal, gives, on precipitation, a pale-yellow base. The substance so obtained is very different from nitrocodeine; it is extremely soluble in alcohol, and is deposited from it as an amorphous powder. Once only did I obtain definite crystals, which were brownish rhomboids, but in too small quantity to admit of examination. The amorphous base did not give satisfactory results; and as its preparation is extremely troublesome, I did not pursue its investigation further. Arguing from what we know of the other bases formed by the same process, its constitution ought to be $C_{36}H_{22}N_2O_6$, and it might be called azocodeine.

V. *Action of Bromine on Codeine.*

Bromocodeine.—In order to obtain this substance, bromine-water is added in small successive portions to finely-powdered codeine. The base is rapidly dissolved, and the solution loses its colour of bromine, but acquires a peculiar and characteristic red shade. After a certain quantity of bromine has been added, small crystals make their appearance, which are hydrobromate of bromocodeine; but these are only observed if the bromine-water has been thoroughly saturated, and are deposited in small quantity only, the remainder being retained in solution. When the whole of the codeine has been got into solution, ammonia is added, and bromocodeine is immediately thrown down as a silvery-white powder. In this state it contains a small quantity of unchanged codeine. It is collected on a filter; washed several times with cold water, and redissolved in hydrochloric acid, from which it is reprecipitated by ammonia, and finally crystallised from boiling spirit. Bromocodeine is scarcely soluble in cold water; but by boiling, a somewhat larger quantity is taken up, and deposited again on cooling in minute prisms, terminated by dihedral summits. It is readily soluble in alcohol, particularly on boiling, and is best crystallised from spirit diluted with its bulk of water. The crystals in which it is deposited are always very small, but brilliantly white. It is scarcely soluble in ether. Exposed to heat, it melts into a colourless fluid, which is destroyed at a temperature slightly above its melting point. It dissolves in cold sulphuric acid, and the solution when heated becomes dark coloured. It is attacked by nitric acid, but much less rapidly than codeine itself.

Considerable difficulty was experienced in getting it absolutely free from codeine; and the first of the following analyses has given an excess in the carbon:—

I. { 6·119 grains bromocodeine, dried at 212°, gave
12·941 ... of carbonic acid, and
3·000 ... of water.

II. { 5·940 grains bromocodeine gave
12·461 ... of carbonic acid, and
2·910 ... of water.

5·268 grains gave 2·663 grains of bromide of silver.

	Experiment.		Calculation.		
	I.	II.			
Carbon, . . .	57·67	57·21	57·14	C_{36}	216
Hydrogen, . . .	5·44	5·44	5·29	H_{20}	20
Bromine, . . .	...	21·50	21·16	Br	80
Nitrogen, . . .	...	...	3·70	N	14
Oxygen, . . .	...	...	12·71	O_6	48
			100·00		378

The formula is therefore $C_{36}H_{20}BrNO_6$. Bromocodeine is capable of uniting with water in two different proportions, as appears by the determination of the loss by drying.

11·784	crystallised bromocodeine, lost at 212°		0·273,	=2·32 per cent.
9·308	...	...	0·623,	=6·69 ...
7·707	...	...	0·512,	=6·64 ...

The first of these results corresponds exactly to one equivalent of water, the calculated result for which gives 2·32 per cent. The other two give three equivalents, for which the calculation is 6·66. I am unable now to recollect how the bromocodeine used in the first experiment was obtained, but my impression is, that it was prepared in exactly the same manner as the rest.

Hydrochlorate of Bromocodeine is obtained in radiated needles closely resembling those of hydrochlorate of codeine.

Hydrobromate of Bromocodeine.—The crystals, which have been mentioned as making their appearance during the preparation of bromocodeine, are this salt. It is sparingly soluble in cold water, readily soluble in boiling water, and is deposited from the solution in small prismatic crystals. It contains two equivalents of water which are not expelled at 212°.

{ 8·424 grains of hydrobromate, dried at 212°, gave
13·956 ... of carbonic acid, and
3·985 ... of water.

	Experiment.	Calculation.		
Carbon, ...	45·18	45·28	C_{36}	216
Hydrogen, ...	5·25	4·84	H_{23}	23
Bromine, ...	...	33·54	Br_2	160
Nitrogen, ...	...	2·93	N	14
Oxygen, ...	...	13·41	O_8	64
		100·00		477

The formula of the salt is therefore $C_{36}\,H_{20}\,Br\,NO_6\,H\,Br + 2\,HO$.

Platinochloride of Bromocodeine is precipitated as a pale-yellow powder, insoluble in water and alcohol.

8·126 grains, dried at 212°, gave 1·380 grains platinum.

	Experiment.	Calculation.		
Carbon,	...	36·97	C_{36}	216
Hydrogen,	...	3·59	H_{20}	20
Bromine,	...	13·70	Br	80
Nitrogen,	...	2·39	N	14
Oxygen,	...	5·23	O_6	48
Chlorine,	...	18·23	Cl	106·5
Platinum,	16·98	16·89	Pt	98·7
		100·00		584·2

Tribromocodeine.—By continuing the addition of bromine water beyond the point at which bromocodeine is formed, a further action takes place, and a bright-yellow precipitate makes its appearance, which at first redissolves in the fluid, but after a time becomes permanent, and goes on gradually increasing until a very large quantity of bromine has been employed, when at length a point is reached at which no further precipitate is produced. If the solution be left till next day, however, bromine again causes a precipitate; and if it be added, as long as anything falls, and the solution be again left standing, another precipitate is produced identical in all respects with that before obtained, and this may be repeated day after day for a very considerable time. The yellow precipitate so obtained is the hydrobromate of tribromocodeine. It is collected on a filter, and washed with water, in which it is very sparingly soluble. In order to obtain the base, this substance is dissolved in dilute hydrochloric acid and ammonia added, when the tribromocodeine is immediately precipitated as a flocky powder, which is washed with water, and purified by solution in alcohol, and precipitation with water.

Tribromocodeine is thus obtained as a bulky white precipitate, perfectly amorphous, and when dry, more or less gray in its colour. It is insoluble in water and ether, but readily soluble in alcohol. It is sparingly soluble in hydrochloric acid in the cold, but much more so by boiling. In this process, however, it appears to undergo a partial decomposition, as a small quantity is always left insoluble. Heated on platinum foil it becomes brown, and is entirely decomposed at its melting point, leaving a coal difficult of incineration.

The tribromocodeine employed for analysis was purified by a second solution in alcohol, and precipitation by ether. It gave the following results:—

8·014 grains of tribromocodeine, dried at 212°, gave
11·665 ... of carbonic acid, and
2·645 ... of water.

3·55 grains of tribromocodeine gave 3·727 grains bromide of silver.

	Experiment.	Calculation.		
Carbon, ...	39·69	40·27	C_{36}	216
Hydrogen, ...	3·66	3·35	H_{18}	18
Bromine, ...	44·68	44·72	B_3	240
Nitrogen, ...	...	2·61	N	14
Oxygen, ...	...	9·00	O_6	48
		100·00		536

These results agree sufficiently well with the formula $C_{36}\,H_{18}\,Br_3\,O_6$ produced by the substitution of three equivalents of bromine; and this formula has been confirmed by the analysis of its platinum salt, which will be given below.

In such cases as have been hitherto examined, the substitution of three equivalents of bromine in a base, has entirely destroyed its basic properties, but tribromocodeine is still a base, though an extremely feeble one. Its salts are all sparingly soluble in water and amorphous; and as there is no possibility of ascertaining their purity, I have not pursued their investigation to any extent.

Hydrochlorate of Tribromocodeine.—It is obtained by dissolving the base in hot dilute hydrochloric acid, and is deposited on cooling as an amorphous powder.

Hydrobromate of Tribromocodeine.—This is the substance deposited during the preparation of tribromocodeine. It is a bright-yellow powder, perfectly amorphous, and very sparingly soluble in cold water. On boiling, however, a larger quantity is taken up, and deposited unchanged on cooling.

Its analysis gave the following results:—

I. { 7·501 grains hydrobromate, dried at 212°, gave
8·868 ... of carbonic acid, and
1·915 ... of water.

II. { 6·840 grains hydrobromate, from another preparation, gave
8·072 ... of carbonic acid, and
1·767 ... of water.

3·762 grains hydrobromate gave 4·865 grains bromide of silver.

	Experiment.		Calculation.		
	I.	II.			
Carbon, . .	32·24	32·18	32·84	C_{72}	432
Hydrogen, . .	2·83	2·86	2·96	H_{39}	39
Bromine, . .	...	55·03	54·75	Br_9	720
Nitrogen, . .	...	...	2·12	N_2	28
Oxygen, . .	...	...	7·33	O_{12}	96
			100·00		1315

These results approach most nearly to the formula:—

$$2\,(C_{36}\,H_{18}\,Br_3\,N\,O_6) + 3\,H\,Br.$$

They present, however, a certain deficiency, both in the carbon and hydrogen, and an excess in the bromine; but no other formula can be found at all approxi-

mating to the experimental numbers, and the recurrence of the results, in portions prepared at different times, leaves no doubt that this is their real constitution; and, in all probability, the error may be due to the salt retaining a small excess of hydrobromic acid. The constitution is therefore remarkable, and I am not aware of any similar salt having been before observed.

Platinochloride of Tribromocodeine.—Bichloride of platinum throws down from solution of tribromocodeine, in hydrochloric acid, this salt, in the form of a brownish-yellow powder soluble in water and alcohol.

5·142 grains of platinum salt, dried at 212°, gave 0·669 grains of platinum.

	Experiment.	Calculation.		
Carbon, . .	...	29·10	C_{36}	216
Hydrogen, . .	...	2·55	H_{19}	19
Bromine, . .	...	32·33	Br_3	240
Nitrogen, . .	...	1·88	N	14
Oxygen, . .	...	6·57	O_6	48
Chlorine, . . .	...	14·34	Cl_3	106·5
Platinum, .	13·07	13·29	Pt	98·7
		100·00		742·2

I have reason to believe that the action of bromine upon codeine does not terminate with the production of the base now described; but its further action did not appear to afford any products of a sufficient interest to induce me to prosecute the investigation in this direction. There must also no doubt exist a *dibromocodeine*, $C_{36}\ H_{19}\ Br_2\ N\ O_6$, but I did not meet with it in the course of my experiments, and have not made any special attempts to obtain it.

VI. *Action of Chlorine upon Codeine.*

We might anticipate that the action of chlorine upon codeine should be exactly similar to that of bromine; but this is not the case, as in place of a simple and definite action complex products are immediately obtained. When a current of chlorine is passed through an aqueous solution of codeine, the fluid immediately acquires a brown colour, which soon becomes very deep, and eventually almost black. From this solution ammonia throws down an amorphous, resinous base. With chlorine-water the solution also becomes rapidly brown, and a similar precipitate is obtained. As there was no method of determining in either of these cases when the action was complete, I did not attempt to examine the product. I succeeded better, however, by the action of chlorate of potash, and obtained a base corresponding to bromocodeine.

Chlorocodeine.—For the preparation of chlorocodeine a sufficient quantity of codeine is dissolved in an excess of dilute hydrochloric acid, at the temperature of about 150° or 160°. Finely-powdered chlorate of potash is then added, and the solution agitated. In the course of a few minutes a small quantity of the fluid is

tested with ammonia, in order to see whether a precipitate is formed; and the action is allowed to go on until this is obtained, and the chlorocodeine is then precipitated by a slight excess of ammonia. The successful performance of this experiment requires exactly the same precautions as the preparation of nitrocodeine; and, unless the action is stopped at the right moment, further products of decomposition are obtained. The reaction which takes place is represented by this equation:—

$$3\,(C_{36}\,H_{21}\,NO_{36}\,H\,Cl) + 3\,H\,Cl + KO\,Cl\,O_5 = K\,Cl + 6\,HO + 3\,(C_{36}\,H_{20}\,Cl\,NO_6\,H\,Cl).$$

The chlorocodeine is precipitated in the form of a silvery crystalline powder, closely resembling bromocodeine; it has generally a yellowish colour, and the fluid from which it has deposited is coloured dark-red by the presence of a small quantity of some products of the further action of chlorine. It retains also a small quantity of codeine, from which it is purified by dissolving in hydrochloric acid, boiling with animal charcoal, and reprecipitating with ammonia; and it is finally obtained in crystals from its solution in boiling spirit.

In its general properties chlorocodeine closely resembles bromocodeine; so much so, indeed, that they may be easily confounded with one another. It is sparingly soluble in boiling water, and deposited, on cooling, in minute prisms exactly similar to, and apparently isomorphous with, those of bromocodeine. It is readily soluble in strong alcohol, especially with heat, and sparingly soluble in ether. It dissolves in sulphuric acid in the cold without change, but the solution is charred by heating. Nitric acid dissolves it, and the solution is decomposed by boiling, but not by any means so readily as codeine. Red fumes are evolved along with a peculiar and excessively pungent vapour.

Analysis gave the following results:—

I. { 6·425 grains of chlorocodeine, dried at 212°, gave
15·315 ... of carbonic acid, and
3·601 ... of water.

II. { 6·162 grains of chlorocodeine gave
14·597 ... of carbonic acid, and
3·372 ... of water.

5·030 grains of chlorocodeine gave 2·100 grains chloride of silver.

	Experiment.		Calculation.		
	I.	II.			
Carbon,	65·00	64·62	64·76	C_{36}	216
Hydrogen,	6·22	6·08	5·99	H_{20}	20
Chlorine,	...	10·32	10·64	Cl	35·5
Nitrogen,	...	...	4·19	N	14
Oxygen,	...	...	14·42	O_6	48
			100·00		333·5

The crystallised base contains water which is expelled at 212°.

7·67 grains chlorocodeine lost 0·551 grains water, =7·18 per cent.
9·82 ... ... 0·740 ... =7·53 ...

The calculated number for three equivalents of water is 7·48 per cent.; and the formula of the crystallised base is therefore $C_{36}\,H_{20}\,Cl\,NO_6 + 3\,HO$.

The salts of chlorocodeine are exactly similar in their properties to those of bromocodeine; so much so, that I have not thought it necessary to examine more than one or two of them.

Hydrochlorate of Chlorodeine.—Crystallises in groups of needles, readily soluble in water.

Sulphate of Chlorocodeine is deposited from its hot solution in radiated groups of short prisms, which dissolve abundantly in boiling water and alcohol.

10·874 grains of the crystallised salt, dried at 212°, gave 0·953 grains of water, and 3·078 grains of sulphate of baryta.

	Experiment.	Calculation.		
Chlorocodeine, . .	79·34	79·63	Base.	333·5
Sulphuric acid, . .	11·90	11·75	$HOSO_3$	49·0
Water, . . .	8·76	8·662	4 HO	36·0
	100·00	100·00		418·5

Platinochloride of Chlorocodeine is obtained in the usual way, as a pale-yellow precipitate scarcely soluble in water. Its analysis gave the following results:—

7·212 grains platinochloride, dried at 212°, gave
10·658 ... of carbonic acid, and
2·655 ... of water.

8·793 grains platinochloride gave 1·608 grains platinum.

	Experiment.	Calculation.		
Carbon, . .	40·30	40·02	C_{36}	216
Hydrogen, . . .	4·09	3·89	H_{21}	21
Nitrogen, . . .	...	2·59	N	14
Oxygen, . . .	...	8·91	O_6	48
Chlorine, . . .	...	26·31	Cl_4	142
Platinum, . . .	18·29	18·28	Pt	98·7
	100·00			539·7

VII. *Action of Cyanogen on Codeine.*

Dicyanocodeine.—When a current of cyanogen is passed into a solution of codeine in the smallest possible quantity of alcohol, the gas is rapidly absorbed, and the fluid acquires, first a yellow, and, by continued action, a brown colour. If the solution be then left to itself for some time, the smell of cyanogen disappears,

and is replaced by that of hydrocyanic acid, and crystals are gradually deposited. In order to obtain the new compound in sufficient quantity, it is best to keep up a continuous slow current of cyanogen, by which means crystals are deposited during the action in considerable abundance. These are collected on a filter, and washed with a small quantity of alcohol; and the filtrate, on being again exposed to the action of cyanogen, yields an additional quantity of crystals inferior in purity to those obtained in the first part of the operation. The product is purified by solution with the aid of heat, in a mixture of alcohol and ether, from which it is deposited in crystals, which are colourless, or slightly yellow. Obtained in this way, however, they are apt to retain a small quantity of codeine: and it is, therefore, advantageous to pass cyanogen into the mixture to be used for their solution, by which means the last traces of codeine are converted into the new compound.

The substance so obtained is a new base, to which I give the name of Dicyanocodeine. It is soluble in boiling absolute alcohol, or a mixture of alcohol and ether, and is deposited on cooling in thin six-sided plates, with a brilliant lustre. It is difficultly soluble in water, but on the addition of alcohol it is dissolved; nothing, however, is deposited from the solution on standing, and by evaporation it is decomposed, and crystals of codeine are left behind. With hydrochloric acid, it is converted into a crystalline salt, but decomposition takes place immediately; for on the addition of potash to the fluid, ammonia escapes, and if it be left for four-and-twenty hours, hydrocyanic acid is evolved. With sulphuric and oxalic acid, it likewise gives somewhat sparingly soluble compounds, which decompose rapidly with the evolution of ammonia and hydrocyanic acid. The crystals deposited from alcohol and ether are anhydrous. Their analysis gave the following results :—

I. { 4·552 grains, dried in vacuo, gave
11·388 ... of carbonic acid, and
2·431 ... of water.

II. { 4·325 grains, dried in vacuo, gave
10·790 ... of carbonic acid, and
2·405 ... of water.

4·954 grains gave by Warrentrap and Will's method 9·320 grains of platinochloride of ammonium.

5·310 grains gave by the same method 9·890 grains of platinochloride.

	Experiment.		Calculation.		
Carbon, . .	68·22	68·04	68·37	C_{40}	240
Hydrogen, . .	5·93	6·17	5·97	H_{21}	21
Nitrogen, . .	11·81	11·50	11·68	N_3	42
Oxygen, . .	14·04	14·27	13·97	O_6	48
	100·00	100·00	100·00		351

These results correspond exactly with the formula $C_{40}\,H_{21}\,N_3\,O_6$. The method of its formation, however, indicates unequivocally, that its rational formula must be $C_{36}\,H_{21}\,NO_6\,2\,C_2\,N$, representing it as formed by two equivalents of cyanogen coupled with one of codeine, and belonging to the same class of compounds as cyaniline. It differs, however, from that substance, in containing two equivalents of cyanogen; and owing to this circumstance, I was at first inclined to take a different view of its constitution, and to consider it as the hydrocyanate of a cyanocodeine formed by substitution, and represented by the formula $C_{36}\,H_{20}\,Cy\,NO_6 + H\,Cy$, according to which its formation could obviously be equally well explained, and I considered the evolution of hydrocyanic acid, by treating it with acids, as favourable to this view. Attentive observation, however, convinced me, that though hydrocyanic acid always is produced by heating it with strong acids, it is never evolved immediately, as it necessarily must be, if it existed as such; but that it only makes its appearance after the lapse of some time, and that only as the result of an advanced decomposition; for long before it is observed, the addition of potash to the acid solution causes an abundant evolution of ammonia.

The ease with which dicyanocodeine is decomposed has prevented my examining any of its compounds. I attempted to prepare a platinum salt by rapid solution in hydrochloric acid, and precipitation by bichloride of platinum; but the instant the latter substance was added, evolution of hydrocyanic acid was observed, and the results obtained were, as might be expected, wholly incongruous and unsatisfactory. The decompositions of dicyanocodeine evidently afford several different substances; but I have not attempted to follow them out, as their investigation seemed to present some difficulties, among which, not the least was that of obtaining the base itself in sufficient quantity.

VIII. *Action of Alkalies on Codeine.*

Codeine, when treated at moderate temperatures with potash, yields more than one volatile base, according to the circumstances in which the experiment is made. I have found that similar results are obtained by the use of hydrate of potash, or of potash-lime, or soda-lime prepared in the usual way. The method employed in the experiment was to mix codeine with four or five times its weight of potash-lime or soda-lime, and introduce the mixture into a retort with a tubulated receiver, having a doubly-bent tube attached to its tubulature, the end of which passed into a small flask containing hydrochloric acid, in order to retain any of the very volatile base which might not be condensed in the receiver. The retort was introduced into an oil-bath, and kept at a uniform temperature of 250 Fahr. As soon as this temperature is reached, a slight peculiar odour is observed, which soon becomes more powerful, and a small quantity of water, retain-

ing the bases in solution, collects in the receiver. The decomposition at 250°, however, is excessively slow, and even after many days, bases are evolved apparently in undiminished quantity, but I retained the mixture steadily at this point, in hopes of obtaining the product free from ammonia, which my preliminary trials had shewn to be produced at higher temperatures; but I found that even with this low heat it was evolved always in appreciable, and, in some experiments, even in considerable quantity. I therefore gradually raised the temperature to about 350°, when a larger quantity of base was obtained; and after the heat had been sustained for some time, small crystals made their appearance, which deposited themselves in a line round the retort, just above the level of the oil in the bath, but which soon rose into and collected in the neck of the retort.

These crystals resemble benzoic acid in their external appearance, and are at first perfectly colourless, but soon acquire a brownish shade by exposure to light and air. They are a base, and rapidly restore the colour of reddened litmus. They are sparingly soluble in water, but readily in acids, and give a precipitate with bichloride of platinum. The quantity of this substance obtained was excessively minute; and though considerable quantities of codeine were operated upon, all that was obtained served only to make the few qualitative experiments now detailed.

The watery fluid which collected in the receiver possessed a pungent and peculiar smell; it restored the colour of reddened litmus with great rapidity, and gave abundant fumes with hydrochloric acid. On the addition of solid potash, a highly volatile and pungent oily base collected as a layer on the surface of the fluid, and at the same time a gaseous base escaped along with ammonia. From the small quantity of these substances which I was able to obtain, I could not attempt to prepare either of them in a pure state. I was therefore under the necessity of determining their constitution by the analysis of their platinum salts, which can be separated from one another, though not without difficulty. In order to prepare these salts, the basic fluid was saturated with hydrochloric acid, and evaporated to dryness in the water-bath, when it left behind a beautifully crystalline mass, highly soluble in water, and deliquescent in moist air. This was dissolved in absolute alcohol, to separate ammonia, and the filtered solution mixed with an alcoholic solution of bichloride of platinum, when the platinum salts were immediately thrown down as a pale-yellow powder, very sparingly soluble in absolute alcohol, but readily dissolved on the addition of water. The separation of the two bases is best effected by heating the washed precipitate with boiling absolute alcohol, and adding water in small quantities until the whole is dissolved. The crystals which deposit on cooling are one of the salts in a state of purity, if the process have been properly managed, or, at all events, only require a repetition of the process to make them absolutely pure. The salt thus obtained is scarcely soluble in absolute alcohol or ether, but is readily

soluble in water and dilute spirit, and is thrown down from the latter solution by ether in the form of fine yellow scales. Its analysis gave the following results:—

8·723 grains, dried at 212°, gave
1·753 ... of carbonic acid, and
2·090 ... of water.

9·880 grains, dried at 212°, gave 4·100 grains of platinum.
7·734 ... ... 3·196 ... ...

	Experiment.		Calculation.		
	I.	II.			
Carbon, . . .	5·48	...	5·06	C_2	12
Hydrogen, . .	2·66	...	2·52	H_6	6
Nitrogen, . .	...	...	5·90	N	14
Chlorine, . .	...	...	44·91	Cl_3	106·5
Platinum, . .	41·49	41·32	41·61	Pt	98·7
			100·00		237·2

The formula of the salt is therefore $C_2 H_5 N H Cl Pt Cl_2$; and the base is, consequently, the methylamine of WURTZ, with whose description of that substance and its platinum salt it perfectly agrees.

The preparation of the platinum salt of the other base was attended with much greater difficulty; and I did not succeed in obtaining it quite free from methylamine. In order to obtain it, the fluid which had deposited the methylamine salt was evaporated to a small bulk, the salt which separated filtered off, and ether added to the mother-liquor. Immediately a precipitate is obtained, generally in the form of minute yellow-needles, but sometimes in scales. It is sparingly soluble in alcohol and ether, and highly soluble in water, from which it crystallises in long needles, and with such facility, that a few drops evaporated on a watch-glass leave the salt they contain in the form of five or six needles crossing the whole space occupied by the solution. The quantity of this salt which I had at my disposal was too small to admit of my carrying its purification by recrystallisation as far as was to be desired, and, consequently, a small quantity of methylamine remained in those subjected to analysis.

I. 5·521 grains of platinum salt, dried at 212°, gave
2·485 ... of carbonic acid, and
1·800 ... of water.

10·475 grains platinum salt, gave 3·951 grains platinum.
6·475 ... ... 2·432 ... ...

	Experiment.		Calculation.		
	I.	II.			
Carbon, . . .	12·27	...	13·57	C_6	36
Hydrogen, . .	3·62	...	3·77	H_{10}	10
Nitrogen, . .	...	...	5·27	N	14
Chlorine, . .	...	...	40·18	Cl_3	106·5
Platinum, . .	37·71	37·56	37·21	Pt	98·7
			100·00		265·2

These results approach most closely to the formula $C_6 H_9 N H Cl Pt Cl_2$; and though the carbon is very deficient, and the platinum considerably in excess, there can be no doubt that this is due to the imperfect separation of the methylamine, and that this is its true formula; and that of the base itself $C_6 H_9 N$. The base, then, obviously belongs to the same series as methylamine, and forms the term of the series corresponding to metacetonic acid, and, in accordance with the system of nomenclature adopted by WURTZ, it receives the name of metacetamine. I have not attempted the examination of the salts of this base, as I did not obtain it in sufficient quantity for that purpose; but I take the opportunity of stating, that before I had obtained it from codeine I had ascertained its existence among the products of destructive distillation of animal substances, and that I shall, at a future period, detail the properties of its compounds.*

The residue in the retort after these bases have been evolved, is dark cinnamon-brown, and slightly coherent; it dissolves in water, with a dark-brown, almost black colour, and gives with acids a flocculent brown precipitate of a humus-like substance, and perfectly amorphous, which I have not thought it necessary to examine. It still contains nitrogen; and by exposure to a heat gradually raised to low redness, it gives an additional quantity of volatile bases, among which ammonia becomes more and more abundant as the temperature rises. A non-basic oil also makes its appearance, but only in very small quantity.

Since these experiments were made, I have received the February number of the *Annalen der Chimie und Pharmacie*, which contains a preliminary notice of an investigation by WERTHEIM of the action of soda-lime on certain organic bases. He has obtained metacetamine from narcotine, and methylamine from morphia; and considering these substances to be directly eliminated from the bases, he expects to obtain the residual atoms in the form of a definite compound. I entertained a similar idea with regard to codeine, until I detected the formation of two different bases, which seemed to me rather to indicate that these substances appear as the result of a true destructive distillation; and that possibly by varying the circumstances of the experiment, other bases may be obtained.

I have also observed another remarkable decomposition of codeine, by which volatile bases are obtained. I have already mentioned the formation, by the action of nitric acid, of a resinous acid, with the examination of which I am still engaged. This acid, which is insoluble in water, dissolves readily in dilute potash, with a red colour; and the solution on boiling evolves a volatile base in

* I may at the same time mention, that I have convinced myself that the petinine described by me two years since as existing in bone-oil, is represented by the formula $C_8 H_{11} N$, and not by $C_8 H_{10} N$, which I then gave for it. Indeed, my analysis of the platinum salt, which is most to be depended upon, tallies equally well with either formula. I have also ascertained the existence of ethylamine and methylamine in bone-oil. The details of these experiments will be contained in the second part of my paper on the Products of the Destructive Distillation of Animal Matters.

great abundance. I have not yet determined the whole circumstances under which this change takes place, but reserve this for a future communication.*

I have likewise examined the action of iodine on codeine, which yields a magnificent crystalline compound presenting the phenomena of pleochroism in a remarkable manner. Difficulties connected with the analysis have, however, prevented my hitherto completing its investigation.

The following is a Tabular View of the constitutions of the substances described in this paper:—

Codeine,	$C_{36}\ H_{21}\ NO_6$.
... crystallised,	$C_{36}\ H_{21}\ NO_6 + 2\ HO$.
Hydrochlorate,	$C_{36}\ H_{21}\ NO_6\ HCl + 4\ HO$.
Hydriodate,	$C_{36}\ H_{21}\ NO_6\ HI + 2\ HO$.
Sulphate,	$C_{36}\ H_{21}\ NO_6\ HO\ SO_3 + 5\ HO$.
Nitrate,	$C_{36}\ H_{21}\ NO_6\ HO\ NO_5$.
Phosphate,	$(C_{36}\ H_{21}\ NO_6\ HO)\ 2\ HO\ PO_5 + 3\ HO$.
Oxalate,	$C_{36}\ H_{21}\ NO_6\ HO\ C_2\ O_3 + 3\ HO$.
Hydrosulphocyanate,	$C_{36}\ H_{21}\ NO_6\ HC_2\ NS_2 + HO$.
Platinum salt dried at 212°,	$C_{36}\ H_{21}\ NO_6\ HCl\ Pt\ Cl_2 + HO$.
... crystallised,	$C_{36}\ H_{21}\ NO_6\ HCl\ Pt\ Cl_2 + 3\ HO$.
Amorphous codeine,	$C_{36}\ H_{21}\ NO_6$.
Nitrocodeine,	$C_{36}\ H_{20}\ (NO_4)\ NO_6$.
Sulphate,	$C_{36}\ H_{20}\ (NO_4)\ NO_6\ HO\ SO_3$.
Platinum salt,	$C_{36}\ H_{20}\ (NO_4)\ NO_6\ HCl\ Pt\ Cl_2 + 4\ HO$.
Bromocodeine,	$C_{36}\ H_{20}\ Br\ NO_6$.
... hydrate,	$C_{36}\ H_{20}\ Br\ NO_6 + HO$.
... terhydrate,	$C_{36}\ H_{20}\ Br\ NO_6 + 3\ HO$.
Hydrobromate,	$C_{36}\ H_{20}\ Br\ NO_6\ HBr + 2\ HO$.
Platinum salt,	$C_{36}\ H_{20}\ Br\ NO_6\ HCl\ Pt\ Cl_2$.
Tribromocodeine,	$C_{36}\ H_{18}\ Br_3\ NO_6$.
Hydrobromate,	$2\ (C_{36}\ H_{18}\ Br_3\ NO_6)\ 3\ HBr$.
Platinum salt,	$C_{36}\ H_{18}\ Br_3\ NO_6\ HCl\ Pt\ Cl_2$.
Chlorocodeine,	$C_{36}\ H_{20}\ Cl\ NO_6$.
... terhydrate,	$C_{36}\ H_{20}\ Cl\ NO_6 + 3\ HO$.
Sulphate,	$C_{36}\ H_{20}\ Cl\ NO_6\ HO\ SO_3 + 4\ HO$.
Platinum salt,	$C_{36}\ H_{20}\ Cl\ NO_6\ HCl\ Pt\ Cl_2$.
Dicyanocodeine,	$C_{36}\ H_{21}\ NO_6\ 2\ C_2\ N$.
Metacetamine,	$C_6\ H_9\ N$.

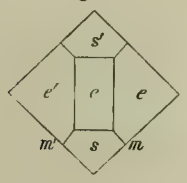
* The action of nitric acid on the organic alkalies, in this point of view, is now under investigation in my laboratory. Narcotine has been found to undergo a precisely similar change, yielding a compound, which gives off a volatile base by ebullition with potash, and a whole series of other substances, the constitution of which will be detailed so soon as the investigations are completed.

SUPPLEMENT.

While engaged with the investigation of codeine, I sent to Professor MILLER, of Cambridge, some crystals of the base and its sulphate for crystallographic measurement. Owing to Professor MILLER's other avocations, he was unable to furnish me with the results in sufficient time to admit of their being incorporated with the foregoing paper. I have, therefore, introduced them here in the shape of a supplement, as they form a valuable addition to the observations contained in the paper.

Codeine.—Prismatic. The symbols of the simple forms are, *c* 001, *s* 011, *e* 101, *u* 102, *m* 110. The angles between normals to the faces are:

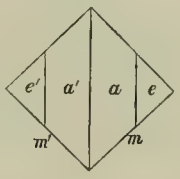
Fig. 1.



	°	′
m c	90	0
s c	38	37
s s'	77	14
e c	39	46
e e'	79	32
u c	22	35
u u'	45	10
m m'	87	40
e m	63	42
s m	63	15
s e	53	3

Cleavage, *c*.

Fig. 2.



The faces *m m'* are usually of very unequal magnitude. The faces *s s'* were not observed upon the same crystals. The form *s* is probably hemihedral.

Fig. 1. Codeine crystallised from alcohol.

Fig. 2. Codeine crystallised from water.

The agreement of the different observations is not very good, so that the above measures must be considered as approximations only.

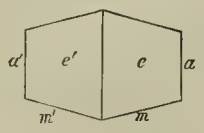
Sulphate of Codeine.—Prismatic. Symbols of the simple forms, *a* 100, *e* 101, *m* 110.

The angles between normals to the faces are:

	°	′
e a	66	15
e e'	46	30
m a	75	36
m m'	28	48

Cleavage, *a*.

Fig. 3.



IV.—*On the Equilibrium of Elastic Solids.* By James Clerk Maxwell, Esq.

(Read 18th February, 1850).

There are few parts of mechanics in which theory has differed more from experiment than in the theory of elastic solids.

Mathematicians, setting out from very plausible assumptions with respect to the constitution of bodies, and the laws of molecular action, came to conclusions which were shewn to be erroneous by the observations of experimental philosophers. The experiments of Œrsted proved to be at variance with the mathematical theories of Navier, Poisson, and Lamé and Clapeyron, and apparently deprived this practically important branch of mechanics of all assistance from mathematics.

The assumption on which these theories were founded may be stated thus:—

Solid bodies are composed of distinct molecules, which are kept at a certain distance from each other by the opposing principles of attraction and heat. When the distance between two molecules is changed, they act on each other with a force whose direction is in the line joining the centres of the molecules, and whose magnitude is equal to the change of distance multiplied into a function of the distance which vanishes when that distance becomes sensible.

The equations of elasticity deduced from this assumption contain only *one* coefficient, which varies with the nature of the substance.

The insufficiency of one coefficient may be proved from the existence of bodies of different degrees of solidity.

No effort is required to retain a liquid in any form, if its volume remain unchanged; but when the form of a solid is changed, a force is called into action which tends to restore its former figure; and this constitutes the difference between elastic solids and fluids. Both tend to recover their *volume*, but fluids do not tend to recover their *shape*.

Now, since there are in nature bodies which are in every intermediate state from perfect solidity to perfect liquidity, these two elastic powers cannot exist in every body in the same proportion, and therefore all theories which assign to them an invariable ratio must be erroneous.

I have therefore substituted for the assumption of Navier the following axioms as the results of experiments.

If three pressures in three rectangular axes be applied at a point in an elastic solid,—

1. *The sum of the three pressures is proportional to the sum of the compressions which they produce.*

2. *The difference between two of the pressures is proportional to the difference of the compressions which they produce.*

The equations deduced from these axioms contain two coefficients, and differ from those of NAVIER only in not assuming any invariable ratio between the cubical and linear elasticity. They are the same as those obtained by Professor STOKES from his equations of fluid motion, and they agree with all the laws of elasticity which have been deduced from experiments.

In this paper *pressures* are expressed by the number of units of weight to the unit of surface; if in English measure, in pounds to the square inch, or in atmospheres of 15 pounds to the square inch.

Compression is the proportional change of any dimension of the solid caused by pressure, and is expressed by the quotient of the change of dimension divided by the dimension compressed.*

Pressure will be understood to include tension, and compression dilatation; pressure and compression being reckoned positive.

Elasticity is the force which opposes pressure, and the *equations of elasticity* are those which express the relation of pressure to compression.†

Of those who have treated of elastic solids, some have confined themselves to the investigation of the laws of the bending and twisting of rods, without considering the relation of the coefficients which occur in these two cases; while others have treated of the general problem of a solid body exposed to any forces.

The investigations of LEIBNITZ, BERNOULLI, EULER, VARIGNON, YOUNG, LA HIRE, and LAGRANGE, are confined to the equilibrium of bent rods; but those of NAVIER, POISSON, LAMÉ and CLAPEYRON, CAUCHY, STOKES, and WERTHEIM, are principally directed to the formation and application of the general equations.

The investigations of NAVIER are contained in the seventh volume of the *Memoirs of the Institute*, page 373; and in the *Annales de Chimie et de Physique*, 2e *Série*, xv., 264, and xxxviii., 435; *L'Application de la Mécanique*, tom. i.

Those of POISSON in *Mém. de l'Institut*, viii., 429; *Annales de Chimie*, 2e *Série*, xxxvi., 334; xxxvii., 337; xxxviii., 338; xlii. *Journal de l'École Polytechnique*, cahier xx., with an abstract in *Annales de Chimie* for 1829.

The memoir of MM. LAMÉ and CLAPEYRON is contained in CRELLE'S *Mathematical Journal*, vol. vii.; and some observations on elasticity are to be found in LAMÉ'S *Cours de Physique*.

M. CAUCHY'S investigations are contained in his *Exercises de Analyse*, vol. iii., p. 180, published in 1828.

Instead of supposing each pressure proportional to the linear compression which it produces, he supposes it to consist of two parts, one of which is propor-

* The laws of pressure and compression may be found in the Memoir of Lamé and Clapeyron. See note A.

† See note B.

tional to the linear compression in the direction of the pressure, while the other is proportional to the diminution of volume. As this hypothesis admits two coefficients, it differs from that of this paper only in the values of the coefficients selected. They are denoted by K and k, and $K=\mu-\frac{1}{3}m$, $k=m$.

The theory of Professor STOKES is contained in Vol. viii., Part 3, of the *Cambridge Philosophical Transactions*, and was read April 14, 1845.

He states his general principles thus:—"The capability which solids possess of being put into a state of isochronous vibration, shews that the pressures called into action by small displacements depend on homogeneous functions of those displacements of one dimension. I shall suppose, moreover, according to the general principle of the superposition of small quantities, that the pressures due to different displacements are superimposed, and, consequently, that the pressures are linear functions of the displacements."

Having assumed the proportionality of pressure to compression, he proceeds to define his coefficients.—"Let $-A\delta$ be the pressures corresponding to a uniform linear dilatation δ when the solid is in equilibrium, and suppose that it becomes $mA\delta$, in consequence of the heat developed when the solid is in a state of rapid vibration. Suppose, also, that a displacement of shifting parallel to the plane xy, for which $\delta x=kx$, $\delta y=-ky$, and $\delta z=0$, calls into action a pressure $-Bk$ on a plane perpendicular to the axis of x, and a pressure Bk on a plane perpendicular to the axis of y; the pressure on these planes being equal and of contrary signs; that on a plane perpendicular to z being zero, and the tangential forces on those planes being zero." The coefficients A and B, thus defined, when expressed as in this paper, are $A=3\mu$, $B=\frac{m}{2}$.

Professor STOKES does not enter into the solution of his equations, but gives their results in some particular cases.

1. A body exposed to a uniform pressure on its whole surface.
2. A rod extended in the direction of its length.
3. A cylinder twisted by a statical couple.

He then points out the method of finding A and B from the two last cases.

While explaining why the equations of motion of the luminiferous ether are the same as those of incompressible elastic solids, he has mentioned the property of *plasticity* or the tendency which a constrained body has to relieve itself from a state of constraint, by its molecules assuming new positions of equilibrium. This property is opposed to linear elasticity; and these two properties exist in all bodies, but in variable ratio.

M. WERTHEIM, in *Annales de Chimie*, 3^e *Série*, xxiii., has given the results of some experiments on caoutchouc, from which he finds that $K=k$, or $\mu=\frac{1}{3}m$; and concludes that $k=K$ in all substances. In his equations, μ is therefore made equal to $\frac{1}{3}m$.

The accounts of experimental researches on the values of the coefficients are so numerous that I can mention only a few.

Canton, Perkins, Œrsted, Aimé, Colladon and Sturm, and Regnault, have determined the cubical compressibilities of substances; Coulomb, Duleau, and Giulio, have calculated the linear elasticity from the torsion of wires; and a great many observations have been made on the elongation and bending of beams.

I have found no account of any experiments on the relation between the doubly refracting power communicated to glass and other elastic solids by compression, and the pressure which produces it;* but the phenomena of bent glass seem to prove, that, in homogeneous singly-refracting substances exposed to pressures, the principal axes of pressure coincide with the principal axes of double refraction; and that the difference of pressures in any two axes is proportional to the difference of the velocities of the oppositely polarised rays whose directions are parallel to the third axis. On this principle I have calculated the phenomena seen by polarised light in the cases where the solid is bounded by parallel planes.

In the following pages I have endeavoured to apply a theory identical with that of Stokes to the solution of problems which have been selected on account of the possibility of fulfilling the conditions. I have not attempted to extend the theory to the case of imperfectly elastic bodies, or to the laws of permanent bending and breaking. The solids here considered are supposed not to be compressed beyond the limits of perfect elasticity.

The equations employed in the transformation of co-ordinates may be found in Gregory's *Solid Geometry*.

I have denoted the displacements by δx, δy, δz. They are generally denoted by α, β, γ; but as I had employed these letters to denote the principal axes at any point, and as this had been done throughout the paper, I did not alter a notation which to me appears natural and intelligible.

The laws of elasticity express the relation between the changes of the dimensions of a body and the forces which produce them.

These forces are called Pressures, and their effects Compressions. Pressures are estimated in pounds on the square inch, and compressions in fractions of the dimensions compressed.

Let the position of material points in space be expressed by their co-ordinates x, y, and z, then any change in a system of such points is expressed by giving to these co-ordinates the variations δx, δy, δz, these variations being functions of x, y, z.

The quantities δx, δy, δz, represent the absolute motion of each point in the directions of the three co-ordinates; but as compression depends not on absolute, but on relative displacement, we have to consider only the nine quantities—

* See note C.

$$\begin{array}{ccc} \frac{d\,\delta x}{d x} & \frac{d\,\delta x}{d y} & \frac{d\,\delta x}{d z} \\ \frac{d\,\delta y}{d x} & \frac{d\,\delta y}{d y} & \frac{d\,\delta y}{d z} \\ \frac{d\,\delta z}{d x} & \frac{d\,\delta z}{d y} & \frac{d\,\delta z}{d z} \end{array}$$

Since the number of these quantities is nine, if nine other independent quantities of the same kind can be found, the one set may be found in terms of the other. The quantities which we shall assume for this purpose are—

1. Three compressions, $\frac{\delta\,\alpha}{\alpha}, \frac{\delta\,\beta}{\beta}, \frac{\delta\,\gamma}{\gamma}$, in the directions of three principal axes α, β, γ.

2. The nine *direction-cosines* of these axes, with the *six connecting equations*, leaving three independent quantities. (See GREGORY'S *Solid Geometry*).

3. The small angles of rotation of this system of axes about the axes of x, y, z,

The cosines of the angles which the axes of x, y, z make with those of α, β, γ are—

$$\cos(\alpha\,0\,x)=a_1,\ \cos(\beta\,0\,x)=b_1,\ \cos(\gamma\,0\,x)=c_1,$$
$$\cos(\alpha\,0\,y)=a_2,\ \cos(\beta\,0\,y)=b_2,\ \cos(\gamma\,0\,y)=c_2,$$
$$\cos(\alpha\,0\,z)=a_3,\ \cos(\beta\,0\,z)=b_3,\ \cos(\gamma\,0\,z)=c_3,$$

These *direction-cosines* are connected by the six equations,

$$a_1^2+b_1^2+c_1^2=1 \qquad a_1 a_2+b_1 b_2+c_1 c_2=0$$
$$a_2^2+b^2{}_2+c_2^2=1 \qquad a_2 a_3+b_2 b_3+c_2 c_3=0$$
$$a_3^2+b_3^2+c_3^2=1 \qquad a_3 a_1+b_3 b_1+c_3 c_1=0$$

The rotation of the system of axes α, β, γ, round the axis of

$$x, \text{ from } y \text{ to } z, = \delta\,\theta_1,$$
$$y, \text{ from } z \text{ to } x, = \delta\,\theta_2,$$
$$z, \text{ from } x \text{ to } y, = \delta\,\theta_3;$$

By resolving the displacements $\delta\,\alpha, \delta\,\beta, \delta\,\gamma, \theta_1, \theta_2, \theta_3$, in the directions of the axes x, y, z, the displacements in these axes are found to be

$$\delta x = a_1\,\delta\,\alpha + b_1\,\delta\,\beta + c_1\,\delta\,\gamma - \theta_2 z + \theta_3 y$$
$$\delta y = a_2\,\delta\,\alpha + b_2\,\delta\,\beta + c_2\,\delta\,\gamma - \theta_3 x + \theta_1 z$$
$$\delta z = a_3\,\delta\,\alpha + b_3\,\delta\,\beta + c_2\,\delta\,\gamma - \theta_1 y + \theta_2 x$$

But $$\delta\,\alpha = \alpha\frac{\delta\,\alpha}{\alpha},\ \delta\,\beta = \beta\frac{\delta\,\beta}{\beta},\ \text{and}\ \delta\,\gamma = \gamma\frac{\delta\,\gamma}{\gamma},$$

and $$\alpha = a_1 x + a_2 y + a_3 z,\ \beta = b_1 x + b_2 y + b_3 z,\ \text{and}\ \gamma = c_1 x + c_2 y + c_3 z.$$

Substituting these values of $\delta\,\alpha, \delta\,\beta$, and $\delta\,\gamma$ in the expressions for $\delta x, \delta y,$

δz, and differentiating with respect to x, y, and z, in each equation, we obtain the equations—

$$\left.\begin{aligned}
\frac{d\,\delta x}{dx} &= \frac{\delta\alpha}{\alpha}a_1^2 + \frac{\delta\beta}{\beta}b_1^2 + \frac{\delta\gamma}{\gamma}c_1^2 \\
\frac{d\,\delta y}{dy} &= \frac{\delta\alpha}{\alpha}a_2^2 + \frac{\delta\beta}{\beta}b_2^2 + \frac{\delta\gamma}{\gamma}c_2^2 \\
\frac{d\,\delta z}{dz} &= \frac{\delta\alpha}{\alpha}a_3^2 + \frac{\delta\beta}{\beta}b_3^2 + \frac{\delta\gamma}{\gamma}c_3^2
\end{aligned}\right\} \quad (1).$$

Equations of compression.

$$\left.\begin{aligned}
\frac{d\,\delta x}{dy} &= \frac{\delta\alpha}{\alpha}a_1a_2 + \frac{\delta\beta}{\beta}b_1b_2 + \frac{\delta\gamma}{\gamma}c_1c_2 + \delta\theta_3 \\
\frac{d\,\delta x}{dz} &= \frac{\delta\alpha}{\alpha}a_1a_3 + \frac{\delta\beta}{\beta}b_1b_3 + \frac{\delta\gamma}{\gamma}c_1c_3 - \delta\theta_2 \\
\frac{d\,\delta y}{dz} &= \frac{\delta\alpha}{\alpha}a_2a_3 + \frac{\delta\beta}{\beta}b_2b_3 + \frac{\delta\gamma}{\gamma}c_2c_3 + \delta\theta_1 \\
\frac{d\,\delta y}{dx} &= \frac{\delta\alpha}{\alpha}a_2a_1 + \frac{\delta\beta}{\beta}b_2b_1 + \frac{\delta\gamma}{\gamma}c_2c_1 - \delta\theta_3 \\
\frac{d\,\delta z}{dx} &= \frac{\delta\alpha}{\alpha}a_3a_1 + \frac{\delta\beta}{\beta}b_3b_1 + \frac{\delta\gamma}{\gamma}c_3c_1 + \delta\theta_2 \\
\frac{d\,\delta z}{dy} &= \frac{\delta\alpha}{\alpha}a_3a_2 + \frac{\delta\beta}{\beta}b_3b_2 + \frac{\delta\gamma}{\gamma}c_3c_2 - \delta\theta_1
\end{aligned}\right\} \quad (2).$$

Equations of the equilibrium of an element of the solid.

The forces which may act on a particle of the solid are:—

1. Three attractions in the direction of the axes, represented by X, Y, Z.
2. Six pressures on the six faces.
3. Two tangential actions on each face.

Let the six faces of the small parallelopiped be denoted by x_1, y_1, z_1, x_2, y_2, and z_2, then the forces acting on x_1 are:—

1. A normal pressure p_1 acting in the direction of x on the area $dy\,dz$.
2. A tangential force q_3 acting in the direction of y on the same area.
3. A tangential force q_2^1 acting in the direction of z on the same area, and so on for the other five faces, thus:—

Forces which act in the direction of the axes of

		x	y	z
On the face	x_1	$-p_1\,dy\,dz$	$-q_3\,dy\,dz$	$-q_2^1\,dy\,dz$
... ...	x_2	$(p_1+\frac{dp_1}{dx}dx)\,dy\,dz$	$(q_3+\frac{dq_3}{dx}dx)\,dy\,dx$	$(q_2^1+\frac{dq_2^1}{dx}dx)\,dy\,dz$
... ...	y_1	$-q_3^1\,dz\,dx$	$-p_2\,dz\,dx$	$-q_1\,dz\,dx$
... ...	y_2	$(q_3+\frac{dq_3^1}{dy}dy)\,dz\,dx$	$(p_2+\frac{dp_2}{dy}dy)\,dz\,dx$	$(q_1+\frac{dq_1}{dy}dy)\,dz\,dx$

On the face	z_1	$-q_2\,dx\,dy$	$-q^1\,dx\,dy$	$-p_3\,dx\,dy$
... ...	z_2	$(q_2+\frac{dq_2}{dz}\,dz)\,dx\,dy$	$(q^1+\frac{dp^1}{dz}\,dz)\,dx\,dy$	$(p_3+\frac{dp_3}{dz}\,dz)\,dx\,dy$
Attractions,		$\rho\,X\,dx\,dy\,dz$	$\rho\,Y\,dx\,dy\,dz$	$\rho\,Z\,dx\,dy\,dz$

Taking the moments of these forces round the axes of the particle, we find

$$q_1{}^1=q_1 \quad q_1{}^2=q_2 \quad q_3{}^1=q_3\,;$$

and then equating the forces in the directions of the three axes, and dividing by dx, dy, dz, we find the equations of pressures.

$$\left.\begin{aligned} \frac{dp_1}{dx}+\frac{dq_3}{dy}+\frac{dq_2}{dz}+\rho X&=0\\ \frac{dp_2}{dy}+\frac{dq_1}{dz}+\frac{dq_3}{dx}+\rho Y&=0\\ \frac{dp_3}{dz}+\frac{dq_2}{dx}+\frac{dq_1}{dy}+\rho Z&=0 \end{aligned}\right\}\quad \text{Equations of Pressures.}\quad \ldots\ (3.)$$

The resistance which the solid opposes to these pressures is called Elasticity, and is of two kinds, for it opposes either change of *volume* or change of *figure*. These two kinds of elasticity have no necessary connection, for they are possessed in very different ratios by different substances. Thus *jelly* has a cubical elasticity little different from that of water, and a linear elasticity as small as we please; while *cork*, whose cubical elasticity is very small, has a much greater linear elasticity than jelly.

HOOKE discovered that the elastic forces are proportional to the changes that excite them, or, as he expressed it, "Ut tensio sic vis."

To fix our ideas, let us suppose the compressed body to be a parallelopiped, and let pressures P_1, P_2, P_3 act on its faces in the direction of the axes α, β, γ, which will become the principal axes of compression, and the compressions will be $\frac{\delta\alpha}{\alpha}$, $\frac{\delta\beta}{\beta}$, $\frac{\delta\gamma}{\gamma}$.

The fundamental assumption from which the following equations are deduced is an extension of HOOKE's law, and consists of two parts.

I. The sum of the compressions is proportional to the sum of the pressures.

II. The difference of the compressions is proportional to the difference of the pressures.

These laws are expressed by the following equations:—

$$\text{I. } (P_1+P_2+P_3)=3\mu\left(\frac{\delta\alpha}{\alpha}+\frac{\delta\beta}{\beta}+\frac{\delta\gamma}{\gamma}\right)\quad \ldots\ (4.)$$

$$\text{II. } \left.\begin{aligned} (P_1-P_2)&=m\left(\frac{\delta\alpha}{\alpha}-\frac{\delta\beta}{\beta}\right)\\ (P_2-P_3)&=m\left(\frac{\delta\beta}{\beta}-\frac{\delta\gamma}{\gamma}\right)\\ (P_3-P_1)&=m\left(\frac{\delta\gamma}{\gamma}-\frac{\delta\alpha}{\alpha}\right) \end{aligned}\right\}\quad \text{Equations of Elasticity.}\quad \ldots\ (5.)$$

The quantity μ is the coefficient of cubical elasticity, and m that of linear elasticity.

By solving these equations, the values of the pressures P_1, P_2, P_3, and the compressions $\frac{\delta\alpha}{\alpha}$, $\frac{\delta\beta}{\beta}$, $\frac{\delta\gamma}{\gamma}$ may be found.

$$\left.\begin{aligned} P_1 &= (\mu - \tfrac{1}{3}m)\left(\frac{\delta\alpha}{\alpha}+\frac{\delta\beta}{\beta}+\frac{\delta\gamma}{\gamma}\right) + m\frac{\delta\alpha}{\alpha} \\ P_2 &= (\mu - \tfrac{1}{3}m)\left(\frac{\delta\alpha}{\alpha}+\frac{\delta\beta}{\beta}+\frac{\delta\gamma}{\gamma}\right) + m\frac{\delta\beta}{\beta} \\ P_3 &= (\mu - \tfrac{1}{3}m)\left(\frac{\delta\alpha}{\alpha}+\frac{\delta\beta}{\beta}+\frac{\delta\gamma}{\gamma}\right) + m\frac{\delta\gamma}{\gamma} \end{aligned}\right\} \quad . \quad (6.)$$

$$\left.\begin{aligned} \frac{\delta\alpha}{\alpha} &= \left(\frac{1}{9\mu}-\frac{1}{3m}\right)(P_1+P_2+P_3) + \frac{1}{m}P_1 \\ \frac{\delta\beta}{\beta} &= \left(\frac{1}{9\mu}-\frac{1}{3m}\right)(P_1+P_2+P_3) + \frac{1}{m}P_2 \\ \frac{\delta\gamma}{\gamma} &= \left(\frac{1}{9\mu}-\frac{1}{3m}\right)(P_1+P_2+P_3) + \frac{1}{m}P_3 \end{aligned}\right\} \quad . \quad (7.)$$

From these values of the pressures in the axes α, β, γ, may be obtained the equations for the axes x, y, z, by resolution of pressures and compressions.*

For $$p = a^2 P_1 + b^2 P_2 + c^2 P_3$$

and $$q = a\,a\,P_1 + b\,b\,P_2 + c\,c\,P_3$$

$$\left.\begin{aligned} p_1 &= (\mu - \tfrac{1}{3}m)\left(\frac{d\delta x}{dx}+\frac{d\delta y}{dy}+\frac{d\delta z}{dz}\right) + m\frac{d\delta x}{dx} \\ p_2 &= (\mu - \tfrac{1}{3}m)\left(\frac{d\delta x}{dx}+\frac{d\delta y}{dy}+\frac{d\delta z}{dz}\right) + m\frac{d\delta y}{dy} \\ p_3 &= (\mu - \tfrac{1}{3}m)\left(\frac{d\delta x}{dx}+\frac{d\delta y}{dy}+\frac{d\delta z}{dz}\right) + m\frac{d\delta z}{dz} \end{aligned}\right\} \quad . \quad (8.)$$

$$\left.\begin{aligned} q_1 &= \frac{m}{2}\left(\frac{d\delta y}{dz}+\frac{d\delta z}{dy}\right) \\ q_2 &= \frac{m}{2}\left(\frac{d\delta z}{dx}+\frac{d\delta x}{dz}\right) \\ q_3 &= \frac{m}{2}\left(\frac{d\delta x}{dy}+\frac{d\delta y}{dx}\right) \end{aligned}\right\} \quad . \quad (9.)$$

$$\left.\begin{aligned} \frac{d\delta x}{dx} &= \left(\frac{1}{9\mu}-\frac{1}{3m}\right)(p_1+p_2+p_3) + \frac{1}{m}p_1 \\ \frac{d\delta y}{dy} &= \left(\frac{1}{9\mu}-\frac{1}{3m}\right)(p_1+p_2+p_3) + \frac{1}{m}p_2 \\ \frac{d\delta z}{dz} &= \left(\frac{1}{9\mu}-\frac{1}{3m}\right)(p_1+p_2+p_3) + \frac{1}{m}p_3 \end{aligned}\right\} \quad . \quad (10.)$$

* See the Memoir of Lamé and Clapeyron, and note A.

$$\left.\begin{aligned} \frac{d\,\delta x}{dy} - \delta\theta_3 = \frac{d\,\delta y}{dx} + \delta\theta_3 = \frac{1}{m}q_3 \\ \frac{d\,\delta y}{dz} - \delta\theta_1 = \frac{d\,\delta z}{dy} + \delta\theta_1 = \frac{1}{m}q_1 \\ \frac{d\,\delta z}{dx} - \delta\theta_2 = \frac{d\,\delta x}{dz} + \delta\theta_2 = \frac{1}{m}q_2 \end{aligned}\right\} . \quad (11.)$$

By substituting in Equations (3.) the values of the forces given in Equations (8.) and (9.), they become

$$\left.\begin{aligned} (\mu+\tfrac{1}{6}m)\left(\frac{d}{dx}\left(\frac{d\,\delta x}{dx}+\frac{d\,\delta y}{dy}+\frac{d\,\delta z}{dz}\right)\right)+\frac{m}{2}\left(\frac{d^2}{dx^2}\delta x+\frac{d^2}{dy^2}\delta y+\frac{d^2}{dz^2}\delta z\right)+\rho X=0 \\ (\mu+\tfrac{1}{6}m)\left(\frac{d}{dy}\left(\frac{d\,\delta x}{dx}+\frac{d\,\delta y}{dy}+\frac{d\,\delta z}{dz}\right)\right)+\frac{m}{2}\left(\frac{d^2}{dx^2}\delta x+\frac{d^2}{dy^2}\delta y+\frac{d^2}{dz^2}\delta z\right)+\rho Y=0 \\ (\mu+\tfrac{1}{6}m)\left(\frac{d}{dz}\left(\frac{d\,\delta x}{dx}+\frac{d\,\delta y}{dy}+\frac{d\,\delta z}{dz}\right)\right)+\frac{m}{2}\left(\frac{d^2}{dx^2}\delta x+\frac{d^2}{dy^2}\delta y+\frac{d^2}{dz^2}\delta z\right)+\rho Z=0 \end{aligned}\right\} \quad (12.)$$

These are the general equations of elasticity, and are identical with those of M. CAUCHY, in his *Exercises d'Analyse*, vol. iii., p. 180, published in 1828, when k stands for m, and K for $\mu-\frac{m}{2}$, and those of Mr STOKES, given in the *Cambridge Philosophical Transactions*, vol. viii., part 3, and numbered (30.); in his equations $A=3\mu$, $B=\frac{m}{2}$.

If the temperature is variable from one part to another of the elastic solid, the compressions $\frac{d\,\delta x}{dx}$, $\frac{d\,\delta y}{dy}$, $\frac{d\,\delta z}{dz}$, at any point will be diminished by a quantity proportional to the temperature at that point. This principle is applied in Cases X. and XI. Equations (10.) then become

$$\left.\begin{aligned} \frac{d\,\delta x}{dx}=\left(\frac{1}{9\mu}-\frac{1}{3m}\right)(p_1+p_2+p_3)+c_3 v+\frac{1}{m}p_1 \\ \frac{d\,\delta y}{dy}=\left(\frac{1}{9\mu}-\frac{1}{3m}\right)(p_1+p_2+p_3)+c_3 v+\frac{1}{m}p_2 \\ \frac{d\,\delta x}{dz}=\left(\frac{1}{9\mu}-\frac{1}{3m}\right)(p_1+p_2+p_3)+c_3 v+\frac{1}{m}p_3 \end{aligned}\right\} \quad . \quad . \quad . \quad (13.)$$

$c_3 v$ being the linear expansion for the temperature v.

Having found the general equations of the equilibrium of elastic solids, I proceed to work some examples of their application, which afford the means of determining the coefficients μ, m, and ω, and of calculating the stiffness of solid figures. I begin with those cases in which the elastic solid is a hollow cylinder exposed to given forces on the two concentric cylindric surfaces, and the two parallel terminating planes.

In these cases the co-ordinates x, y, z are replaced by the co-ordinates

$x=\mathrm{X}$, measured along the axis of the cylinder.

$y=r$, the radius of any point, or the distance from the axis.

$z=r\theta$, the arc of a circle measured from a fixed plane passing through the axis.

$\frac{d\delta x}{dx}=\frac{d\delta x}{dx}$, $p_1=0$, are the compression and pressure in the direction of the axis at any point.

$\frac{d\delta y}{dy}=\frac{d\delta r}{dr}$, $p_2=p$, are the compression and pressure in the direction of the radius.

$\frac{d\delta z}{dz}=\frac{d\delta r\theta}{dr\theta}=\frac{\delta r}{r}$, $p_3=q$, are the compression and pressure in the direction of the tangent.

Equations (9.) become, when expressed in terms of these co-ordinates—

$$\left.\begin{aligned} q_1 &= \frac{m}{2} r \frac{d\delta\theta}{dr} \\ q_2 &= \frac{m}{2} r \frac{d\delta\theta}{dx} \\ q_3 &= \frac{m}{2} \cdot \frac{d\delta x}{dr} \end{aligned}\right\} \quad . \quad (14.)$$

The length of the cylinder is b, and the two radii a_1 and a_2 in every case.

Case I.

The first equation is applicable to the case of a hollow cylinder, of which the outer surface is fixed, while the inner surface is made to turn through a small angle $\delta\theta$, by a couple whose moment is M.

The twisting force M is resisted only by the elasticity of the solid, and therefore the whole resistance, in every concentric cylindric surface, must be equal to M.

The resistance at any point, multiplied into the radius at which it acts, is expressed by $r q_1 = \frac{m}{2} r^2 \frac{d\delta\theta}{dr}$.

Therefore for the whole cylindric surface

$$\frac{d\delta\theta}{dr} m \pi r^3 b = \mathrm{M}.$$

Whence
$$\delta\theta = \frac{\mathrm{M}}{2\pi m b}\left(\frac{1}{a_1^2} - \frac{1}{a_2^2}\right)$$

and
$$m = \frac{\mathrm{M}}{2\pi b \delta\theta}\left(\frac{1}{a_1^2} - \frac{1}{a_2^2}\right) \quad . \quad . \quad (15.)$$

The optical effect of the pressure of any point is expressed by

$$\mathrm{I} = \omega q_1 b = \omega \frac{\mathrm{M} b}{\pi r^2} \quad . \quad . \quad (16.)$$

Therefore, if the solid be viewed by polarized light (transmitted parallel to the axis), the difference of retardation of the oppositely polarized rays at any point in the solid will be inversely proportional to the square of the distance from the axis of the cylinder, and the planes of polarization of these rays will be inclined 45° to the radius at that point.

The general appearance is therefore a system of coloured rings arranged oppositely to the rings in uniaxal crystals, the tints ascending in the scale as they approach the centre, and the distance between the rings decreasing towards the centre. The whole system is crossed by two dark bands inclined 45° to the plane of primitive polarization, when the plane of the analysing plate is perpendicular to that of the first polarizing plate.

A jelly of isinglass poured when hot between two concentric cylinders forms, when cold, a convenient solid for this experiment; and the diameters of the rings may be varied at pleasure by changing the force of torsion applied to the interior cylinder.

By continuing the force of torsion while the jelly is allowed to dry, a hard plate of isinglass is obtained, which still acts in the same way on polarized light, even when the force of torsion is removed.

It seems that this action cannot be accounted for by supposing the interior parts kept in a state of constraint by the exterior parts, as in unannealed and heated glass; for the optical properties of the plate of isinglass are such as would indicate a strain preserving in every part of the plate the direction of the original strain, so that the strain on one part of the plate cannot be maintained by an opposite strain on another part.

Two other uncrystallised substances have the power of retaining the polarizing structure developed by compression. The first is a mixture of wax and resin pressed into a thin plate between two plates of glass, as described by Sir David Brewster, in the *Philosophical Transactions* for 1815 and 1830.

When a compressed plate of this substance is examined with polarized light, it is observed to have no action on light at a perpendicular incidence; but when inclined, it shews the segments of coloured rings. This property does not belong to the plate as a whole, but is possessed by every part of it. It is therefore similar to a plate cut from a uniaxal crystal perpendicular to the axis.

I find that its action on light is like that of a *positive* crystal, while that of a plate of isinglass similarly treated would be *negative*.

The other substance which possesses similar properties is gutta percha. This substance in its ordinary state, when cold, is not transparent even in thin films; but if a thin film be drawn out gradually, it may be extended to more than double its length. It then possesses a powerful double refraction, which it retains so strongly that it has been used for polarizing light.* As one of its refractive in-

* By Dr Wright, I believe.

dices is nearly the same as that of Canada balsam, while the other is very different, the common surface of the gutta percha and Canada balsam will transmit one set of rays much more readily than the other, so that a film of extended gutta percha placed between two layers of Canada balsam acts like a plate of nitre treated in the same way. That these films are in a state of constraint may be proved by heating them slightly, when they recover their original dimensions.

As all these permanently compressed substances have passed their limit of perfect elasticity, they do not belong to the class of elastic solids treated of in this paper; and as I cannot explain the method by which an uncrystallised body maintains itself in a state of constraint, I go on to the next case of twisting, which has more practical importance than any other. This is the case of a cylinder fixed at one end, and twisted at the other by a couple whose moment is M.

Case II.

In this case let $\delta\theta$ be the angle of torsion at any point, then the resistance to torsion in any circular section of the cylinder is equal to the twisting force M.

The resistance at any point in the circular section is given by the second Equation of (14.)

$$q_2 = \frac{m}{2}\, r\, \frac{d\,\delta\theta}{dx}.$$

This force acts at the distance r from the axis; therefore its resistance to torsion will be $q_2 r$, and the resistance in a circular annulus will be

$$q_2 r\, 2\pi r\, dr = m\pi r^3 \frac{d\,\delta\theta}{dx}\, dr$$

and the whole resistance for the hollow cylinder will be expressed by

$$\mathrm{M} = \frac{m\pi}{4}\frac{d\,\delta\theta}{dx}(a_1^4 - a_2^4) \quad . \quad . \quad (16.)$$

$$m = 4\,\mathrm{M}\,\frac{1}{\pi\,\frac{\delta\theta}{b}(a_1^4 - a_2^4)}$$

$$m = \frac{720}{\pi^2}\,\frac{\mathrm{M}}{n}\left(\frac{b}{a_1^4 - a_2^4}\right) \quad . \quad . \quad (17.)$$

In this equation, m is the coefficient of linear elasticity; a_1 and a_2 are the radii of the exterior and interior surfaces of the hollow cylinder in inches; M is the moment of torsion produced by a weight acting on a lever, and is expressed by the product of the number of pounds in the weight into the number of inches in the lever; b is the distance of two points on the cylinder whose angular motion is measured by means of indices, or more accurately by small mirrors attached to

the cylinder; n is the difference of the angle of rotation of the two indices in degrees.

This is the most accurate method for the determination of m independently of μ, and it seems to answer best with thick cylinders which cannot be used with the balance of torsion, as the oscillations are too short, and produce a vibration of the whole apparatus.

Case III.

A hollow cylinder exposed to normal pressures only. When the pressures parallel to the axis, radius, and tangent are substituted for p_1, p_2, and p_3, Equations (10) become

$$\frac{d\,\delta x}{dx} = \left(\frac{1}{9\mu} - \frac{1}{3m}\right)(o+p+q) + \frac{1}{m}\,o \quad . \quad . \quad (18.)$$

$$\frac{d\,\delta r}{dr} = \left(\frac{1}{9\mu} - \frac{1}{3m}\right)(o+p+q) + \frac{1}{m}\,p \quad . \quad . \quad (19.)$$

$$\frac{d\,\delta(r\theta)}{d(r\theta)} = \frac{\delta r}{r} = \left(\frac{1}{9\mu} - \frac{1}{3m}\right)(o+p+q) + \frac{1}{m}\,q \quad . \quad . \quad (20.)$$

By multiplying Equation (20) by r, differentiating with respect to r, and comparing this value of $\frac{d\,\delta r}{dr}$ with that of Equation (19.)

$$\frac{p-q}{rm} = \left(\frac{1}{9\mu} - \frac{1}{3m}\right)\left(\frac{do}{dr} + \frac{dp}{dr} + \frac{dq}{dr}\right) - \frac{1}{m}\,\frac{dq}{dr}$$

The equation of the equilibrium of an element of the solid is obtained by considering the forces which act on it in the direction of the radius. By equating the forces which press it outwards with those pressing it inwards, we find the equation of the equilibrium of the element,

$$\frac{q-p}{r} = \frac{dp}{dr} \quad . \quad . \quad . \quad . \quad (21.)$$

By comparing this equation with the last, we find

$$\left(\frac{1}{9\mu} - \frac{1}{3m}\right)\frac{do}{dr} + \left(\frac{1}{9\mu} + \frac{2}{3m}\right)\left(\frac{dp}{dr} + \frac{dq}{dr}\right) = 0$$

Integrating,

$$\left(\frac{1}{9\mu} - \frac{1}{3m}\right)o + \left(\frac{1}{9\mu} + \frac{2}{3m}\right)(p+q) = C_1$$

Since o, the longitudinal pressure, is supposed constant, we may assume

$$c_2 = \frac{c_1 - \left(\frac{1}{9\mu} - \frac{1}{3m}\right)0}{\frac{1}{9\mu} + \frac{2}{3m}} = (p+q)$$

$$\therefore \quad q - p = c_2 - 2p, \quad \text{therefore by (21.)}$$

$$\frac{dp}{dr} + \frac{2p}{r} = \frac{c_2}{r}$$

a linear equation, which gives

$$p = c_3 \frac{1}{r_2} + \frac{c_2}{2}$$

The coefficients c_2 and c_3 must be found from the conditions of the surface of the solid. If the pressure on the exterior cylindric surface whose radius is a_1 be denoted by h_1, and that on the interior surface whose radius is a_2 by h_2,

$$\text{then } p = h_1 \text{ when } r = a_1$$
$$\text{and } p = h_2 \text{ when } r = a_2$$

and the general value of p is

$$p = \frac{a_1^2 h_1 - a_2^2 h_2}{a_1^2 - a_2^2} - \frac{a_1^2 a_2^2}{r^2} \frac{h_1 - h_2}{a_1^2 - a_2^2} \quad . \quad . \quad . \quad (22.)$$

$$r \frac{dp}{dr} = q - p = 2 \frac{a_1^2 a_2^2}{r^2} \frac{h_1 - h_2}{a_1^2 - a_1^2} \quad \text{by (21.)}$$

$$q = \frac{a_1^2 h_1 - a_2^2 h}{a_1^2 - a_2^2} + \frac{a_1^2 a_2^2}{r^2} \frac{h_1 - h_2}{a_1^2 - a_2^2} \quad . \quad . \quad . \quad (23.)$$

$$\mathrm{I} = b\omega (p - q) = b\omega \frac{a_1^2 a_2^2}{r^2} \frac{h_1 - h_2}{a_1^2 - a_2^2} \quad . \quad . \quad . \quad (24.)$$

This last equation gives the optical effect of the pressure at any point. The law of the magnitude of this quantity is the inverse square of the radius, as in Case I.; but the direction of the principal axes is different, as in this case they are parallel and perpendicular to the radius. The dark bands seen by polarized light will therefore be parallel and perpendicular to the plane of polarization, instead of being inclined at an angle of 45°, as in Case I.

By substituting in Equations (18.) and (20.), the values of p and q given in (22.) and (23.), we find that when $r = a_1$,

$$\left.\begin{aligned} \frac{\delta x}{x} &= \left(\frac{1}{9\mu}\right)\left(o + 2\frac{a_1^2 h_1 - a_2^2 h^2}{a_1^2 - a_2^2}\right) + \frac{2}{3m}\left(o - \frac{a_1^2 h_1 - a_2^2 h_2}{a_1^2 - a_2^2}\right) \\ &= o\left(\frac{1}{9\mu} + \frac{2}{3m}\right) + 2(h_1 a_1^2 - h_1 a_2^2)\frac{1}{a_1^2 - a_2^2}\left(\frac{1}{9\mu} - \frac{1}{3m}\right) \end{aligned}\right\} \quad (25.)$$

$$\left.\begin{aligned} \text{When } r = a_1, \frac{\delta r}{r} &= \frac{1}{9\mu}\left(o + 2\frac{a_1^2 h_1 - a_2^2 h_2}{a_1^2 - a_2^2}\right) + \frac{1}{3m}\left(\frac{a_1^2 h_1 + 3a_2^2 h_1 - 4a_2^2 h_2}{a_1^2 - a_2^2} - o\right) \\ &= o\left(\frac{1}{9\mu} - \frac{1}{3m}\right) + h_1 \frac{1}{a_1^2 - a_2^2}\left(\frac{2a_1}{9\mu} + \frac{a_1^2 + 3a_2^2}{3m}\right) + h_2 \frac{a_2}{2a_1 - a_2}\left(\frac{2}{9\mu} - \frac{4}{3m}\right) \end{aligned}\right\} \quad (26.)$$

From these equations it appears that the longitudinal compression of cylindric tubes is proportional to the longitudinal pressure referred to unit of surface when the lateral pressures are constant, so that for a given pressure the compression is inversely as the sectional area of the tube.

These equations may be simplified in the following cases:—

1. When the external and internal pressures are equal, or $h_1 = h_2$.

2. When the external pressure is to the internal pressure as the square of the interior diameter is to that of the exterior diameter, or when $a_1^2 h_1 = a_2^2 h_2$.

3. When the cylinder is solid, or when $a_2 = 0$.

4. When the solid becomes an indefinitely extended plate with a cylindric hole in it, or when a_2 becomes infinite.

5. When pressure is applied only at the plane surfaces of the solid cylinder, and the cylindric surface is prevented from expanding by being inclosed in a strong case, or when $\frac{\delta r}{r} = 0$.

6. When pressure is applied to the cylindric surface, and the ends are retained at an invariable distance, or when $\frac{\delta x}{x} = 0$.

1. When $h_1 = h_2$, the equations of compression become

$$\left.\begin{aligned} \frac{\delta x}{x} &= \frac{1}{9\mu}(o + 2h_1) + \frac{2}{3m}(o - h_1) \\ o &= \left(\frac{1}{9\mu} + \frac{2}{3m}\right) + 2h_1\left(\frac{1}{9\mu} - \frac{1}{3m}\right) \\ \frac{\delta r}{r} &= \frac{1}{9\mu}(o + 2h_1) + \frac{1}{3m}(h_1 - o) \\ o &= \left(\frac{1}{9\mu} - \frac{1}{3m}\right) + h_1\left(\frac{2}{9\mu} + \frac{1}{3m}\right) \end{aligned}\right\} \quad . \quad . \quad (27.)$$

When $h_1 = h_2 = o$, then

$$\frac{\delta x}{x} = \frac{\delta r}{r} = \frac{h_1}{3\mu}.$$

The compression of a cylindrical vessel exposed on all sides to the same hydrostatic pressure is therefore independent of m, and it may be shewn that the same is true for a vessel of any shape.

2. When $a_1^2 h_1 = a_2^2 h_2$,

$$\left.\begin{aligned} \frac{\delta x}{x} &= o\left(\frac{1}{9\mu} + \frac{2}{3m}\right) \\ \frac{\delta r}{r} &= \frac{1}{9\mu}(o) + \frac{1}{3m}(3h_1 - o) \\ &= o\left(\frac{1}{9\mu} - \frac{1}{3m}\right) + \frac{h_1}{m} \end{aligned}\right\} \quad . \quad . \quad (28.)$$

In this case, when $o = 0$, the compressions are independent of μ.

3. In a solid cylinder, $a_2 = 0$,

$$p = q = h_1.$$

The expressions for $\frac{\delta x}{x}$ and $\frac{\delta r}{r}$ are the same as those in the first case, when $h_1 = h_2$.

When the longitudinal pressure o vanishes,

$$\frac{\delta x}{x} = 2h_1\left(\frac{1}{9\mu}-\frac{1}{3m}\right)$$

$$\frac{\delta r}{r} = h_1\left(\frac{2}{9\mu}+\frac{1}{3m}\right)$$

When the cylinder is pressed on the plane sides only,

$$\frac{\delta x}{x} = o\left(\frac{1}{9\mu}+\frac{2}{3m}\right)$$

$$\frac{\delta r}{r} = o\left(\frac{1}{9\mu}-\frac{1}{3m}\right)$$

4. When the solid is infinite, or when a_2 is infinite,

$$\left.\begin{aligned}
p &= h_2 + \frac{1}{r^2}a_1^2(h_1-h_2)\\
q &= h_2 - \frac{1}{r^2}a_1^2(h_1-h_2)\\
\mathrm{I} &= \omega(q-p) = -\frac{2\omega}{r^2}a_1^2(h_1-h_2)\\
\frac{dx}{x} &= \frac{1}{9\mu}(o+2h_2)+\frac{2}{3m}(o-h_2)\\
&= o\left(\frac{1}{9\mu}+\frac{2}{3m}\right)+2h_2\left(\frac{1}{9\mu}-\frac{1}{3m}\right)\\
\frac{dr}{r} &= \frac{1}{9\mu}(o+2h_2)+\frac{1}{3m}(4h_2-3h_2-o)\\
&= o\left(\frac{1}{9\mu}-\frac{1}{3m}\right)+2h_2\left(\frac{1}{9\mu}+\frac{2}{3m}\right)-\frac{h_1}{m}
\end{aligned}\right\} \quad . \; . \; (29.)$$

5. When $\delta r = 0$ in a solid cylinder,

$$\left.\begin{aligned}
\frac{\delta x}{x} &= \frac{3o}{2m+3\mu}\\
\text{6. When}\quad \frac{\delta x}{x} &= o \qquad \frac{\delta r}{r} = \frac{3h}{m+6\mu}
\end{aligned}\right\} \quad . \; . \; (30.)$$

Since the expression for the effect of a longitudinal strain is

$$\frac{\delta x}{x} = o\left(\frac{1}{9\mu}+\frac{2}{3m}\right)$$

if we make $\mathrm{E} = \dfrac{9m\mu}{m+6\mu}$, then $\dfrac{\delta x}{x} = o\dfrac{1}{\mathrm{E}}$ . . . (31.)

The quantity E may be deduced from experiment on the extension of wires or rods of the substance, and μ is given in terms of m and E by the equation,

$$\mu = \frac{\mathrm{E}m}{9m-6\mathrm{E}} \quad . \; . \; . \; (32.)$$

and

$$\mathrm{E} = \frac{\mathrm{P}b}{s\,\delta x} \quad . \; . \; . \; . \; (33.)$$

P being the extending force, b the length of the rod, s the sectional area, and δx the elongation, which may be determined by the deflection of a wire, as in the apparatus of S' GRAVESANDE, or by direct measurement.

CASE IV.

The only known direct method of finding the compressibility of liquids is that employed by CANTON, ŒRSTED, PERKINS, AIMÉ, &c.

The liquid is confined in a vessel with a narrow neck, then pressure is applied, and the descent of the liquid in the tube is observed, so that the difference between the change of volume of the liquid and the change of internal capacity of the vessel may be determined.

Now, since the substance of which the vessel is formed is compressible, a change of the internal capacity is possible. If the pressure be applied only to the contained liquid, it is evident that the vessel will be distended, and the compressibility of the liquid will appear too great. The pressure, therefore, is commonly applied externally and internally at the same time, by means of a hydrostatic pressure produced by water compressed either in a strong vessel or in the depths of the sea.

As it does not necessarily follow, from the equality of the external and internal pressures, that the capacity does not change, the equilibrium of the vessel must be determined theoretically. ŒRSTED, therefore, obtained from POISSON his solution of the problem, and applied it to the case of a vessel of lead. To find the cubical elasticity of lead, he applied the theory of POISSON to the numerical results of TREDGOLD. As the compressibility of lead thus found was greater than that of water, ŒRSTED expected that the apparent compressibility of water in a lead vessel would be *negative*. On making the experiment the apparent compressibility was *greater* in lead than in glass. The quantity found by TREDGOLD from the extension of rods was that denoted by E, and the value of μ deduced from E alone by the formulæ of POISSON cannot be true, unless $\frac{\mu}{m}=\frac{5}{6}$; and as $\frac{\mu}{m}$ for lead is probably more than 3, the calculated compressibility is much too great.

A similar experiment was made by Professor FORBES, who used a vessel of caoutchouc. As in this case the apparent compressibility vanishes, it appears that the cubical compressibility of caoutchouc is equal to that of water.

Some who reject the mathematical theories as unsatisfactory, have conjectured that if the sides of the vessel be sufficiently thin, the pressure on both sides being equal, the compressibility of the vessel will not affect the result. The following calculations shew that the apparent compressibility of the liquid depends on the compressibility of the vessel, and is independent of the thickness when the pressures are equal.

A hollow sphere, whose external and internal radii are a_1 and a_2, is acted on

by external and internal normal pressures h_1 and h_2, it is required to determine the equilibrium of the elastic solid.

The pressures at any point in the solid are:—

1. A pressure p in the direction of the radius;
2. A pressure q in the perpendicular plane.

These pressures depend on the distance from the centre, which is denoted by r.

The compressions at any point are $\frac{d\,\delta r}{dr}$ in the radial direction, and $\frac{\delta r}{r}$ in the tangent plane, the values of these compressions are:—

$$\frac{d\,\delta r}{dr} = \left(\frac{1}{9\mu} - \frac{1}{3m}\right)(p+2q) + \frac{1}{m}p \quad . \quad . \quad . \quad (34.)$$

$$\frac{\delta r}{r} = \left(\frac{1}{9\mu} - \frac{1}{3m}\right)(p+2q) + \frac{1}{m}q \quad . \quad . \quad . \quad (35.)$$

Multiplying the last equation by r, differentiating with respect to r, and equating the result with that of the first equation, we find

$$r\left(\frac{1}{9\mu} - \frac{1}{3m}\right)\left(\frac{dp}{dr} + 2\frac{dq}{dr}\right) + \frac{1}{m}\left(r\frac{dq}{dr} + q - p\right) = 0$$

Since the forces which act on the particle in the direction of the radius must balance one another, or $2q\,dr\,d\theta + p\,(r\,d\theta)^2 = \left(p + \frac{dp}{dr}dr\right)(r+dr)^2\,\theta$

$$\therefore \qquad q - p = \frac{r}{2}\frac{dp}{dr} \quad . \quad . \quad . \quad (36.)$$

Substituting this value of $q-p$ in the preceding equation, and reducing,

$$\therefore \qquad \frac{dp}{dr} + 2\frac{dq}{dr} = 0$$

Integrating, $\qquad p + 2q = c_1$

But $\qquad q = \frac{r}{2}\frac{dp}{dr} + p$, and the equation becomes

$$\frac{dp}{dr} + 3\frac{p}{r} + \frac{c_1}{r} = 0$$

$$\therefore \qquad p = c_2\frac{1}{r^3} + \frac{c_1}{3}$$

Since $p = h_1$ when $r = a_1$, and $p = h_2$ when $r = a_2$, the value of p at any time is found to be

$$p = \frac{a_1^3 h_1 - a_2^3 h_2}{a_1^3 - a_2^3} - \frac{a_1^3 a_2^3}{r^3}\frac{h_1 - h_2}{a^3 - b^3} \quad . \quad . \quad . \quad . \quad (37.)$$

$$q = \frac{a_1^3 h_1 - a_2^3 h_2}{a_1^3 - a_2^3} - \frac{a_1^3 a_2^3}{r^3}\frac{h_1 - h_2}{a_1^3 - a_2^3} \quad . \quad . \quad (38.)$$

$$\frac{\delta V}{V} = 3\frac{\delta r}{r} = \frac{a_1^3 h_1 - a_2^3 h_2}{a_1^3 - a_2^3}\frac{1}{\mu} + \frac{3}{2}\frac{a_1^3 a_2^3}{r^3}\frac{h_1 - h_2}{a_1^3 - a_2^3}\frac{1}{m}$$

$$\text{When } r=a_1 \quad \frac{\delta V}{V} = \frac{a_2^3 h_1 - a_2^3 h_2}{a_1^3 - a_2^3}\frac{1}{\mu} + \frac{3}{2}a_2^3\frac{h_1-h_2}{a_1^3-a_2^3}\frac{1}{m}$$
$$= \frac{h_1}{a_1^3-a_2^3}\left(\frac{a_1^3}{\mu}+\frac{3a_2^3}{2m}\right) - \frac{h_2 a_2^3}{a_1^3-a_2^3}\left(\frac{1}{\mu}+\frac{3}{2m}\right) \quad . \quad . \quad (39.)$$

When the external and internal pressures are equal

$$h_1=h_2=p=q, \text{ and } \frac{\delta V}{V}=\frac{h_1}{u}=\frac{h_1}{u} \quad . \quad . \quad (40.)$$

the change of internal capacity depends entirely on the cubical elasticity of the vessel, and not on its thickness or its linear elasticity.

When the external and internal pressures are inversely as the cubes of the radii of the surfaces on which they act,

$$a_1^3 h_1 = a_2^3 h_2, \; p=\frac{a^3}{r^3}h_1, \; q=-\frac{1}{2}\frac{a_1^3}{r^3}h_1$$
$$\frac{\delta V}{V} = -\frac{3}{2}\frac{a_1^3}{r^3}\frac{h_1}{m} \quad . \quad . \quad (41.)$$
$$\text{when} \qquad r=a_1 \quad \frac{\delta V}{V} = -\frac{3}{2}\frac{h_1}{m}$$

In this case the change of capacity depends on the linear elasticity alone.

M. Regnault, in his researches on the theory of the steam engine, has given an account of the experiments which he made in order to determine with accuracy the compressibility of mercury.

He considers the mathematical formulæ very uncertain, because the theories of molecular forces from which they are deduced are probably far from the truth; and even were the equations free from error, there would be much uncertainty in the ordinary method by measuring the elongation of a rod of the substance, for it is difficult to ensure that the material of the rod is the same as that of the hollow sphere.

He has, therefore, availed himself of the results of M. Lamé for a hollow sphere in three different cases, in the first of which the pressure acts on the interior and exterior surface at the same time, while in the other two cases the pressure is applied to the exterior or interior surface alone. Equation (39.) becomes in these cases,—

1. When $h_1=h_2 \quad \frac{\delta V}{V}=\frac{h_1}{\mu}$ and the compressibility of the enclosed liquid being μ_2, and the apparent diminution of volume $\delta' V, \quad \frac{\delta' V}{V} = h_1\left(\frac{1}{\mu_2}-\frac{1}{\mu}\right) \quad . \quad . \quad (42.)$

2. When $\quad h_1=o, \quad \frac{\delta V}{V}=\frac{\delta' V}{V}=-h_2\frac{a_2^3}{a_1^3-a_2^3}\left(\frac{1}{\mu}+\frac{3}{2m}\right) \quad . \quad . \quad . \quad . \quad (43.)$

3. When $\quad h_2=o, \quad \frac{\delta V}{V}=\frac{h_1}{a_1^3-a_2^3}\left(\frac{a_1^3}{\mu}+\frac{3}{2}\frac{a_2^3}{m}\right)$

$$\frac{\delta' V}{V} = \frac{h_1}{a_1^3-a_2^3}\left(\frac{a_1^3}{\mu}+\frac{3}{2}\frac{a_2^3}{m}+(a_2^3-a_1^3)\frac{1}{\mu_2}\right)$$

M. LAMÉ's equations differ from these only in assuming that $\mu = \frac{5}{6}m$. If this assumption be correct, then the coefficients μ, m, and μ_2, may be found from two of these equations; but since one of these equations may be derived from the other two, the *three* coefficients cannot be found when μ is supposed independent of m. In Equations (39.), the quantities which may be varied at pleasure are h_1 and h_2, and the quantities which may be deduced from the apparent compressions are,

$$c_1 = \left(\frac{1}{\mu} + \frac{3}{2m}\right) \text{ and } \left(\frac{1}{\mu} - \frac{1}{\mu_2}\right) = c_2$$

therefore some independent equation between these quantities must be found, and this cannot be done by means of the sphere alone; some other experiment must be made on the liquid, or on another portion of the substance of which the vessel is made.

The value of μ_2, the elasticity of the liquid, may be previously known.

The linear elasticity m of the vessel may be found by twisting a rod of the material of which it is made;

Or, the value of E may be found by the elongation or bending of the rod, and $\frac{1}{E} = \frac{1}{9\mu} + \frac{2}{3m}$.

We have here five quantities, which may be determined by experiment.

(43.)	1.	$c_1 = \left(\frac{1}{\mu} + \frac{3}{2m}\right)$ by external pressure	on sphere.
(42.)	2.	$c_2 = \left(\frac{1}{\mu} - \frac{1}{\mu_2}\right)$ equal pressures	on sphere.
(31.)	3.	$\frac{1}{E} = \left(\frac{1}{9\mu} + \frac{2}{3m}\right)$	by elongation of a rod.
(17.)	4.	m	by twisting the rod.
	5.	μ_2	the elasticity of the liquid.

When the elastic sphere is solid, the internal radius a_1 vanishes, and $h_2 = p = q$, and $\frac{\delta V}{V} = \frac{h_2}{\mu}$.

When the case becomes that of a spherical cavity in an infinite solid, the external radius a_2 becomes infinite, and

$$\left.\begin{aligned} p &= h_2 + \frac{a_1^3}{r^3}(h_1 - h_2) \\ q &= h_2 - \frac{1}{2}\frac{a_1^3}{r^3}(h_1 - h_2) \\ \frac{\delta r}{r} &= h_2 \frac{1}{3\mu} - \frac{1}{2}\frac{a_1^3}{r^3}(h_1 - h_2)\frac{1}{m} \\ \frac{\delta V}{V} &= \frac{h_2}{\mu} - \frac{1}{2}\frac{h_1 - h_2}{m} \end{aligned}\right\} \quad . \quad . \quad . \quad (46.)$$

The effect of pressure on the surface of a spherical cavity on any other part of an elastic solid is therefore inversely proportional to the cube of its distance from the centre of the cavity.

When one of the surfaces of an elastic hollow sphere has its radius rendered invariable by the support of an incompressible sphere, whose radius is a_1, then

$$\frac{\delta r}{r}=0, \quad \text{when } r=a_1$$

$$\left.\begin{aligned} \therefore \quad p &= h_2 \frac{3a_2^3\mu}{2a_1^3 m+3a_2^3\mu}+h_2\frac{a_1^3 a_2^3}{r^3}\frac{2m}{2a_1^3 m+3a_2^3\mu} \\ q &= h_2 \frac{3a_2^3\mu}{2a_1^3 m+3a_2^3\mu}-h_2\frac{a_1^3 a_2^3}{r^3}\frac{m}{2a_1^3 m+3a_2^3\mu} \\ \frac{\delta r}{r} &= h_2 \frac{a_2^3}{2a_1^3 m+3a_2^3\mu}-h_2\frac{a_1^3 a_2^3}{r^3}\frac{1}{2a_1^3 m+3a_2^3\mu} \end{aligned}\right\} \quad . \ . \ . \quad (45.)$$

When $r=a_2$ $\dfrac{\delta V}{V}=h_2\dfrac{3a_2^3-3a_1^3}{2a_1^3 m+3a_2^3\mu}$

Case V.

On the equilibrium of an elastic beam of rectangular section uniformly bent.

By supposing the bent beam to be produced till it returns into itself, we may treat it as a hollow cylinder.

Let a rectangular elastic beam, whose length is $2\pi c$, be bent into a circular form, so as to be a section of a hollow cylinder, those parts of the beam which lie towards the centre of the circle will be longitudinally compressed, while the opposite parts will be extended.

The expression for the tangential compression is therefore

$$\frac{\delta r}{r}=\frac{c-r}{c}.$$

Comparing this value of $\dfrac{\delta r}{r}$ with that of Equation (20.)

$$\frac{c-r}{r}=\left(\frac{1}{9\mu}-\frac{1}{3m}\right)(o+p+q)+\frac{q}{m}$$

and by (21.) $$q=p+r\frac{dp}{dr}.$$

By substituting for q its value, and dividing by $r\left(\dfrac{1}{9\mu}+\dfrac{1}{3m}\right)$, the equation becomes

$$\frac{dp}{dr}+\frac{2m+3\mu}{m+6\mu}\frac{p}{r}=\frac{9m\mu-(m-3\mu)o}{(m+6\mu)r}-\frac{9m\mu}{(m+6\mu)c}$$

a linear differential equation, which gives

$$p=C_1 r^{-\frac{2m+3\mu}{m+6\mu}}-\frac{3m\mu}{m+3\mu}\frac{r}{c}+\frac{9\mu m-(m-3\mu)o}{2m+3\mu} \quad . \ . \quad (46.)$$

C_1 may be found by assuming that when $r=a_1$ $p=h_1$, and q may be found from p by Equation (21.)

As the expressions thus found are long and cumbrous, it is better to use the following approximations:—

$$q=-\left(\frac{9\,m\,\mu}{m+6\,\mu}\right)\frac{y}{c} \quad . \quad . \quad . \quad . \quad . \quad . \quad (47.)$$

$$p=\left(\frac{9\,m\,\mu}{m+6\,\mu}\right)\frac{1}{2\,c}\left(\frac{c^2-a^2}{y-c}+c+y\right) \quad . \quad (48.)$$

In these expressions a is half the depth of the beam, and y is the distance of any part of the beam from the neutral surface, which in this case is a cylindric surface, whose radius is c.

These expressions suppose c to be large compared with a, since most substances break when $\frac{a}{c}$ exceeds a certain small quantity.

Let b be the breadth of the beam, then the force with which the beam resists flexure $=\mathrm{M}$.

$$\mathrm{M}=\int b\,y\,q=\frac{q\,m\,\mu}{m+6\,\mu}\frac{b}{c}\frac{a^3}{3}=\mathrm{E}\frac{a^3\,b}{3\,c} \quad . \quad . \quad . \quad (49.)$$

which is the ordinary expression for the stiffness of a rectangular beam.

The stiffness of a beam of any section, the form of which is expressed by an equation between x and y, the axis of x being perpendicular to the plane of flexure, or the osculating plane of the axis of the beam at any point, is expressed by

$$\mathrm{M}\,c=\mathrm{E}\int y^2\,dx, \quad . \quad . \quad . \quad (50.)$$

M being the moment of the force which bends the beam, and c the radius of the circle into which it is bent.

Case VI.

At the meeting of the British Association in 1839, Mr James Nasmyth described his method of making concave specula of silvered glass by bending.

A circular piece of silvered plate-glass was cemented to the opening of an iron vessel, from which the air was afterwards exhausted. The mirror then became concave, and the focal distance depended on the pressure of the air.

Buffon proposed to make burning-mirrors in this way, and to produce the partial vacuum by the combustion of the air in the vessel, which was to be effected by igniting sulphur in the interior of the vessel by means of a burning-glass. Although sulphur evidently would not answer for this purpose, phosphorus might; but the simplest way of removing the air is by means of the air-pump. The mirrors which were actually made by Buffon, were bent by means of a screw acting on the centre of the glass.

To find an expression for the curvature produced in a flat, circular, elastic plate, by the difference of the hydrostatic pressures which act on each side of it,—

Let t be the thickness of the plate, which must be small compared with its diameter.

Let the form of the middle surface of the plate, after the curvature is produced, be expressed by an equation between r, the distance of any point from the axis, or normal to the centre of the plate, and x the distance of the point from the plane in which the middle of the plate originally was, and let $ds = \sqrt{(dx)^2 + (dr)^2}$.

Let h_1 be the pressure on one side of the plate, and h_2 that on the other.

Let p and q be the pressures in the plane of the plate at any point, p acting in the direction of a tangent to the section of the plate by a plane passing through the axis, and q acting in the direction perpendicular to that plane.

By equating the forces which act on any particle in a direction parallel to the axis, we find

$$tp\frac{dr}{ds}\frac{dx}{ds} + tr\frac{dp}{ds}\frac{dx}{ds} + trp\frac{d^2x}{ds^2} + r(h_1 - h_2)\frac{dr}{ds} = 0$$

By making $p=0$ when $r=0$ in this equation,

$$p = -\frac{r}{2t}\frac{ds}{dx}(h_1 - h_2) \quad . \quad . \quad . \quad (51.)$$

The forces perpendicular to the axis are

$$tp\left(\frac{dr}{ds}\right)^2 + tr\frac{dp}{ds}\frac{dr}{ds} + trp\frac{d^2r}{ds^2} - (h_1 - h_2)r\frac{dx}{ds} - qt = 0$$

Substituting for p its value, the equation gives

$$q = -\frac{(h_1 - h_2)}{t}r\left(\frac{dr}{ds}\frac{dr}{dx} + \frac{dx}{ds}\right) + \frac{(h_1 - h_2)}{2t}r^2\left(\frac{dr}{dx}\frac{ds}{dx}\frac{d^2x}{ds^2} - \frac{ds}{dx}\frac{d^2r}{ds^2}\right) \quad . \quad . \quad . \quad (52.)$$

The equations of elasticity become

$$\frac{d\delta s}{ds} = \left(\frac{1}{9\mu} - \frac{1}{3m}\right)\left(p + q + \frac{h_1 + h_2}{2}\right) + \frac{p}{m}$$

$$\frac{\delta r}{r} = \left(\frac{1}{9\mu} - \frac{1}{3m}\right)\left(p + q + \frac{h_1 + h_2}{2}\right) + \frac{q}{m}$$

Differentiating $\frac{d\delta r}{dr} = \frac{d}{dr}\left(\frac{\delta r}{r}r\right)$, and in this case

$$\frac{d\delta r}{dr} = 1 - \frac{dr}{ds} + \frac{dr}{ds}\frac{d\delta s}{ds}$$

By a comparison of these values of $\frac{d\delta r}{ds}$

$$\left(1 - \frac{dr}{ds}\right)\left(\frac{1}{9\mu} - \frac{1}{3m}\right)\left(p + q + \frac{h_1 + h_2}{2}\right) + \frac{q}{m} + \frac{dr}{ds}\frac{p}{m} + r\left(\frac{1}{9\mu} - \frac{1}{3m}\right)\left(\frac{dp}{dr} + \frac{dq}{dr}\right)$$

$$+ \frac{r}{m}\frac{dq}{dr} + \frac{dr}{ds} - 1 = 0.$$

To obtain an expression for the curvature of the plate at the vertex, let a be the radius of curvature, then, as an approximation to the equation of the plate, let

$$x = \frac{r^2}{2a}.$$

By substituting the value of x in the values of p and q, and in the equation of elasticity, the approximate value of a is found to be

$$a = \frac{t}{h_1 - h_2} \frac{(h_1 + h_2)\left(\frac{1}{9\mu} - \frac{1}{3m}\right) - 2}{10\left(\frac{1}{9\mu} - \frac{1}{3m}\right) - \frac{9}{m}}$$

$$a = \frac{t}{h_1 - h_2} \frac{-18m\mu}{10m + 51\mu} + t\frac{h_1 + h_2}{h_1 - h_2} \frac{m - 3\mu}{10m + 51\mu} \quad . \quad . \quad . \quad (53.)$$

Since the focal distance of the mirror, or $\frac{a}{2}$, depends on the difference of pressures, a telescope on Mr NASMYTH's principle would act as an aneroid barometer, the focal distance varying inversely as the pressure of the atmosphere.

CASE VII.

To find the conditions of torsion of a cylinder composed of a great number of parallel wires bound together without adhering to one another.

Let x be the length of the cylinder, a its radius, r the radius at any point, $\delta\theta$ the angle of torsion, M the force producing torsion, δx the change of length, and P the longitudinal force. Each of the wires becomes a helix whose radius is r_1, its angular rotation $\delta\theta$, and its length along the axis $x - \delta\theta$.

Its length is therefore $\sqrt{(r\delta\theta)^2 + x\left(1 - \frac{\delta x}{x}\right)^2}$

and the tension is $= \mathrm{E}\left(1 - \sqrt{\left(1 - \frac{\delta x}{x}\right)^2 + r^2\left(\frac{\delta\theta}{x}\right)^2}\right)$

This force, resolved parallel to the axis, is

$$\frac{d}{d\theta}\frac{d}{dr}\mathrm{P} = \mathrm{E}\left(\frac{1}{\sqrt{\left(1 - \frac{\delta x}{x}\right)^2 + r^2\left(\frac{\delta\theta}{x}\right)^2}} - 1\right)$$

and since $\frac{\delta x}{x}$ and $r\frac{\delta\theta}{x}$ are small, we may assume

$$\frac{d}{d\theta}\frac{d}{dr}\mathrm{P} = \mathrm{E}\left(\frac{\delta x}{x} - \frac{r^2}{2}\left(\frac{\delta\theta}{x}\right)^2\right)$$

$$\mathrm{P} = \pi\mathrm{E}\left(r^2\frac{\delta x}{x} - \frac{r^4}{4}\left(\frac{\delta\theta}{x}\right)^2\right) \quad . \quad . \quad . \quad (54.)$$

The force, when resolved in the tangential direction, is approximately

$$\frac{1}{r^2}\frac{d}{d\theta}\frac{d}{dx}\mathrm{M} = \mathrm{E}\left(r\frac{\delta\theta}{x}\frac{\delta x}{x} - \frac{r^3}{2}\left(\frac{\delta\theta}{x}\right)^3\right)$$

$$\mathrm{M} = \pi\mathrm{E}\left(\frac{r^4}{2}\frac{\delta\theta}{x}\frac{\delta x}{x} - \frac{r^6}{6}\left(\frac{\delta\theta}{x}\right)^3\right) \quad . \quad . \quad . \quad (55.)$$

By eliminating $\frac{\delta x}{x}$ between (54.) and (55.) we have

$$M=\frac{r^2}{2}\frac{\delta\theta}{x}P-E\pi\frac{r^6}{24}\left(\frac{\delta\theta}{x}\right)^3 \quad . \quad . \quad . \quad (56.)$$

When $P=0$, M depends on the sixth power of the radius and the cube of the angle of torsion, when the cylinder is composed of separate filaments.

Since the force of torsion for a homogeneous cylinder depends on the fourth power of the radius and the first power of the angle of torsion, the torsion of a wire having a fibrous texture will depend on both these laws.

The parts of the force of torsion which depend on these two laws may be found by experiment, and thus the difference of the elasticities in the direction of the axis and in the perpendicular directions may be determined.

A calculation of the force of torsion, on this supposition, may be found in YOUNG'S *Mathematical Principles of Natural Philosophy;* and it is introduced here to account for the variations from the law of Case II., which may be observed in a twisted rod.

CASE VIII.

It is well known that grindstones and fly-wheels are often broken by the centrifugal force produced by their rapid rotation. I have therefore calculated the strains and pressure acting on an elastic cylinder revolving round its axis, and acted on by the centrifugal force alone.

The equation of the equilibrium of a particle (see Equation (21.)), becomes

$$q-p=r\frac{dp}{dr}-\frac{4\pi^2k}{gt^2}r^2;$$

where q and p are the tangential and radial pressures, k is the weight in pounds of a cubic inch of the substance, g is twice the height in inches that a body falls in a second, t is the time of revolution of the cylinder in seconds.

By substituting the value of g and $\frac{dq}{dr}$ in Equations (19.), (20.), and neglecting o,

$$0=\left(\frac{1}{9\mu}-\frac{1}{3m}\right)\left(3\frac{dp}{dr}-2\frac{4\pi^2k}{gt^2}r+r\frac{d^2p}{dr^2}\right)+\frac{1}{m}\left(3\frac{dp}{dr}-3\frac{4\pi^2k}{gt^2}r+r\frac{d^2p}{dr^2}\right)$$

which gives

$$\left.\begin{aligned} p&=c_1\frac{1}{r^2}+\frac{\pi^2k}{2gt^2}\left(2+\frac{E}{m}\right)r^2+c_2 \\ \therefore\quad q-p&=-c_1\frac{1}{r^2}+\frac{\pi^2k}{2gt}\left(-4+\frac{2E}{m}\right)r^2 \\ q&=-c_1\frac{1}{r^2}+\frac{\pi^2k}{2gt^2}\left(-2+\frac{3E}{m_3}\right)r^2+c_2 \quad . \end{aligned}\right\} \quad . \quad . \quad . \quad (57.)$$

If the radii of the surfaces of the hollow cylinder be a_1 and a_2, and the pressures acting on them h_1 and h_2, then the values of c_1 and c_2 are

$$\left.\begin{aligned} c_1 &= a_1^2\, a_2^2\, \frac{\pi^2 k}{2 g t^2}\left(2+\frac{\mathrm{E}}{m}\right) - a_1^2\, a_2^2\, \frac{h_1-h_2}{a_1^2-a_2^2} \\ c_2 &= \frac{a_1^2 h_1 - a_2^2 h_2}{a_1^2-a_2^2} - (a_1^2+a_2^2)\,\frac{\pi^2 k}{2 g t^2}\left(2+\frac{\mathrm{E}}{m}\right). \end{aligned}\right\} \quad . \quad . \quad . \quad (58.)$$

When $a_2=0$, as in the case of a solid cylinder, $c_1=0$, and

$$c_2 = h_1 - a_1^2\,\frac{\pi^2 k}{2 g t^2}\left(2+\frac{\mathrm{E}}{m}\right)$$

$$q = h_1 + \frac{\pi^2 k}{2 g t^2}\left\{2(r^2+a_1^2) + \frac{\mathrm{E}}{m}(3r^2-a_1^2)\right\} \quad . \quad . \quad . \quad . \quad (59.)$$

When $h_1=0$, and $r=a_1$,

$$q = \frac{\pi^2 k a^2}{g t^2}\left(\frac{\mathrm{E}}{m}-2\right) \quad . \quad . \quad . \quad . \quad (60.)$$

When q exceeds the tenacity of the substance in pounds per square inch, the cylinder will give way; and by making q equal to the number of pounds which a square inch of the substance will support, the velocity may be found at which the bursting of the cylinder will take place.

Since $\mathrm{I}=b\,\omega\,(q-p)=\frac{\pi^2 k\,\omega}{g t}\left(\frac{\mathrm{E}}{m}-2\right) b r^2$, a transparent revolving cylinder, when polarized light is transmitted parallel to the axis, will exhibit rings whose diameters are as the square roots of an arithmetical progression, and brushes parallel and perpendicular to the plane of polarization.

CASE IX.

A hollow cylinder or tube is surrounded by a medium of a constant temperature while a liquid of a different temperature is made to flow through it. The exterior and interior surfaces are thus kept each at a constant temperature till the transference of heat through the cylinder becomes uniform.

Let v be the temperature at any point, then when this quantity has reached its limit,

$$\frac{r\,d\,v}{d\,r} = c_1$$

$$v = c_1 \log r + c_2 \quad . \quad . \quad . \quad . \quad (61.)$$

Let the temperatures at the surfaces be θ_1 and θ_2, and the radii of the surfaces a_1 and a_2, then

$$c_1 = \frac{\theta_1-\theta_2}{\log a_1 - \log a_2} \quad c_2 = \frac{\log a_1\,\theta_2 - \log a_2\,\theta_1}{\log a_1 - \log a_2}$$

Let the coefficient of linear dilatation of the substance be c_3, then the proportional dilatation at any point will be expressed by $c_3\,v$, and the equations of elasticity (18.), (19.), (20.), become

$$\frac{d\,\delta\,x}{d\,x} = \left(\frac{1}{9\mu}-\frac{1}{3m}\right)(o+p+q) + \frac{o}{m} - c_3\,v$$

$$\frac{d\,\delta r}{d\,r} = \left(\frac{1}{9\,\mu} - \frac{1}{3\,m}\right)(o+p+q) + \frac{p}{m} - c_3\,v$$

$$\frac{\delta r}{r} = \left(\frac{1}{9\,\mu} - \frac{1}{3\,m}\right)(o+p+q) + \frac{q}{m} - c_3\,v$$

The equation of equilibrium is

$$q = p + r\frac{d\,p}{d\,r} \quad . \quad . \quad (21.)$$

and since the tube is supposed to be of a considerable length

$$\frac{d\,\delta x}{d\,x} = c_4 \text{ a constant quantity.}$$

From these equations we find that

$$o = \frac{c_4 + c_3\,v - \left(\frac{1}{9\,\mu} - \frac{1}{3\,m}\right)\left(2p + r\frac{d\,p}{d\,r}\right)}{\frac{1}{9\,\mu} + \frac{2}{3\,m}}$$

and hence $v = c_1 \log r + c_2$, p may be found in terms of r.

$$p = \left(\frac{2}{9\,\mu} + \frac{1}{3\,m}\right)^{-1} c_1\,c_3 \log r + c_5\frac{1}{r^2} + c_6$$

Hence $$q = \left(\frac{2}{9\,\mu} + \frac{1}{3\,m}\right)^{-1} c_1\,c_3 \log r - c_5\frac{1}{r^2} + c_6 + \left(\frac{2}{9\,\mu} + \frac{1}{3\,m}\right) c_1\,c_3$$

Since $$\mathrm{I} = b\,\omega\,(q+p) = b\,\omega\left(\frac{2}{9\,\mu} + \frac{1}{3\,m}\right)^{-1} c_1\,c_3 - 2\,b\,\omega\,c_5\frac{1}{r^2}$$

the rings seen in this case will differ from those described in Case III. only by the addition of a constant quantity.

When no pressures act on the exterior and interior surfaces of the tube $h_1 = h_2 = 0$, and

62.
$$\begin{cases} p = \left(\frac{2}{9\,\mu} + \frac{1}{3\,m}\right)^{-1} c_1\,c_3 \left\{ \log r + \frac{a_1^{\,2}\,a_2^{\,2}}{r^2}\frac{\log a_1 - \log a_2}{a_1^{\,2} - a_2^{\,2}} + \frac{a_1^{\,2}\log a_1 - a_2^{\,2}\log a_2}{a_1^{\,2} - a_2^{\,2}} \right\} \\ q = \left(\frac{2}{9\,\mu} + \frac{1}{3\,m}\right)^{-1} c_1\,c_3 \left\{ \log r - \frac{a_1^{\,2}\,a_2^{\,2}}{r^2}\frac{\log a_1 - \log a_2}{a_1 - a_2} + \frac{a_1^{\,2}\log a_1 - a_2^{\,2}\log a_2}{a_1^{\,2} - a_2^{\,2}} + 1 \right\} \\ \mathrm{I} = b\left(\frac{2}{9\,\mu} + \frac{1}{3\,m}\right)^{-1} c_1\,c_3\,\omega \left\{ 1 - 2\,\frac{a_1^{\,2}\,a_2^{\,2}}{r^2}\frac{\log a_1 - \log a_2}{a_1^{\,2} - a_2^{\,2}} \right\} \end{cases}$$

There will, therefore, be no action on polarized light for the ring whose radius is r when

$$r^2 = 2\,\frac{a_1^{\,2}\,a_2^{\,2}}{a_1^{\,2} - a_2^{\,2}} \log \frac{a_1}{a_2}$$

CASE X.

Sir DAVID BREWSTER has observed (*Edinburgh Transactions*, vol. viii.), that when a solid cylinder of glass is suddenly heated at the cylindric surface a polarizing force is developed, which is at any point proportional to the square of the distance from the axis of the cylinder; that is to say, that the difference of retarda-

tion of the oppositely polarized rays of light is proportional to the square of the radius r, or

$$I = b\, c_1\, \omega\, r^2 = b\, \omega\, (q-p) = b\, \omega\, r \frac{dp}{dr}$$

$$\therefore \frac{dp}{dr} = c_1\, r \therefore p = \frac{c_1}{2} r^2 + c_2$$

Since if a be the radius of the cylinder, $p=o$ when $r=a$,

$$p = \frac{c_1}{2}(r^2 - a^2)$$

Hence $$q = \frac{c_1}{2}(3\, r^2 - a^2)$$

By substituting these values of p and q in equations (19) and (20), and making $\frac{d}{dr}\frac{\delta r}{r} r = \frac{d\, \delta r}{dr}$, I find,

(63.) $$v = \frac{2\, c_1}{c_3}\left(\frac{1}{9\,\mu} + \frac{2}{3\, m}\right) r^2 + c_4$$

c_4 being the temperature of the axis of the cylinder, and c_3 the coefficient of linear expansion for glass.

Case XI.

Heat is passing uniformly through the sides of a spherical vessel, such as the ball of a thermometer, it is required to determine the mechanical state of the sphere. As the methods are nearly the same as in Case IX., it will be sufficient to give the results, using the same notation.

$$r^2 \frac{dv}{dv} = c_1 \therefore v = c_2 - \frac{c_1}{r}$$

$$c_1 = a_1\, a_2 \frac{\theta_1 - \theta_2}{a_1 - a_2} \qquad c_2 = \frac{\theta_1\, a_1 - \theta_2\, a_2}{a_1 - a_2}$$

$$p = c_4 \frac{1}{r^3} + \left(\frac{2}{9\,\mu} + \frac{1}{3\, m}\right)^{-1} c_1\, c_3 = \frac{1}{r} - c_5$$

When $h_1 = h_2 = 0$ the expression for p becomes

(64.) $$p = \left(\frac{2}{9\,\mu} + \frac{1}{3\, m}\right)^{-1} c_3\, (\theta_1 - \theta_2) \left\{ \frac{a_1^3\, a_2^3}{a_1^3 - a_2^3} \frac{1}{r^3} + \frac{a_1\, a_2}{a_1 - a_2} \frac{1}{r} - a_1\, a^2 \frac{a_1^2 - a_2^2}{(a_1 - a_2)(a_1^3 - a_2^3)} \right\}$$

From this value of p the other quantities may be found, as in Case IX., from the equations of Case IV.

Case XII.

When a long beam is bent into the form of a closed circular ring (as in Case V.), all the pressures act either parallel or perpendicular to the direction of the length of the beam, so that if the beam were divided into planks, there would be no tendency of the planks to slide on one another.

But when the beam does not form a closed circle, the planks into which it may be supposed to be divided will have a tendency to slide on one another, and

the amount of sliding is determined by the linear elasticity of the substance. The deflection of the beam thus arises partly from the bending of the whole beam, and partly from the sliding of the planks; and since each of these deflections is small compared with the length of the beam, the total deflection will be the sum of the deflections due to bending and sliding.

Let $$A = M\,c = E\int x y^2\,dy \quad . \quad . \quad (65.)$$

A is the stiffness of the beam as found in Case V., the equation of the transverse section being expressed in terms of x and y, y being measured from the neutral surface.

Let a horizontal beam, whose length is $2l$, and whose weight is $2w$, be supported at the extremities and loaded at the middle with a weight W.

Let the deflection at any point be expressed by $\delta_1 y$, and let this quantity be small compared with the length of the beam.

At the middle of the beam, $\delta_1 y$ is found by the usual methods to be

$$\delta_1 y = \frac{1}{A}\left(\frac{5}{24} l^3 w + \frac{1}{6} l^3 W\right) \quad . \quad . \quad . \quad . \quad (66.)$$

Let $$B = \frac{m}{2}\int x\,dy = \frac{m}{2}\,(\text{sectional area}). \quad . \quad . \quad (67.)$$

B is the resistance of the beam to the sliding of the planks. The deflection of the beam arising from this cause is

$$\delta_2 y = \frac{l}{2B}(w + W) \quad . \quad . \quad . \quad . \quad . \quad . \quad (68.)$$

The quantity is small compared with $\delta_1 y$, when the depth of the beam is small compared with its length.

The whole deflection $\Delta y = \delta_1 y + \delta_2 y$

$$\Delta y = \frac{l^3}{6A}\left(\frac{5}{4} w + W\right) + \frac{l}{2B}(w + W)$$

$$\Delta y = w\left(\frac{5}{24}\frac{l^3}{A} + \frac{1}{2}\frac{l}{B}\right) + W\left(\frac{l^3}{6A} + \frac{1}{2}\frac{l}{B}\right) \quad . \quad (69.)$$

Case XIII.

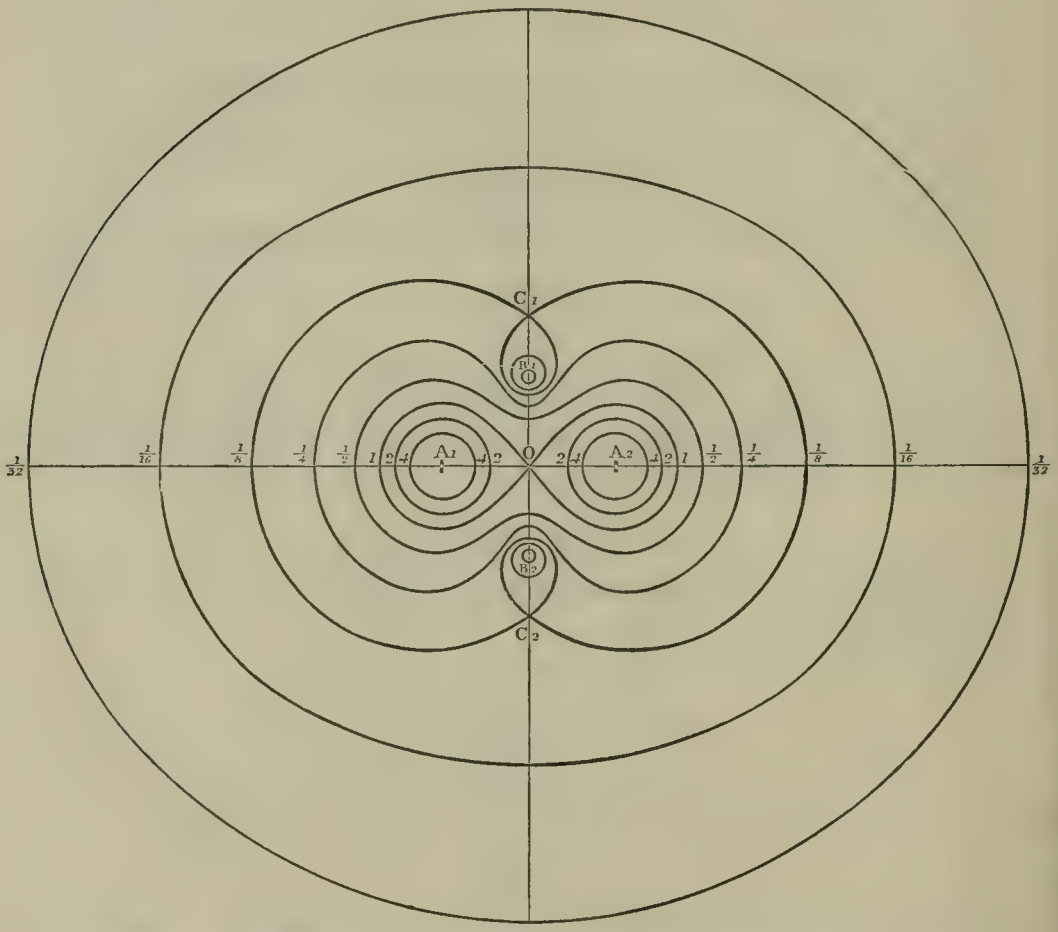
When the values of the compressions at any point have been found, when two different sets of forces act on a solid separately, the compressions, when the forces act at the same time, may be found by the composition of compressions, because the small compressions are independent of one another.

It appears from Case I., that if a cylinder be twisted as there described, the compressions will be inversely proportional to the square of the distance from the centre.

If *two* cylindric surfaces, whose axes are perpendicular to the plane of an indefinite elastic plate, be equally twisted in the same direction, the resultant compression in any direction may be found by adding the compression due to each resolved in that direction.

The result of this operation may be thus stated geometrically. Let A_1 and A_2 (fig. 1.) be the centres of the twisted cylinders. Join A_1 A_2, and bisect A_1 A_2 in O. Draw OBC at right angles, and cut off OB_1 and OB_2 each equal to OA_1.

Fig. 1.



Then the difference of the retardation of oppositely polarized rays of light passing perpendicularly through any point of the plane varies directly as the product of its distances from B_1 and B_2, and inversely as the square of the product of its distances from A_1 and A_2.

The isochromatic lines are represented in the figure.

The retardation is infinite at the points A_1 and A_2; it vanishes at B_1 and B_2;

and if the retardation at o be taken for unity, the isochromatic curves 2, 4, surround A_1 and A_2; that in which the retardation is unity has two loops, and passes through O; the curves $\frac{1}{2}$, $\frac{1}{4}$ are continuous, and have points of contrary flexure; the curve $\frac{1}{8}$ has multiple points at C_1 and C_2, where $A_1 C_1 = A_1 A_2$, and two loops surrounding B_1 and B_2; the other curves, for which $I = \frac{1}{16}, \frac{1}{32}$, &c., consists each of two ovals surrounding B_1 and B_2, and an exterior portion surrounding all the former curves.

I have produced these curves in the jelly of isinglass described in Case I. They are best seen by using circularly polarized light, as the curves are then seen without interruption, and their resemblance to the calculated curves is more apparent. To avoid crowding the curves toward the centre of the figure, I have taken the values of I for the different curves, not in an arithmetical, but in a geometrical progression, ascending by powers of 2.

Case XIV.

On the determination of the pressures which act in the interior of transparent solids, from observations of the action of the solid on polarized light.

Sir David Brewster has pointed out the method by which polarized light might be made to indicate the strains in elastic solids; and his experiments on bent glass confirm the theories of the bending of beams.

The phenomena of heated and unannealed glass are of a much more complex nature, and they cannot be predicted and explained without a knowledge of the laws of cooling and solidification, combined with those of elastic equilibrium.

In Case X. I have given an example of the inverse problem, in the case of a cylinder in which the action on light followed a simple law; and I now go on to describe the method of determining the pressures in a general case, applying it to the case of a triangle of unannealed plate-glass.

The lines of equal intensity of the action on light are seen without interruption, by using circularly polarized light. They are represented in fig. 2, where A, BBB, DDD are the neutral points, or points of no action on light, and CCC, EEE are the points where that action is greatest; and the intensity of the action at any other point is determined by its position with respect to the isochromatic curves.

The direction of the principal axes of pressure at any point is found by transmitting plane polarized light, and analysing it in the plane perpendicular to that of polarization. The light is then restored in every part of the triangle, except in those points at which one of the principal axes is parallel to the plane of polarization. A dark band formed of all these points is seen, which shifts its position as the triangle is turned round in its own plane. Fig. 3 represents these

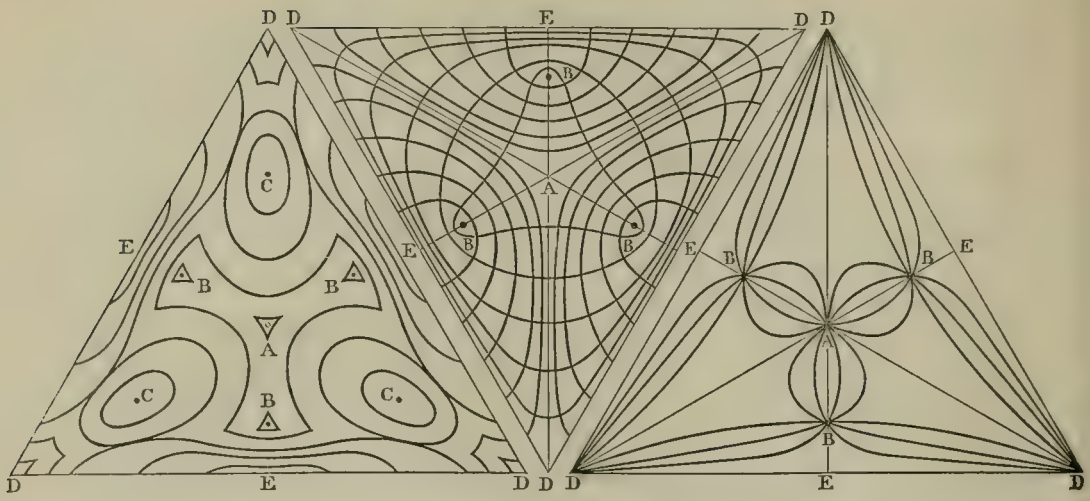


Fig. 2. Fig. 4. Fig. 3.

curves for every fifteenth degree of inclination. They correspond to the lines of equal variation of the needle in a magnetic chart.

From these curves others may be found which shall indicate, by their own direction, the direction of the principal axes at any point. These curves of direction of compression and dilatation are represented in fig. 4; the curves whose direction corresponds to that of *compression* are concave toward the centre of the triangle, and intersect at right angles the curves of dilatation.

Let the isochromatic lines in fig. 2 be determined by the equation

$$\phi_1(x_1 y) = \mathrm{I}\frac{1}{z} = \omega(q-p)\frac{1}{z}$$

where I is the difference of retardation of the oppositely polarized rays, and q and p the pressure in the principal axes at any point, z being the thickness of the plate.

Let the lines of equal inclination be determined by the equation

$$\phi_2(x_1 y) = \tan\theta$$

θ being the angle of inclination of the principal axes; then the differential equation of the curves of direction of compression and dilatation (fig. 4) is

$$\phi_2(x_1 y) = \frac{dy}{dx}$$

By considering any particle of the plate as a portion of a cylinder whose axis passes through the centre of curvature of the curve of compression, we find

$$q-p = r\frac{dp}{dr} \quad . \quad . \quad (21.)$$

Let R denote the radius of curvature of the curve of compression at any point, and let S denote the length of the curve of dilatation at the same point,

$$\phi_3(x_1 y) = \mathrm{R} \qquad \phi_4(x_1 y) = \mathrm{S}$$

$$q-p = \mathrm{R}\frac{dp}{ds}$$

and since $(q-p)$, R and S are known, and since at the surface, where $\phi_5(x_1 y)=0$, $p=0$, all the data are given for determining the absolute value of p by integration.

Though this is the best method of finding p and q by graphic construction, it is much better, when the equations of the curves have been found, that is, when ϕ_1 and ϕ_2 are known, to resolve the pressures in the direction of the axes.

The new quantities are p_1, p_2, and q_3; and the equations are

$$\tan\theta=\frac{q_3}{p_1-p_2}, \qquad (p-q)^2=q_3{}^2+(p_1-p_2)^2, \qquad p_1+p_2=p+q$$

It is therefore possible to find the pressures from the curves of equal tint and equal inclination, in any case in which it may be required. In the meantime the curves of figs. 2, 3, 4 shew the correctness of Sir JOHN HERSCHELL's ingenious explanation of the phenomena of heated and unannealed glass.

NOTE A.

As the mathematical laws of compressions and pressures have been very thoroughly investigated, and as they are demonstrated with great elegance in the very complete and elaborate memoir of MM. LAMÉ and CLAPEYRON, I shall state as briefly as possible their results.

Let a solid be subjected to compressions or pressures of any kind, then, if through any point in the solid lines be drawn whose lengths, measured from the given point, are proportional to the compression or pressure at the point resolved in the directions in which the lines are drawn, the extremities of such lines will be in the surface of an ellipsoid, whose centre is the given point.

The properties of the system of compressions or pressures may be deduced from those of the ellipsoid.

There are three diameters having perpendicular ordinates, which are called the *principal axes* of the ellipsoid.

Similarly, there are always three directions in the compressed particle in which there is no tangential action, or tendency of the parts to slide on one another. These directions are called the *principal axes* of *compression* or of *pressure*, and in homogeneous solids they always coincide with each other.

The compression or pressure in any other direction is equal to the sum of the products of the compressions or pressures in the principal axes multiplied into the squares of the cosines of the angles which they respectively make with that direction.

NOTE B.

The fundamental equations of this paper differ from those of NAVIER, POISSON, &c., only in not assuming an invariable ratio between the linear and the cubical elasticity; but since I have not attempted to deduce them from the laws of molecular action, some other reasons must be given for adopting them.

The experiments from which the laws are deduced are—

1*st*, Elastic solids put into motion vibrate isochronously, so that the sound does not vary with the amplitude of the vibrations.

2*d*, REGNAULT's experiments on hollow spheres shew that both linear and cubic compressions are proportional to the pressures.

3*d*, Experiments on the elongation of rods and tubes immersed in water, prove that the elongation, the decrease of diameter, and the increase of volume, are proportional to the tension.

4th, In COULOMB's balance of torsion, the angles of torsion are proportional to the twisting forces.

It would appear from these experiments, that compressions are always proportional to pressures.

Professor STOKES has expressed this by making one of his coefficients depend on the cubical elasticity, while the other is deduced from the displacement of shifting produced by a given tangential force.

M. CAUCHY makes one coefficient depend on the linear compression produced by a force acting in one direction, and the other on the change of volume produced by the same force.

Both of these methods lead to a correct result; but the coefficients of STOKES seem to have more of a real signification than those of CAUCHY; I have therefore adopted those of STOKES, using the symbols m and μ, and the fundamental equations (4.) and (5.), which define them.

NOTE C.

As the coefficient ω, which determines the optical effect of pressure on a substance, varies from one substance to another, and is probably a function of the linear elasticity, a determination of its value in different substances might lead to some explanation of the action of media on light.

NATURAL HISTORY.

PERUVIAN SYRINX.

V.—*Dissertation on a Peruvian Musical Instrument like the Syrinx of the Ancients.* By Thomas Stewart Traill, M.D., F.R.S.E., Professor of Medical Jurisprudence in the University of Edinburgh.

(Read 1st April 1850.)

The attention which has of late years been paid to the elucidation of the manners and arts of the ancient inhabitants of America, has been productive of the most convincing proofs of the communication between the Eastern and Western Continents at remote but unknown epochs. The learned and highly-interesting researches of Humboldt on the antiquities of the New World, have irresistibly led him to this conclusion, which has farther been strengthened by the researches of later travellers. The comparison of the idioms of the Asiatic and American tongues, has hitherto not afforded very direct proof; because the philologist has not yet been put in possession of a sufficient number of materials to make the comparison with advantage. Our ignorance of the languages and customs of Central Asia is a great bar to such studies, and needs not any other illustration than the fact that a highly-polished nation, with a literature and arts hitherto almost unknown in Europe, should have existed for ages in Central Asia. Our countryman, Dr Gerard, stimulated by the humane desire of extending the blessings of vaccination to Thibet, has been for some time in that country, and has discovered in its language an Encyclopædia in forty-four volumes, of which the medical part alone fills five volumes; and he finds, that the art of *Lithography*, so new in Europe, has been practised from time immemorial in Kinnaour, a principal city in Thibet, where he found it employed to display the anatomy of the human body. Attempts have been made to supply such deficiencies in the knowledge of Asiatic languages, chiefly by the Germans; especially in the first volume of the *Mithridates* by Adelung, and in the *Asia Polyglotta* of Klaproth. When our acquaintance with Central Asia shall be more extensive, and the American languages more studied, we may be able to trace the origin of the nations of that continent with greater success; and Humboldt does not think it impossible, that traces may yet be discovered in America of tongues and nations that have disappeared from the older hemisphere. It would be curious if future inquirers should discover in America vestiges of those torments of the philologist and antiquary, the Median, Oscan, Phœnician, and Hetruscan tongues.

"If language supply," says Humboldt, "but feeble evidence of communication between the two worlds, this communication is fully proved by the cosmo-

gonies, the monuments, the hieroglyphics, and institutions of the people of America and Asia."

It is impossible to consider the Mexican account of the *Serpent-woman*, *Tonacacihua*, or "Woman of our Flesh," the parent of mankind, with her fall from her state of pristine innocence and happiness; their traditions of a great inundation, in which the human race perished, with the exception of a single family that escaped on a raft; their account of the building of a vast pyramid, which was intended to reach to the sky, and consequent dispersion of the sons of men, and the origin of different languages, caused by the anger of the gods, when they overthrew this monument of human presumption; without perceiving the prototypes of these traditions in the sacred writings of the Hebrews.

The Mexican cosmogony notes *five* epochs of the world; like the people of Thibet, and the Tatar tribes who have retained the ancient religion of the Llama. The first is the age of *earth*; the second that of *fire*; the third the age of *wind* or *air*; the fourth that of *water*; *we* live in the *fifth* epoch. It was in the end of the fourth age that the deluge took place, and that a single family was preserved to repeople the earth. As might be expected, *Coxcox* (the Mexican Noah) is represented as the immediate ancestor of the inhabitants of that country. In the first four epochs we may trace the *four ages* of classical antiquity, with the Tatar addition of a fifth. Like the Chinese and Indians, the Mexicans supposed an enormous duration to our earth in all its cataclysms. The Mexican legends extended the age of the world to upwards of 20,000 years.

In the astronomical cycles of some of the American nations, we find strong analogies with the systems of the inhabitants of Thibet, and the various tribes of the Mantscheou Tatars. The Mexican division of the year into 365 days, distributed into 18 months, of 20 days each; the annual intercalation of five days to complete the year, and still more their curious cycle of 52 years, and great cycle of 104 years, in which they intercalated 25 days, to bring the commencement of the next cycle again to correspond with the winter solstice, shew so exact a determination of the true length of the year, that the celebrated LAPLACE is of opinion it could not have originated among a people in so rude a state of society as the Mexicans at their discovery by the Spaniards. The intercalation "of 25 days in 104 years," says he, "supposes a more exact determination of the tropical year than that of HIPPARCHUS, and, what is very remarkable, almost equal to the year of the astronomers of Al-Mamon. When we consider the difficulty of attaining so exact a determination, we are led to believe that it is not the work of the Mexicans, and that it reached them from the Old Continent."

The vast pyramidal temples, accurately placed to the cardinal points, and constructed, as at Cholula, of *sun-dried bricks*, with interposed layers of clay; and occasionally, as at Papantla, with their successive stages neatly covered with hewn stone, sculptured with hieroglyphics, reminded us of the structures of the

early ages of Babylonian and Egyptian architecture. The great pyramid of Cholula has a basis 1440 feet on each side, or twice as broad as the great pyramid of Giza; but its height is only 164 English feet. It is built in four stages, and had a small temple on its upper platform, while the interior contained sepulchral chambers;—circumstances which still farther connect this American temple with the pyramids of Egypt, and the Chaldean monuments described by Rich and others. The curious and systematic mode of hieroglyphic paintings of the Mexicans, which combined natural and conventional signs, and, according to Humboldt, also *phonetic* characters, bears a striking similarity to the hieroglyphical papyri of Egypt; and it may not be unworthy of notice, that the Mexican MSS. were folded up *zigzag-wise*, or something like a fan,—precisely almost as the Siamese papyri MSS. are folded to this day.

The singular resemblance between the institutions of the Peruvian lawgiver Manco Capac and the systems of Hindostan, are not to be overlooked; the same exaltation of a theocracy, drawing its descent from heaven; the same exaction of passive obedience to the head of this theocracy, who, like the first legislator of India, traced his pedigree to the sun; the same division of the people into *castes*.

The Peruvians, like the Hindoos, were, by such institutions, trained into a patient, laborious, little-intellectual people; and, like their Asiatic prototypes, have left behind astonishing monuments of patient industry in some of their public works.

I have introduced this comparison between the people of both hemispheres, in order to shew that I do not assume too much in supposing an instrument invented by the ancient inhabitants of the eastern hemisphere, the *original* of the subject of this paper,—*a musical instrument* of stone found in a *Huaca*, or sepulchral tumulus, which is said to have covered the body of an Inca of Peru.

It was brought from South America by my friend Joshua Rawdon, Esq. He received it from General Paroissien, a native of England of French extraction, who had obtained it as an article of value and great rarity in Peru.

It was customary with the natives of South America to raise large tumuli over distinguished men; and in these were buried domestic utensils in wood, stone, and the precious metals, often with very considerable treasures, especially in Peru. It would seem that the contents of the rich *Huacas* are still known to the Peruvian Indians, either from tradition or from some species of record. They appear to consider it a sacrilegious act for one of themselves to violate the tomb for the sake of its treasures; but there are more than one instance of their rewarding an European for kindness done them by revealing where he may dig with the certainty of obtaining a golden harvest. The vast Huaca near Truxillo, in the Plain of Chimù, was discovered to Juan Gutierres de Toledo, in 1576, by an Indian, and the bars and utensils of gold it yielded to the fortunate Spaniard equalled 46,810 oz. of gold, or upwards of £181,288 sterling. It appears to have

been customary to deposit with the dead the instruments they used, or articles they delighted in; and we may suppose that the Inca with whom this musical instrument was buried was not ignorant of its use. There is no figure of such an instrument among any of the published remains of an American race, as far as my researches have extended; nor am I aware that it has been mentioned among the implements found among them by their Spanish conquerors. It therefore must be of considerably anterior date to the Spanish conquest; as we cannot suppose that since that era, so disastrous to the natives of America, any prince of a native race would have obtained the honours of a *Huaca*, in regions held by the fierce and bigoted conquerors.

Description of the Instrument.

The Peruvian antiquity in question is, in form and principle, similar to the Syrinx of the Greeks and Romans, or Pan's Pipe, well known in England by the somewhat barbarous name of *Pandean Pipes;* and in the Italo-Helvetian cantons by the appropriate denomination of *Organetto*, a diminutive of *Organo*, of which it is most probably the prototype.

The Peruvian instrument, however, is not constructed of unequal reeds bound together; but it is cut out of a solid mass of a compact, softish stone, which appears to me to be a variety of Potstone (*Lapis ollaris*). It is cut with great neatness and precision. Its form will be best understood by inspection of the figure. Its sides are not parallel, but they slightly converge toward the upper part of the instrument, for the purpose, apparently, of rendering the orifices of the pieces thin, without endangering the solidity of the whole. The corners of the bottom of the instrument are smoothly and slightly rounded, as if by friction from the hand of the player. The surface seems to have been covered with a brownish shining varnish, similar to the vegetable varnish employed still by the natives on the Essequibo and Orinoco to cover their pottery. It has in part decayed, and in one place bears the impression of cloth of a coarse texture having adhered to it.

The surface, which has evidently been intended for the outside when played, is ornamented with a very regular pattern. The volutes are very neatly executed, and the regular removal of the angular spaces on the right-hand side of the zigzag lines, shews an attempt at variety not unpleasing. The horizontal band of what we would call *Maltese crosses*, is very well executed.

The extreme breadth of the instrument, including the handle, is 6·2 inches; its greatest depth 5·3; the thickness of the body of the instrument is from 0·7 to 0·5 of an inch. The handle projects 1·1 inch from one end, and is perforated by four holes, two of which appear at its extremity, and one on each of its edges, each of them communicating, in the thickness of the handle, with one of the other

holes. Their obvious use is to receive a cord, for the convenience of holding the instrument more firmly, or of hanging it up.

There are eight pipes or cylindrical tubes scooped out in the thickness of the stone: they have a diameter of about 0·3 inch, and rise in a sort of general neck three-fourths of an inch above the body of the instrument, forming a horizontal connected series of tubes, which, however, have no communication with each other. Their upper edges, on one side, are slightly thinned, which, no less than the ornaments on the side, shew what part of the tube was pressed against the lips.

These circumstances prove that the Peruvian instrument, like the organetto, was held by the player with the longest tubes, or lowest notes, toward his *right hand*. The depth of the tubes was carefully measured, and is as follows:—

No.		Inches.	No.		Inches.
1.	=	4·90	5.	=	2·45
2.	=	4·50	6.	=	2·85
3.	=	4·12	7.	=	2·00
4.	=	3·50	8.	=	1·58

Though these measurements do not seem quite to accord with the usual proportionate length of pipes with regular musical intervals, they seem to have been adjusted from experimental trials by the maker; and I used every precaution in measuring them with a delicate instrument.

In the common organetto, the tubes are portions of the Spanish reed (*Arundo Donax*), of unequal lengths. These are usually 16 in number; and as each pipe differs from the next a note of the ordinary musical scale, the compass of the instrument, with the usual mode of blowing it, is two octaves. These tubes are open at both ends; and the instrument is tuned by the introduction of a piece of cork, which is pushed farther down when the tone of the note is too sharp, and pushed farther up when the tone is too flat. The key-note is first pitched from some other instrument, or by a tuning-fork; and the other pipes are adjusted by the ear from the key-note.

In the Peruvian instrument the tone of the notes appears to have been adjusted with considerable skill, by careful drilling of the stone; and this has been done by means of a circular drill with cutting edges and a hollowed centre, as the bottom of the holes still shews. The truth of the tones shews that this boring has not been done without repeated trials of the effect; and there is no reason to doubt that the Peruvian artist knew also how to amend the tone by *stopping* the bottom of the pipe when necessary.

The Peruvian instrument has eight notes, in the ordinary way of blowing it; but, by contracting the orifice of the mouth, and by pressing the orifice of the tube toward the lip, an octave to the first is obtained from each note, and if the force of the blast be very strong at the same time, a third octave may be obtained: so that, in the hands of an expert performer, the instrument had considerable compass. In this paper, however, we shall confine our notice to what may

be termed the ordinary compass of eight notes, produced by moderate and easy blowing, and producing clear tones.

The Peruvian instrument has a contrivance for giving variety to its notes, which appears to me very ingenious, and which, as far as I can learn, is peculiar to it. Four of its pipes, viz., Nos. 2, 4, 6, and 7, have each a *ventilage*, or small hole perforating its front, about an inch below its top, which must be covered with the fingers of the performer when these pipes are to be sounded. These holes are so near the top of the pipes, that, when open, the sound of the note is quite lost; so that if the performer does not mean to sound that particular note in a rapid movement, it is not necessary to avoid blowing into the pipe, but merely to uncover the ventilage, which effectually destroys its sound. From the peculiar adjustment of the instrument, an harmonious and pleasing *tetrachord* is produced by running up the scale with all the ventilages open.

This description renders it evident that the Peruvian has considerable advantages over the simple Grecian syrinx, which is generally represented in sculpture with *seven* pipes, and occasionally with only *six*. In other respects, the Grecian instrument appears to differ little from the modern organetto; but it is, in some of its modifications, of very high antiquity, and perhaps preceded the invention of the single flute (μοναυλος) with numerous ventilages.

Lucretius describes *Pan's* mode of playing to be the same as we now find it among the Italians:

> "Unco sæpe labro calamos percurrit hianteis
> Fistula sylvestrem ne cesset fundere Musam."
> *Lib.* iv., 592.

The ancients ascribed the invention of the syrinx to the disappointed love of the god Pan, amid the hills of his favourite Arcadia.

> "Pan primus calamos cera conjungere plures
> Instituit." Virgil, *Ecl. II.*

Both Pan and the pipe, however, had probably an Egyptian origin, long before the groves of Greece were haunted by any deity; and, if I am not mistaken, we may trace the syrinx to an antediluvian patriarch. *Jubal*, the descendant of Cain, is in Genesis called "the father of all such as handle the harp and *organ*."

The English translators of the Bible have adopted the interpretation of the Latin Vulgate, in which the Hebrew עוגב, *Yogel*,* is rendered *organum*,—"ipse fuit pater canentium cithera et organo." This passage, in the Septuagint, and in the famous Alexandrian MS., runs thus: ὗτος ἦν ὁ καλαθεὶξας ψαλτηριον καὶ κιθαξαν—"He it was who taught the *psaltery* and the harp."†

* Or, with points, as in Walton's Polyglott, עוּגָב.

† The Hebrew name is derived from the verb עָגַב, which, in the Septuagint, is always rendered by ἐπιτιθημι, *I join together*; which would seem to indicate that it consisted of reeds or pipes *put together*.

What the *Yogel* was has been disputed; but PARKHURST explains it to be a wind instrument of several pipes. The ψαλτηριον of the Septuagint is, by several commentators, said to be a *wind instrument*, or "sort of flute used in churches;" not the modern *psaltery*, which is a trapezoidal flat box, with 13 pairs of strings mounted on two bridges, and played with two crooked sticks.

The invention of the modern organ is a subject of dispute; for few critics will receive Ste CECILIA as the inventor of that noble instrument, although RAFFAELLO has introduced the syrinx in his grand picture of that saint in allusion to this fable. It is of considerable antiquity, however, and it will be sufficient here to remark, that the organ itself is only an adaptation of the more ancient *syrinx* to keys, and an artificial blast of air; and its pipes are tuned on the principle of its venerable prototype.

The ancients seem, however, to have possessed an instrument somewhat in principle resembling the modern organ, in so far as it consisted of several pipes attached to a box, which contained compressed air. In the instrument briefly and obscurely noticed by VITRUVIUS, who lived about the commencement or a little before the Christian era, the air seems to have been compressed by forcing *water* into a brazen box, that communicated with the pipes. The instrument was termed by the inventor, CETESEBIUS of Alexandria, ὕδραυλις; and is attempted to be figured from the description in the Italian translation of VITRUVIUS by BARBATO, Patriarch of Aquileia.

I am indebted to my friend Mr W. CADELL for the notice of a coin of NERO, in the British Museum, on which an Ὀργανον, or perhaps ὕδραυλις, is figured. It seems to be the instrument alluded to by SUETONIUS, in the life of NERO—"reliquam diei partem per *organa hydraulica*, novi et ignoti generis circumduxit." There is a dissertation on the Hydraulis in the Gottingen Transactions.

The organ or psaltery of the book of Genesis, I believe, then, to have been the syrinx; an instrument with which we may reasonably suppose MOSES to have been familiar, as ancient authors generally agree in ascribing the invention of the συριγξ, and of the single flute, μοναυλος, to the Egyptians.

Both flute and syrinx are mentioned by HOMER as known to the Trojans—

Αὐλων Συριγγων τ' ἐνοπην ὁμαδον τ' ἀνθρωπων;

so that, without doubt, the syrinx is an instrument of very great antiquity; and we know that it has been most widely diffused among ancient nations.

Among the Arabs it is in use at the present day. In KŒMPFER's History of Japan, two forms of a syrinx of twelve unequal reeds, used by that people, as also some singular Japanese flutes, are figured in *Tab.* xxxi., A. E. G. J.

From time immemorial, it has been in use among the inhabitants of the Alps; and most of the performers on the *organetto*, who perambulate Europe, bring it from the Italian cantons in the vicinity of the Lake of Como.

That it had been introduced into America, the instrument before you fully proves; and voyagers have discovered a musical instrument very like it in Amsterdam Island, or Tongataboo, in the Pacific Ocean. In a letter which I received from the illustrious HUMBOLDT on this subject, he states, that he had found a rude sort of Pan's pipe among the natives on the banks of the Orinoco.—" Il est bien remarquable de vois les mêmes formes se reproduire dans les regions les plus eloignées; j'avais deja été frappé de l'adresse avec laquelle les indigènes de l'Orinoque savoient construire ses *flutes de Pan*, chaque fois que mes canots s'arrêtement là, où le rivage etoit couvert de roseaux."

Scale of the Instrument.

The first attempts at obtaining an idea of the scale of the Peruvian instrument were imperfect, owing to my little skill in either the theory or practice of music. By means of BROADWOOD's C tuning-fork for concert pitch, compared to a piano, I discovered that the lowest note in the Peruvian syrinx was equivalent to E on the first line, and that the next three notes with that formed a tetrachord nearly corresponding to E, F, G, A; that the fifth ascending note was three notes higher than A, or equalled D; that the sixth note was a note *lower* than the preceding; that the seventh was two notes higher than D; and that the eighth was four notes higher than D.

These notes, however, differing from the piano by half a tone, it occurred to me, that, by obtaining the assistance of an accomplished musician on the violincello, the true scale might be ascertained far better than by my unskilful attempts. I employed an expert Italian performer on the organetto to play on the Peruvian instrument, on different evenings, and I was fortunate enough to obtain the assistance of three musical friends, who unite to fine taste great practical skill in music; and to the aid of these gentlemen I am indebted for the following determination of the true scale and powers of the Peruvian syrinx.

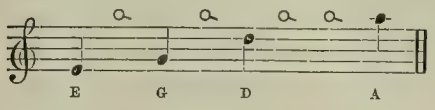
The violincello was tuned to the pitch of the Peruvian instrument, and the value of each of its notes was repeatedly tried by this test. The result of these experiments convinced my musical friends, that the maker of that instrument had proceeded on just musical principles in its formation; and that its eight notes were resolvable into two distinct tetrachords, one of which is in a minor, and the other in a major key.

When the ventilages are all *shut*, the following is the

Scale.

The division of this scale into two tetrachords in different keys is produced by opening the ventilages for the one, and sounding only the notes which were omitted by that process for the other.

Tetrachord in the Key of E *Minor.*



Tetrachord in the Key of F *Major.*

The first tetrachord in the minor key is perfect, and is the most easily performed; for it only requires that all the ventilages be left open, and consequently those notes will not sound. This in all probability was the favourite Peruvian key, and must have imparted, as the minor key always does, a plaintive tone to their music.

The second tetrachord in the major key is nearly perfect; but the instrument on this key is half a note above concert pitch, which throws the F♮ into F♯, and the C♮ into C♯.

It is, however, to be noticed, that, by different modes of ordinary blowing, the tones may be varied nearly half a note; and it is not improbable that the notes now imperfectly pitched, were accurately adjusted by *stuffing*.

The use of the ventilages now becomes very apparent. They enabled the performer to introduce several harmonious modulations, by opening one or more of the holes, without embarrassing him with the attention necessary to avoid the pipes not to be sounded. In this manner considerable variety is given to the succession of sounds, all of which are regulated by the fixed principle of presenting agreeable successions or modulations to the ear. One of my friends was of opinion that some very simple modulations, produced by this means, as an accompaniment to the songs or dances of the Peruvians, was one of the designs of the inventor of the Peruvian syrinx.

The Peruvian performers probably used the succession of simple notes, often reiterated; and we might infer that they often delighted in *slurring* them, by sliding the instrument along the lip, instead of blowing each note distinctly *àllo staccato*, as is usually done by modern performers on the organetto.

It is worthy of remark, that the scale of the Peruvian instrument is founded on a system of *tetrachords*, as was that of a more refined people,—the ancient Greeks. The lyre, according to Diodorus, was invented by the Egyptian Hermes, and had originally only three strings,—λυραν τε ευρειν, ἣν ποιησαι τριχορδον. The historian says that a fourth, "called μεση, was added by the Muses; that Linus added

the fifth string, named λιχανος; that ORPHEUS gave it a sixth, ὑπατη; and that the seventh, παρὑπατη, was the addition of THAMYRIS." Even in this improved state of their musical system, the *fourth* was still a favourite and important interval; for we find that their great musical system, as they termed it, "*extended to two octaves composed of five tetrachords;*" in the same manner that the scale of GUIDO of Arezzo, the inventor of the modern system of musical notation and of *counterpoint*, is composed of different *hexachords*.—See *Burney*.

The sagacity and profound investigations of the learned Sir WILLIAM JONES have clearly proved that the same systems of literature and arts, which once gave lustre to Ethiopia and Egypt, prevailed in India; and more recent investigations, especially those of HUMBOLDT, AGLIO, and several American travellers, have shewn, as we have already noticed, that the arts, the cosmogonies, and astronomy, of the Peruvians, the Mexicans, and some of the other tribes of Central America, betray, in some respects, an Asiatic origin.

VI.—*Some remarks on Theories of Cometary Physics.* *By* C. Piazzi Smyth, Esq., *F.R.S.E., Astronomer Royal for Scotland, and Professor of Practical Astronomy in the University of Edinburgh.*

(Read 1st April, 1850.)

While the physical appearances of comets have ever excited such intense curiosity and interest, all the theories concerning them are generally confessed to be insufficient to explain them; and, certainly, if we may judge from the various views advocated by different writers, and the anomalous forces gratuitously brought in to support the different hypotheses—it is so.

This unsatisfactory state of things, so different from that in which is the theory of the motions of comets,—seems to be owing partly to the difficulty of making the necessary observations by reason of the undefinable nature of the bodies themselves, and partly from the untoward circumstances under which the observations must be made, as well as the rareness of any opportunities offering. Hence, theories are built upon accounts handed down from old astrological times, when men's prejudices would have prevented them, even if their means had been ample, which they were not, from giving any satisfactory and trustworthy accounts of the phenomena displayed by the heavens of their day.

Then, again, the theories appear to have failed from attempting too much, attempting things not legitimately within their reach; it would have been enough to determine the laws of the changes which the tail undergoes during the orbit of a comet; but in place of this, they attempted to shew why the tails were there, and how they came into existence. This is as much as in the planetary theory to attempt to determine why Saturn has rings; a problem which would have eluded the grasp even of Newton, and will for ever remain wrapped up in the mystery of creation; enough for us that the rings are there; we can measure their diameter and thickness, approximate to their weight, and determine the laws of their rotation, and alternate appearance and disappearance to the earth, and to their own planet; and something of the same sort we may expect to be able to do in the case now before us.

It has been remarked that theory and fact sometimes unite, and that something of theory is necessary to enable us to speak correctly of facts. Many instances of this occur in the history of most of the sciences, but in none have the facts been more misinterpreted by the vulgar feeling of the senses, in the absence of correct theory, than in the case of the physical characteristics of comets. No phenomena were so likely to be misinterpreted by reason of the strong prejudices almost innate in men's minds, as well as the specious and inexplicable character of the appearances themselves. Accordingly, because the tails of

comets were seen chiefly at or about the perihelion passages, they were said to be produced then; to have been shot out, and then drawn in again, or dissipated; and numerous have been the theories to explain this creation and extinction. And yet of all the facts that have been ascertained, if any of them can be so considered, with regard to the physical appearances of comets, of none may we be more sure than that the tails of comets, in place of being *largest*, or existing only at the perihelion point of the orbits, are then the *smallest*. Comets of every size, (the distinction of those said to be with and without tails is visionary, or rather the tail is equally a part of the general body of the comet, as the so-called head, and obeys the same laws), when accurately observed, have always been found to decrease in coming to perihelion, and to increase in size in retreating therefrom; this condensation of substance, producing more power to reflect light at that period of the orbit, when, from the closer proximity to the sun, there is more light to reflect. These two causes combining, and both increasing most rapidly with comets of great excentricity and small perihelion distance, occasions the sight, all of a sudden, of a long cometric ray in our skies, when the previous night, or at least the previous clear night, there was none bright enough to catch men's eyes. As the comet leaves the sun, the tail or body expands, and partly from its consequent greater rarity, and the diminishing intensity of its solar illumination, is lost to our sight; and only the denser roundish portion about the head remains visible. This is likewise expanding, and is at length also lost sight of for the same reason. In like manner a comet reappears, first the oval mass about the head, and then the tail gradually strengthens; but its aspect will materially depend not only on its distance from the sun, but on our distance from it, and the direction of our line of sight with the longer axis of the body.

Having had the good fortune to see a rather large number of comets, both great and small, and under circumstances favourable above the average, I hope that I may be of some service to theorists, by stating what data may be looked upon as well fixed with regard to these phenomena; by pointing out some corrections which are absolutely necessary to be made upon the observations, before any good and safe grounds for theorizing can be procured, but which corrections never have been made; and by pointing out the most probable method of improving the observations themselves, which, as at present conducted, are by no means satisfactory.

With regard then to the physical nature of comets, we may take the following as axioms:—

1. A comet consists of a nucleus, and one or more gaseous envelopes.

(1.) No instance has ever been recorded, at least since the fabulous days of astronomy, of a comet having ever been seen without some gaseous appendage, forming, indeed, a distinctive feature at a distance from every other body of the solar system; at a distance, because very close to one of the planets, especially one or two of the asteroids, something of its atmosphere might be ob-

served; but, at the actual distances at which they are viewed, the most powerful telescopes never show these atmospheres in the same manner as those of a comet; they are indicated only by a very different order of phenomena.

When Uranus was first discovered, and no one dreamt of planets beyond Saturn, it was called a comet; not because its form was like that of any recognised comet, but because it was expected that its orbit would prove similar; when, however, the real nature of its path was discovered, the appellation of comet was quickly retracted.

So much for the necessity of a gaseous envelope; of the equal importance of a nucleus, it may be remarked, that although some comets are described as having nuclei, and others as having none; this turns out to be but negative testimony, inasmuch as these latter bodies have always been the fainter, smaller, and more distant ones, in which the nucleus should have been so much the more difficult to distinguish; and if it has not been actually observed itself, there has at least been invariably noticed in every recorded comet, some one point where the gaseous matter was visibly more concentrated than in other parts, indicating thus a virtual or a dark nucleus, if not an actual and a reflective one: while observation, combined with calculation, has satisfactorily shewn, that in comets of every degree of size and excentricity, the mass is so very nearly concentrated in this nucleoid centre, that that need alone be referred to in all determinations of the orbit.

2. The nucleus if solid and material, is exceedingly small.

(2.) Every advance of our knowledge has tended to diminish the possible size of the solid nuclei of comets, planetary perturbation has shewn them to have no sensible mass, and telescopic observation no sensible size; and in the cases of comets of all sizes, observers have witnessed them pass over stars in every position, except, perhaps, exactly centrically with the nucleus, without perceiving any obscuration of the stellar rays.

The old observers have certainly spoken of very large nuclei, but they evidently meant rather the head, which, in some comets at certain parts of the orbit, presents in small telescopes an appearance of planetary opacity and definition.

Such was the case with the great comet of 1843, for three or four days after having passed its perihelion; in small telescopes it was difficult to avoid believing in the existence of an actual planetary nucleus of very notable size; but the fourteen feet reflector of the Cape Observatory shewed the borders of this head to be filmy, and exhibited small stars shining through it; day after day it expanded and became less defined, until at last it ceased to present a solid appearance in any telescope; and at no time was there anything larger than a stellar point, to which the attribute of hard or heavy matter might be expected to apply.

3. The nucleus is excentrically situated in the gaseous body.

(3.) The nucleus actual or virtual, has never been observed in the middle of the envelope, but always nearer one end than the other, the envelopes too, never being round, but invariably more or less elongated.

4. Comets of longest period have the largest bodies.

(4.) This is the general result of cometary statistics, but need not be any more strictly true, than that the largest planets are all at the greatest distances from the sun; they are not strictly ranged in the order of distance agreeably with size, but as a general rule merely, the smaller are closer to the sun than the larger planets. In the same way the telescopic comets, when sufficiently numerous observations have been obtained, have almost always been found to have short periods, and those very brilliant ones, with not only long but broad and dense tails, have invariably been found to be of long period.

5. Those comets whose orbits have the greatest excentricity, are the most excentrically situated in their envelopes, or, vulgarly speaking, have the longest tails.

(5.) This may not be strictly true, but yet is assuredly a very marked feature in the statistics of the question. The great comet of 1843, whose orbit was the most excentric ever known, *i. e.*, had the least perihelion, but great aphelion, distance,—had also the longest and narrowest tail, and the smallest head; consequently the nucleus situated near the centre of the latter was most excentrically situated in the gaseous envelope. Halley's comet, and that of 1811, of less excentricity of orbit, had shorter and broader tails and larger heads; and their nuclei, consequently, less excentric: while the telescopic comets of short period, and aphelion not extraordinarily greater than their perihelion distances, exhibit merely somewhat oval masses of vapour.

6. A comet revolves on an axis passing through the nucleus, and at right angles to the major axis of the envelope, in the same period of time that it takes to revolve about the sun: hence, the tail being turned away from the sun in the normal position, is turned away from him in all other parts of the orbit also.

(6.) Every comet has invariably been observed to have its tail turned away from the sun in every part of its orbit; this was the first notable fact established in cometary physics, and the axiom is but a different statement of it.

7. This axis is not at right angles to the plane of the orbit, but variously inclined in the case of different comets, as with the planets.

(7.) There is no reason to expect the contrary; indeed, analogy rather leads us to this conclusion, and it may, if admitted, be sufficient to explain the apparent want of symmetry observed in the tail of Halley's comet, that of 1819, and most, if not all, which have been the subject of special attention; and it may tend to account for some of the differences in the appearance of the former body in approaching and leaving its perihelion, at considerable but equal distances on either side of that point.

8. A quicker rotation round the longer axis of the body also appears to exist.

(8.) This seemed to be almost proved by some of the changes which took place in the head of the great comet of 1843, night after night, in the earlier part of its apparition; for instance, a double-winged head, laterally, one night, becoming a single and centrically winged, or rather a tailed-head the next night; but when a body is seen for so very short a space of time, for a few minutes only in twenty-four hours; and sometimes, perhaps, for several days, even that short glimpse is prevented by clouds,—it becomes extremely difficult to separate in such a body as a comet, in which there is nothing decided and tangible, and fixed either in size or brightness, any indications of revolution from those of the other motions and changes which are going on simultaneously. But it seems a point well worthy of attention, and to be proved or disproved.

9. A comet shines by reflected light, and shews a sensible phase; the quantity, form, and position, therefore, of its component matter, cannot be judged of by the eye alone.

(9.) That comets shine by reflected light, is considered to have been proved by Arago's polarizing experiment; and was inferred before by every analogy in the planetary system; but all appearance of *phase* has been denied, this, therefore, requires a little explanation. The supposed absence of phase has been attributed to the excessive tenuity of the matter of the comet, and the case has been illustrated by reference to the thin clouds often seen in the west after sunset, or in the east before sunrise, glowing in the solar rays, literally drenched with light, and exhibiting no distinction of light and dark side. A little examination of this instance would have shewn that the conclusion is not so safe; the whole of the cloud being so bright, the difference of illumination of the two sides of it is

merely so much the more difficult to distinguish, by reason of the well-known optical or physiological law, that a small difference in the brightness of two objects is more difficult to perceive in proportion to their absolute brightness. If our sensation and means of measurement are not sufficiently accurate in this case of the thin cloud, we have only to turn to a thicker cloud (of the same species, and in a similar part of the sky with regard to the sun), and there we shall see the same law which must obtain in the former case now visibly developed; and then we come to the necessary conclusion, that the illuminated side of every cloud must be brighter than the other, *i. e.*, that it must shew some phase.

The comets are undoubtedly far rarer than any description of cloud floating in our atmosphere, but they are seen under far more favourable circumstances for exhibiting a phase; for, they are illuminated by the sun from one *end*, so that there must be a much greater difference of intensity of light at the two ends than there would be at the two sides, if transversely lighted, as with the long thin films of bright cloud alluded to; and, further, the comet being of the last degree of faintness, the eye is much better able to detect small differences of luminosity. Then again, comets, though they be exceedingly rare, are very voluminous, so that the rays of light have to traverse a great space of matter in passing through them; and if some is reflected in the anterior parts of the body, as we see is the case by the fact that the body is rendered visible to us, there cannot possibly be so strong an illumination on the posterior parts; therefore, we shall either see them fainter than the others; or not at all, if the anterior portions themselves are but just visible.

With these preliminaries then, we may ask, what comet has ever been seen without some phase? for in every single instance, the anterior part of the head, or the denser portion of the envelope, has been brighter than the posterior, exhibiting sometimes the appearance of a luminous sector in front; and the anterior half only, of the body, has been seen, the comet presenting as a general rule two diverging and slightly curved tails. This has been generally held to be merely the effect of looking transversely through a conical envelope of luminous matter, when the ray of light passing through the central portions would meet with less substance, and that part would therefore appear darker than the limbs. This, doubtless, prevails to a great extent, but then we must further remember, that the exterior coats of the envelope will be more strongly illuminated than the interior; and the dark axis of the comet's tail becomes therefore a particular character of phase. Further, as we proceed to the posterior portions of even the outer coats of the envelope, they will be illuminated by a weaker light from the sun, by reason of their greater distance; and if any convergence of them towards the axis should occur, as has actually been observed in some cases, their illuminating rays being then still further diminished in intensity by absorption and reflection, they will hardly be enabled to make themselves visible to us. Thus, the diverging limbs of the tail, and its forked or many-pointed termination, becomes an effect of phase on a body which may be of a symmetrical and rounded, and complete character.

This point, it is of the greatest importance to determine, for if the actual forms of comets be as we see them, they are altogether anomalous in the heavenly regions; and merely on the score of the form of these supposed conical envelopes and diverging streamers, equally anomalous forces have been introduced to explain the phenomena; electricity and polarity, which have no place in any other department of astronomy, being allowed precedence here.

Granting, that a comet is always a prolate spheroidal mass of vapour of different degrees of prolateness, and of actual length in various cases, but always illumined from one end, then we may expect in the larger and denser comets to see but the anterior half of the body; the posterior half being so much further off from the sun, and the rays of light which reach it, being further so much weakened by having passed through the first half; consequently, in this description of comet, we might expect, and we absolutely do find, the phenomenon of the forked tail most marked. In the smaller and fainter comets, on the other hand, the rays of light which reach the posterior half of the body are not much dimmed either from having passed through the excessively tenuous anterior portion, or from having travelled through any notably greater distance from the centre of radiation; in such cases we may expect to see more completely the whole form of the comet; and in them we do actually find nearly, and sometimes quite oval forms, and all gradations from these, through truncated ovals to the forked tails.

These facts induce us to admit the possibility of the bodies of comets being of a far more regular geometric form than has hitherto been suspected, if we allow that conclusions derived from small comets may be safely so extended, *mutatis mutandis*, to large ones; but this view is further confirmed by a notable observation of one of the largest and most excentric of comets.

As already observed, comets decrease in size and increase in density on approaching the perihelion, and the reverse on receding therefrom; hence the phase ought to be most evident, or the tail

most forked about the perihelion, both on account of the greater absorption and reflection of light in passing through the denser anterior half, and on account of the greater ratio which the difference of distance of the anterior and posterior ends bear to that of the former and the sun; and at a distance from the perihelion, the effects being reversed, we might expect to see less phase, a less dark axis in the tail, and something of a convergence in the limbs of it. Now, both of these phenomena were distinctly and markedly observable in the great comet of 1843; near the perihelion the tail being forked, the axis almost as dark as the sky round about, and the limbs intensely bright and sharp; but long before it was lost on its retreat to aphelion, the oval darkness was almost obliterated, the whole tail was diffuse, and the posterior portion for fully one-third of the whole visible length shewed a convergence inwards.

But the notable phenomenon is still to come; allowing the above increase of phase in approaching perihelion, it is also evident, that if the perihelion distance be very small, the sun may present a very large angle as viewed from the comet; and in this way rays of light may reach every part of the external coats of the body, and these may be also illumined to that intense degree, that as with the sunrise and sunset clouds already referred to, no phase may be seen; so that, with such comets the maximum of phase will occur a short distance on either side of the perihelion, at and very close to it there will be little or none.

As the comet of 1843 almost touched the sun's surface in passing round it, it must have presented as satisfactory and conclusive a proof as man could have wished for. But although it must have been visible to the naked eye, and to nearly the whole world, in this critical part of its orbit, no mortal man is known to have seen it. Rather a melancholy fact of the imperfection of the astronomical watching of the present age; and it appears all the stronger, from Arago having, in his report to the Academy, descriptive of the discovery of a small comet, enlarged on the perfection of the system of search organized at the Parisian Observatory; by which it appeared that nothing could escape detection; for the assistant who made the discovery, having purposely kept silence when he was relieved in his watch by another person, this one discovered the same comet before having been an hour at his post. But to return to the comet of 1843, it was seen while still not very far from the perihelion, when the sun was still subtending a very large angle, viz., on February 28, the perihelion passage being February 27, 1843; but then only by three persons, or rather parties, and none of them have given sufficiently accurate accounts of what they saw, or have attempted what would have been so invaluable, if effectually and faithfully executed, a *drawing* of the appearances; but their statements, as far as they go, decidedly confirm the views above enumerated. The first of these happy three, with whose account I became acquainted, was a person at the Cape of Good Hope, who (decidedly no scientific person, and having no prejudice in favour of any theory), described the comet as he and his shepherd boy saw it at noonday, a bright hazy star, with the hazy matter streaming off on one side, and *collected into a focus* about two feet *behind it*. Allowing him to have estimated the sun's diameter at one foot, the apparent length of the comet's tail is well given; and the comet itself being spoken of as a bright star in the hazy matter, which streamed off, and collected into a focus at a certain distance behind the head; this certainly may be interpreted into a somewhat symmetrical elliptic figure, having the nucleus in the focus nearest the sun.

The next testimony is from the ship Owen Glendower, the crew and passengers of which ship, when off the Cape on February 28, saw the comet plainly about sunset, "as a short dagger-like object close to the sun." This is not particularly explicit, but yet we may certainly conclude from it, that the comet was broad in the middle of its length, and pointed towards each end, and had little or no axial darkness, which sufficiently conforms with our idea of the perfect shape of the envelope of a comet seen under such circumstances.

The last witness is from the United States, where Mr Clarke, of Portland, saw the comet at 3^h P.M., on the same day, and examined it telescopically, and describes it in these words:—"The nucleus, and also every part of the tail, were as well defined as the moon on a clear day. The nucleus and tail bore the same appearance, and resembled a perfectly pure white cloud without any variation, except a slight change near the head, just sufficient to distinguish the nucleus from the tail at that point." The first sentence well describes the increase of density and definition we have already insisted on as a consequence of so near an approach to the sun; and the second paragraph as perfectly describes the absence of axial darkness, a consequence partly of the increased brightness of the illumination of all the external portion, and partly of its being seen in daylight, and so close to the sun; for then, as every one knows, even the darkest shadows amongst the mountains, and in the craters of the moon, those which appear absolutely black at night, are, under those circumstances, barely distinguishable from the brightest portions. As to the shape, Mr Clarke says,—that the

whole body of the comet was seen, but what his idea of the true complete form of its body was, he does not give; but, as he states, that a slight change of brightness near the head was "the only thing to distinguish the nucleus from the tail at that point;" and, further, "that every part of the tail was as well defined as the moon on a clear day;" it would appear to be quite safe to infer, that the tail was not forked; and that the base, instead of a broad or forked, or many-pointed indistinct termination, was as well and sharply outlined as the limbs. A notable distinction this to every subsequent view obtained on succeeding days; and, indeed, in the case of every other comet whatever observed at a great distance from the sun,—when, whatever the definition of the limbs of the tail, the termination or the base has always been so excessively uncertain, that different persons have varied several degrees in assigning the place of it.

Alas! indeed, that the practical astronomy of the present day did not take better account of this unique and critical instance which was offered by the skies of our times; centuries may elapse before another such instance may occur, and this question of the real and complete form of a comet may be in abeyance as long. Something may, doubtless, be done by rigid examination of all the persons who did witness the phenomenon in the comparatively imperfect form of the day after the perihelion passage; but their answers would not be very safe now, so many years after the event, and after the promulgation of a particular theory. Something might also, perhaps, be done, by careful and photometrical observation of the faintest nebulæ, while the darker part of a comet's tail, if it exists, must be passing across them. But this is a very unpromising method, for comets, at all periods, attenuated, become so exceedingly diffuse by the time that they have reached a sufficient distance from the sun, to be viewed for any length of time in a dark sky, and contrasted therein with very faint nebulæ,—that we can hardly expect to obtain any certain indication in this manner. The only sure way is for the comet to be so very close to the sun, that rays from some part or other of his surface will reach every portion of the body of the comet directly, *i. e.*, without having to pass through any other part in order to arrive there.

The fact of this great and invaluable opportunity having been lost, would seem to shew that it is highly desirable that extra meridian observations should be made and watched for by some public observatory in its official routine, instead of being abandoned altogether to amateurs. It is high time that our observatories should be placed in the clearer climates of some of the colonies, and that the most favourable geographical positions should be sought for, rather than the most convenient places in a social point of view; for this results in smoky towns in our own beclouded country being selected as the places where the stars are if possible to be observed.

10. The gaseous envelope is of extreme tenuity, is elastic, and with regard to light is slightly reflective and imperfectly transparent; it decreases in size, but increases in density, and light reflective power in approaching the perihelion, and the reverse when receding from it; and this occurs in a degree proportioned to the excentricity of the orbits of the comets.

(10.) That the gaseous envelope of a comet is of extreme tenuity, and is elastic, slightly reflective and imperfectly transparent, is apparently confessed on all hands, and is proved by the phenomena presented by every comet. That it increases in density and light-reflective power with its proximity to the perihelion, and that this occurs in a degree proportioned to the excentricity of the orbit, requires, at least the latter part does, that the instances on which it is founded should be mentioned; for though the contraction in size of small comets on approaching the sun had been remarked, yet some had maintained it to be accompanied by a decrease in density, by an actual evaporation and disappearance at perihelio; and no one that I am acquainted with had applied it to the larger comets also, or compared the degree of it, with the excentricity of the orbit.

With regard to the effect of excentricity of orbit, a small proportion of it should make a comet visible for a long period on either side of the perihelion, from the lesser degree of attenuation and expansion of its substance at a distance therefrom; and it should also be lost in the sun's rays for a considerable time at and about the perihelion passage, from the matter never being compressed into a sufficiently dense body to be visible in the blaze of day. This rule appears well borne out by both small and large comets; the small ones, for instance Encke's, Biela's, and Faye's, which have for the ratio of the excentricity to the semiaxis major, the numbers respectively, 0·847, 0·755, 0·555, shew no very

well marked changes or even characteristics at any part of their orbits, and are soon lost in the twilight even in their densest states; there being little compression, and by axiom 4, little substance to compress, the mean distances being only 2·216, 3·502, and 3·812, the earth's distance being unity.

In the case of HALLEY's comet, however, the appearances are very different, the excentricity being 0·967, and the semiaxis major 17·988; hence, on this large body approaching the sun and undergoing such a much more intense degree of compression, distinctly marked changes were seen almost from day to day, and at a certain distance from the perihelion it was of great brightness. But the perihelion distance being still large, about half that of the earth, or near fifty millions of miles, the condensation was not sufficient to enable the comet to be seen in moderate twilight, and hence it was not seen after the perihelion passage for more than two months, but then remained visible for nearly four months, so that it was lost sight of at about six months after perihelion passage.

The great comet of 1844–5 had a less perihelion distance, viz., about 25 millions of miles, and a mean distance probably much greater, hence it was sufficiently concentrated in the neighbourhood of the sun to force itself on the notice of men within a week after perihelion: which implies a very much greater degree of brightness, than if HALLEY's comet had been seen as early, when powerful telescopes, directed by means of an accurate ephemeris, were employed in the search. This comet remained in sight between three and four months, and when last seen was a faint nebulosity with little or no apparent concentration in any part.

But the great comet of 1843 is again the decisive test, as this had a perihelion distance of only half a million miles, 60,000 only from the surface of the sun: here, therefore, we might expect to see the brightness excessive at and about the perihelion; but the subsequent expansion, on account of the great mean distance, would be so rapid that the comet would be soon lost sight of by reason of faintness. Accordingly, we find that this comet pressed itself on men's attention one day only after the perihelion passage; and from its being so very bright then, and yet seen by so few, there can be little doubt but that it might have been observed the day before, if it had been looked for; and would have been so seen, were not staring into the sun's face and immediate vicinity rather a trying, and, consequently, an unpleasant occupation to most eyes, and seldom indulged in, especially in the warmer countries of the south, when the sun might have been that day unveiled from cloud, and was high in the sky. But, however, even the day after the perihelion passage, when the comet must have been much less dense than at that epoch, it was quite bright enough to be seen throughout the day within two degrees of the sun, and was then about one degree in apparent length; four days after it had increased to 25 degrees, in a fortnight to double that; in a month it was so faint and distended as to be lost to most person's eyes, and powerful telescopes only kept it in sight a few days longer. Its meteor-like brightness and short ephemeral existence were subjects of general remark in the south.

This instance may be considered to settle the matter, but Mr HIND's interesting comet of 1847 as a later instance, and a well-marked one also, is very deserving of mention. He discovered this on February 6, 1847, as an exceedingly faint nebulous body approaching perihelion; he observed the gradual condensation in the head and appearance of nucleus and tail, this last being about a degree long on March 9; and having computed the orbit and found the time of perihelion passage to be March 30·269, Gr. M. T., and that the distance from the sun was then only four millions of miles, he called general attention to the circumstance under the hope that, 1*st*, the comet might be seen in daylight on that day; and, 2*d*, that a long tail might be visible in the evening after sunset. In the former he was borne out by the fact, for he observed the comet himself with a refractor of 7 inches aperture at 11^h A.M., within two degrees of the sun, and three other persons are recorded to have witnessed it too. I examined that part of the sky myself on the occasion, but with a telescope of only 3·7 inches aperture could see nothing: Mr HIND himself found it a very difficult object to observe, so that the sizes of the two instruments may be taken as giving some measure of its visibility. In the latter supposition he was not confirmed; for no person saw a tail after sunset, and he himself says that the tail which he saw, exceedingly faint certainly, in the telescope in the day time, very nearly at the epoch of the perihelion passage, was only 40″ long,—but the 90th part of its length 21 days before.

He was led to the first conclusion by the consideration, that the intensity of the light would vary as $\frac{1}{r^2 \Delta^2}$ (when r is the comet's radius vector, and Δ its true distance from the earth), whence the comet should be at the time of perihelion 230 times brighter than that on March 8, when it was just perceptible to the naked eye. (*Royal Astronomical Society's Monthly Notices*, vol. vii., p. 248.) But here it will be seen that with regard to the distance from the sun and perihelion, the intensity of solar illumination alone is taken account of; but the concentration of the comet at the perihelion must have greatly assisted the effect, and without this it seems pretty certain that the comet would not have

been visible : then the introduction of the distance from the earth does not appear correct, for although this may change the apparent diameter of the body, it does not at all alter the intrinsic brightness of the surface.

The second conclusion he was led to, by the old erroneous idea (to use his own words) that " the close approach of the comet to the sun would be likely to *produce* a tail of considerable length :" but in place of so doing it was contracted in size to a very small compass. This additional instance of the prevalence of an idea so completely the reverse of the fact, will, I hope, excuse me from having attempted in so very crude a manner to establish what appears to me the grand statistical truths of cometary physics. But if I have not been able to agree with Mr Hind in his physical ideas, I must express my testimony of his high standing in the more important question of the motions of comets ; here he has indeed filled an honourable niche, which had been long, if not always, unfilled in the cometary credit and fame of this country.

A general result in cometography, certainly following the establishment of this axiom, is, that when the length of the tail of any comet of celebrity is described in millions of miles, a very favourite method with most writers, it will be absolutely necessary to accompany it with an account of the part of its orbit, where the comet is supposed to have been at the time : without this, the statement of an actual length, is as absurd as the fixation of the place of the magnetic pole, without a date being attached.

11. The axis of the tail of a comet is straight at the perihelion, but at any point between this and the aphelion is curved, and is concave toward the latter, the radius of curvature being inversely as the excentricity.

(11.) This I will not attempt to lay much stress upon ; but certainly the tails of the comets of Halley, of 1843, and of 1844–5, were sensibly straight near the perihelion ; and the two latter became curved after it, the former more than the latter, and they were concave to the direction in which they were proceeding ; precisely the reverse of the general belief, which states them to bend backwards at the extremity of the tail, as if experiencing some resistance, when whirled round the perihelion with such exceeding velocity.

The direction in which those two comets were proceeding at the time was towards the aphelion ; and I have not had any opportunity of examining a large comet coming up to the perihelion. The great comet of 1843 would have been sufficient to settle the question, but I have only heard of one person (a Commissariat officer voyaging from New South Wales to the Cape), who saw it in the eastern skies before sunrise and the perihelion passage ; and he had made no observations.

12. The molecules composing the envelope of a comet are only held together by their mutual gravitation, each constituting almost a separate projectile, and describing its own parabola about the sun.

The 12th axiom is Sir John Herschel's, and taken in conjunction with the others, seems generally to explain all the principal variations in appearance, and affords ground for testing each exactly by calculation, and thereby of ascertaining what residual phenomena may be due to laws others than those of gravitation, mechanics, and optics.

After alluding to the observed concentration of Encke's comet near perihelio, and the error of attempting to account for it by the pressure of a supposed æther in the vicinity of the sun, Sir John says (*Royal Astronomical Society's Memoirs*, vol. vi.), " It appears to me that the phenomenon is (if not wholly, at least partially) explicable on a much less gratuitous supposition, viz., that of the extremely feeble attractive force by which the matter of a comet must be held together,

owing to the probable minuteness of its mass. Cohesion can hardly be supposed to exist in a gaseous or nebulous body of such tenuity; so that the only bond of union between its molecules must be their feeble gravitation to each other, which is hardly more than mere juxtaposition in space. Hence we must regard each molecule as constituting almost a separate, independent projectile, describing its own parabola about the sun. Now, the interval between two or more parabolas described about a common focus, and having their axes coincident, is a minimum at the perihelion, and increases as we recede from it" in the sesquiplicate ratio of the radius vector. The observations of ENCKE's comet, which Sir JOHN treated by this theory, shewed rather a more rapid rate of increase and decrease; which might, he thought, be readily accounted for by the effect of the brighter background of sky on which the comet was projected as it approached its perihelion, and *vice versâ*. But whether any other forces may have part in the entire phenomenon presented, he concludes that the property above pointed out, cannot but be allowed to be a *vera causa*, and to have some share in the production of the effect.

To the latter part of this opinion every one must assent; and with respect to the want of agreement between theory and observation, in the case quoted by Sir JOHN of a small comet, in addition to the observations themselves requiring correction, for the cause he has mentioned, the theoretical quantity requires it also on account of the greater attraction of the molecules upon each other at the perihelion, by reason of their increased proximity; while, moreover, the figure of the comet, and the direction in which it is seen, require also to be taken into consideration. With regard to *large* comets, which seem generally to have been thought to be under the dominion of absolutely different laws, the decrease and concentration of the tail at perihelio, is fully accounted for by this 12th axiom, as well as some other phenomena, the perplexing nature of which, when viewed by the light of any other theory, may be gathered by the account given by Sir J. HERSCHEL himself, at the conclusion of the chapter on comets in his work of last year (*Outlines of Astronomy.*)

" It is in a physical point of view that these bodies offer the greatest stimulus to our curiosity. There is, beyond question, some profound secret and mystery concerned in the phenomena of their tails. Perhaps it is not too much to hope that future observations, borrowing every aid from rational speculation, grounded on the progress of physical science generally (especially those branches of it which relate to the ethereal or imponderable elements), may ere long enable us to penetrate this mystery, and to declare whether it is really *matter*, in the ordinary acceptation of the term, which is projected from their heads with such extravagant velocity, and if not impelled, at least *directed*, in its course by a reference to the sun, as its point of avoidance. In no respect is the question as to the materiality of the tail more forcibly pressed on us for consideration, than in that of the enormous sweep that it makes round the sun in perihelio, in the manner of

a straight rigid rod, in defiance of the law of gravitation, nay, even of the received laws of motion, extending (as we have seen in the comets of 1680 and 1843)* from near the sun's surface to the earth's orbit, yet whirled round unbroken; in the latter case through an angle of 180° in little more than two hours. It seems utterly incredible that, in such a case, it is one and the same material object which is thus brandished."

This and much more to a similar effect might be quoted from Sir J. Herschel, and other authors, as to the difficulties experienced in the usual method of viewing a comet, as a planetary body at the nucleus, with an appendage attached to, and whirled along with it and by it. No wonder that doubts were expressed as to the attractive force of the small nucleus being able to retain within its grasp portions of matter thrown out to such distances; and that fears were expressed as to the breaking off of the tail; and, because some thought that it ought to bend backwards from the resistance experienced in its course, therefore they said that they did see it bend. Other difficulties also follow from the usual mechanical view of the production of the tail at perihelio, as has been stated by many, from the heat of the sun causing the nucleus to throw out jets of vapour on the side of that luminary, which again has the power to bend them back, and sending them streaming past the nucleus once more, forms the tail. These jets of vapour ought to drive the nucleus away from the sun; for though it may be said that the vapour, being nearly imponderable, should not produce any sensible or visible effect on the nucleus,—yet the nucleus itself is, for anything we know, as imponderable; indeed, if we judge of the masses of the two by the quantity of light reflected, which is almost the only indication we have, and perhaps not a very bad one, then the mass of the tail in most cases exceeds that of the nucleus, *i. e.*, if the quantity of light reflected by the whole envelope were to be concentrated into a single point, it would be brighter than the nucleus. Hence with the extravagant rapidity and the enormous quantity of vapour rush-

* This is not stated with perfect correctness, at least with regard to the comet of 1843, which might have had a tail of that length some days after the perihelion passage, when it had grown with the rapid increase of its radius vector; but the first day after the perihelion passage, the tail was observed to be only double the sun's diameter (excluding inclination), and its distance must then have been 100 times greater than at the perihelion; so that if the sesquiplicate ratio holds good, we shall have for that epoch a size not very different from planetary bodies. (A curious meeting this must be of the molecules brought for an instant into such close proximity, after having been separated for ages by distances so vast and inconceivable as they must be at the aphelion; and when separating for their diverse orbits, what speculations on their next meeting, not in thunder, lightning, and in rain, but in light and heat unspeakable. On the last occasion, February 1843, the heat was equal (according to Sir J. Herschel) to 47,000 of our suns, 1900 whereof are sufficient to melt the very rocks. Such, at least, must have been the heat, if the comet travelled at that part of its orbit only at the mean rate of the earth; but the velocity was really vastly greater, and the heat much modified thereby. The degree to which velocity in the heavenly spaces may modify distance, in respect of heat, is one still open for inquiry; and the result, in the case of our own earth, as far as it may have been very imperfectly examined into, would lead us to expect that the above proportion would be greatly reduced.)

ing forth from one side only of the nucleus, that body should be driven far in the opposite direction: but by comparing its observed daily places during the perihelion passages with the computed, we find that no deviations from any such anomalous causes are ever experienced.

All these difficulties, however, vanish on considering the envelope of a comet to consist of separate molecules, each constituting an independent projectile, and bound together only by their mutual gravitation and the laws mentioned above; for then the size, character, and position of a comet being given at the perihelion, which we must look on as the normal state, all its principal variations of appearance during the rest of the orbit may be readily computed; and the return of every particle of the envelope to perihelion, or, vulgarly speaking, the retention of the tail by the nucleus, will be no more surprising, nor deviating from ordinary laws, than the return of the nucleus itself: certainly there is nothing "in defiance of the laws of gravitation, and even of the received laws of motion," as stated by a supporter of the other theory to be the case with that.

But following this principle further, we may expect that, in the multitude of molecules moving about amongst each other, occasional conglomerations may occur, after passing the proximity of some large planet, whose attraction acting much more strongly on one part of the envelope than the other, will so much alter the motion of the particles therein, that they will, after some revolutions, gradually collect together at a distance from the nucleus, and at length separate and become a distinct comet. Such a case having actually occurred under our own eyes, four years ago, with Biela's comet, when it was not at the time under the immediate influence of any planet in particular, nor in any trying part of its orbit round the sun, adds much additional weight to this view of the constitution of such bodies.

This brings us to the second portion of the subject, viz., the corrections which should be applied to apparent observations to deduce the real phenomena.

A comet being an elongated, gaseous, elastic, and semitransparent body, varying in size and density with its distance from the sun, evidently requires many different corrections, according to the point of view and the distance from which it is seen, to give an idea of its real nature at the instant of observation; and needs other corrections, to reduce it from that instant to some other in which it is in a normal condition. This normal condition is plainly in perihelio (though a better general comparison of the volumes of different comets would be obtained by reducing each to its *mean distance*, as they would then be all of much more nearly equal density), and viewed at right angles to the line of its larger axis. This is a position which seems generally to have been taken for granted, though it never occurs even approximately. If we could see a comet at the instant of its perihelio, the plane of its orbit being inclined 90° to that of the ecliptic, and the radius vector being infinitely small, the above view would be nearly obtained, but would gradually

be altered as the comet proceeded in its orbit. As seen from the sun, the comet should always present a circular body, and be equally illuminated on all sides, except in so far as the longer axis is inclined to the plane of the orbit: when the comet retreats towards the aphelion, the point of view from the earth becoming more nearly the same as that from the sun,—the comet should become rounder and rounder, as well as larger; and this is found actually to be the case,—the tails of HALLEY's and those of 1843 and 1844-5. having been, at and about the perihelion passage, narrow, and intense, and becoming at the last instant in which they were seen, wide, round, and diffuse.

This, perhaps, together with the facts of phase and imperfect transparency (axioms 9 and 10), is sufficient to shew the importance of correcting the observations for both the terrestrial and the solar elements of effect on the appearance of a comet, and be able to deduce its normal condition. That there may be other changes operating is very probable, but be that as it may, these effects of geometry, mechanics, and optics, actually exist to a very sensible amount, and their corrections must be applied before we can expect to discover any of the residual causes.

I ought, doubtless, to apologize for having formed opinions different from Sir J. HERSCHEL's last, as he is confessedly the person, above all others, entitled to paramount weight in cometary physics; and it may be that I have not properly understood, and unintentionally have not here sufficiently represented the reasons upon which his old conclusions have been altered, and on which he has thought it allowable to introduce electrical and other forces, to explain phenomena amongst the celestial bodies; and I would therefore refer inquirers to his works themselves. But while on the one side, I hope that what has been here brought forward with regard to the complete body of a comet and its symmetrical and geometrical form, when freed of the effects of phase, may remove one of the objections which he felt to allowing the all-sufficiency of gravity acting on a group of independent molecules; on the other side, I not only allow, but think it extremely probable, that some other effects besides those already mentioned, may legitimately occur; and if heat and rotation on the earth produce our trade-winds and hurricanes, much greater effects may follow the more violent alterations of temperature and velocity of motion in a comet. Further, as confirming a curious feature noticed in HALLEY's comet, by Sir JOHN, after the perihelion passage, viz., a long thin ray posterior to the nucleus, to which it might perform the office, he suggested, of conveying back the vapour driven off in front at perihelion; I would mention, that a ray of the same sort was seen posterior to the nucleus of the great comet of 1843, of extravagant length and excessive thinness, appearing as a very fine line of light, and traceable for many degrees up the tail: in these particulars, bearing some relation, perhaps, to the excentricity of the orbit, and to the

great length and small breadth of the comet itself. The first day that I saw the comet, three days after perihelio, it was not visible; but, clearly seen on every subsequent day, it existed until the whole was lost in faintness.

The observation of these bodies is not, however, as hinted above, in a sufficiently satisfactory state for rigidly testing any calculable theory. This depends not only on the rarity of the appearance of comets (a matter beyond our control); but also on the insufficient means with which, and the untoward circumstances under which they are generally observed. The telescopes usually turned upon them, have been of so small an optical power, that they would have been considered utterly incompetent for ascertaining the nature of nebulæ high up in the sky; how much more so, when employed on nebulous objects close to the horizon, as the comets usually are, flickering and faint in vapour and smoke, and almost overpowered by the strength of the twilight.

But a sufficiently powerful telescope need not any longer be a difficulty, since the publication of the inventions of the Earl of Rosse, and Messrs Lassel and Nasmyth; and the effect of the vapour of the horizon, and the glare of twilight, might be successfully overcome, by establishing an observatory on high land within the tropics, where the geographical position renders the twilight short even to the plains; and the rarity of mountain air would still further reduce the reflective power of the atmosphere. Micrometrical measures, with such instruments, and under such circumstances, should be combined with *photometrical determinations of the brightness of the various parts of the comet*, and *of the background of the sky*. The former observations are easy and straight forward enough, but the latter are difficult and new; the zero must inevitably be taken from a stellar scale, but none such exists at present; for the telescope measure has invariably failed whenever employed for the purpose, and the eye is still thought the best available mean. Hence, none but the brightest stars have had their magnitudes determined, and that but coarsely, while the great question still remains in much the same state as that in which the application of the telescope to divided instruments was in, before men had learnt how to determine the error of collimation. They knew that there were vast powers of accuracy in the optic tubes, but were afraid of great and mysterious errors, which they neither exactly understood, nor saw how to correct. Similarly in photometry, a telescope of large aperture, is confessed to have a larger scale and range than the unassisted eye, but is suspected of misleading to a greater extent.

This is hardly the place for entering into such an experimental branch of practical astronomy; for pointing out what appears to be the error of the methods adopted by others; and for shewing the correctness and efficacy, as I believe, of another plan, which might be adapted to telescopes of any size. But there can

be little doubt, from the pressing nature of the demand for advance in this department, that those who are more fortunately situated, must, before long, perfect and employ some method by which the great desideratum for inquiry into cometary physics may be obtained, viz., *drawings*, wherein every feature represented shall be accompanied by numerical statements of its dimensions and brightness, with the *probable error* of each determination. Until this shall have been done, the necessary corrections to reduce the apparent to the real phenomena cannot be undertaken, and I shall therefore hope to return to the subject if it should not be taken up in the meantime by any one better able to conduct it to a successful issue.

VII.—*On the Mechanical Action of Heat, especially in Gases and Vapours.* By William John Macquorn Rankine, Civil Engineer, F.R.S.E., F.R.S.S.A., &c.

(Read 4th February 1850.)

INTRODUCTION.

SUMMARY OF THE PRINCIPLES OF THE HYPOTHESIS OF MOLECULAR VORTICES, AND ITS APPLICATION TO THE THEORY OF TEMPERATURE, ELASTICITY, AND REAL SPECIFIC HEAT.

The ensuing paper forms part of a series of researches respecting the consequences of an hypothesis called that of Molecular Vortices, the object of which is, to deduce the laws of elasticity, and of heat as connected with elasticity, by means of the principles of mechanics, from a physical supposition consistent and connected with the theory which deduces the laws of radiant light and heat from the hypothesis of undulations. Those researches were commenced in 1842, and after having been laid aside for nearly seven years, from the want of experimental data, were resumed in consequence of the appearance of the experiments of M. Regnault on gases and vapours.

The investigation which I have now to describe, relates to the mutual conversion of heat and mechanical power by means of the expansion and contraction of gases and vapours.

In the introduction which I here prefix to it, I purpose to give such a summary of the principles of the hypothesis as is necessary to render the subsequent investigation intelligible.

The fundamental suppositions are the following:—

First,—That each atom of matter consists of a nucleus, or central physical point, enveloped by an elastic atmosphere, which is retained in its position by forces attractive towards the nucleus or centre.

Suppositions similar to this have been brought forward by Franklin, Æpinus, Mossotti, and others. They have in general, however, conceived the atmosphere of each nucleus to be of variable mass. I have treated it, on the contrary, as an essential part of the atom. I have left the question indeterminate, whether the nucleus is a small body of a character distinct from that of the atmosphere, or merely a portion of the atmosphere in a highly condensed state, owing to the mutual attraction of its parts.

According to this first supposition, the boundary between two contiguous atoms of a body is an imaginary surface at which the attractions of all the atomic

centres of the body balance each other; and the elasticity of the body is made up of two parts: *First*, the elasticity of the atomic atmospheres at the imaginary boundaries of the atoms, which I shall call the superficial-atomic elasticity; and, *secondly*, the force resulting from the mutual actions of distinct atoms. If the atmospheres are so much condensed round their nuclei or centres, that the superficial-atomic elasticity is insensible, and that the resultants of the mutual actions of all parts of the distinct atoms are forces acting along the lines joining the nuclei or centres, then the body is a perfect solid, having a tendency to preserve not only a certain bulk, but a certain figure; and the elasticity of figure, or *rigidity*, bears certain definite relations to the elasticity of volume.

If the atmospheres are less condensed about their centres, so that the mutual actions of distinct atoms are not reducible to a system of forces acting along the lines joining the atomic centres, but produce merely a cohesive force sufficient to balance the superficial-atomic elasticity, then the condition is that of a *perfect liquid*; and the intermediate conditions between this and perfect solidity constitute the gelatinous, plastic, and viscous states.

When the mutual actions of distinct atoms are very small as compared with the superficial-atomic elasticity, the condition is that of *gas* or *vapour*; and when the substance is so far rarefied that the influence of the atomic nuclei or centres in modifying the superficial elasticity of their atmospheres is insensible, it is then in the state of *perfect gas*.

So far as our experimental knowledge goes, the elasticity of a perfect gas at a given temperature varies simply in proportion to its density. I have therefore assumed this to be the law of the elasticity of the atomic atmospheres, ascribing a specific coefficient of elasticity to each substance.

The *second supposition*, being that from which the hypothesis of molecular vortices derives its name, is the following:—*That the elasticity due to heat arises from the centrifugal force of revolutions or oscillations among the particles of the atomic atmospheres; so that quantity of heat is the* vis viva *of those revolutions or oscillations.*

This supposition appears to have been first definitely stated by Sir Humphry Davy. It has since been supported by Mr Joule, whose valuable experiments to establish the convertibility of heat and mechanical power are well known. So far as I am aware, however, its consequences have not hitherto been mathematically developed.

To connect this hypothesis with the undulatory theory of radiation, I have introduced a *third supposition*:—*That the medium which transmits light and radiant heat consists of the* nuclei *of the atoms, vibrating independently, or almost independently, of their atmospheres*;—so that the absorption of light and of radiant heat, is the transference of motion from the nuclei to their atmospheres, and the

emission of light and of radiant heat, the transference of motion from the atmospheres to their nuclei.

Although in all undulations of sensible length and amplitude, such as those of sound, the nuclei must carry their atmospheres along with them, and vibrating thus loaded, produce a comparatively slow velocity of propagation; yet in all probability the minute vibrations of light and radiant heat may be performed by the atomic nuclei in transparent and diathermanous bodies, without moving the atmospheres more than by that amount which constitutes absorption; and those vibrations will therefore be transmitted according to the laws of the elasticity of *perfect solids*, and with a rapidity corresponding to the extreme smallness of the masses set in motion, as compared with the mutual forces exerted by them.

This supposition is peculiar to my own view of the hypothesis, and is, in fact, the converse of the idea hitherto adopted, of an ether surrounding ponderable particles.

The second and third suppositions involve the assumption, that motion can be communicated between the nuclei and their atmospheres, and between the different parts of the atmospheres; so that there is a tendency to produce some permanent condition of motion, which constitutes equilibrium of heat. It is now to be considered what kind of motion is capable of producing increase of elasticity, and what are the conditions of permanency of that motion.

It is obvious, that the parts of the atomic atmospheres may have motions of alternate expansion and contraction, or of rectilinear oscillation about a position of equilibrium, without affecting the superficial atomic elasticity, except by small periodical changes. Should they have motions, however, of *revolution* about centres, so as to form a group of *vortices*, the centrifugal force will have the effect of increasing the density of the atmosphere at what I have called the bounding surfaces of the atoms, and thus of augmenting the elasticity of the body.

In this summary, I shall not enter into the details of mathematical analysis, but shall state results only. The following, then, are the conditions which must be fulfilled, in order that a group of vortices, of small size as compared with the bulk of an atom, and of various diameters, may permanently coexist, whether side by side, or end to end, in the atomic atmospheres of one substance, or of various substances mixed.

First, The mean elasticity must vary continuously; which involves the condition, that at the surface of contact of two vortices of different substances, side by side, or end to end, the respective densities at each point of contact must be inversely proportional to the coefficients of elasticity. Hence *the specific gravities of the* atmospheric parts of *all substances, under precisely similar circumstances as to heat and molecular forces* (a condition realised in perfect gases at the same pressure and temperature), *are inversely proportional to the coefficients of atmospheric elasticity.* Therefore let μ represent the mass of the atmosphere of one atom of

any substance, b its coefficient of elasticity, and n the number of atoms which, in the state of perfect gas, occupy unity of volume under unity of pressure at the temperature of melting ice;—then

$$n \mu b \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{I.})$$

is a constant quantity for all substances.

Secondly, The superficial elasticity of a vortex must *not* be a function of its diameter: to fulfil which condition, the linear velocity of revolution must be equal throughout all parts of each individual vortex.

Thirdly, In all contiguous vortices of the same substance, the velocities of revolution must be equal; and in contiguous vortices of different substances, the squares of the velocities must be proportional to the coefficients of elasticity of the molecular atmospheres.

The second and third conditions are those of equilibrium of heat, and are equivalent to this law:—

Temperature *is a function of the square of the velocity of revolution in the molecular vortices divided by the coefficient of elasticity of the atomic atmospheres*;—or

$$\text{Temperature} = \phi \left(\frac{w^2}{b}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{II.})$$

where w represents that velocity.

The *mean* elasticity which a vortex exerts endways is not affected by its motion, being equal to

$$b \varrho \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{III.})$$

where ϱ is its mean density. The superficial elasticity at its lateral surfaces, however, is expressed by

$$\frac{w^2 \varrho}{2g} + b \varrho \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{IV.})$$

The additional elasticity $\frac{w^2 \varrho}{2g}$, being that which is due to the motion, is independent of the diameter. The divisor g (the force of gravity) is introduced, on the supposition of the density ϱ being measured by weight.

Supposing the atmosphere of an atom to be divided into concentric spherical layers, it may be shewn that the effect of the coexistence of a great number of small vortices in one of those layers whose radius is r, and mean density ϱ, is to give it a centrifugal force, expressed by

$$\frac{w^2}{g r} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\text{V.})$$

which tends to increase the density and elasticity of the atmosphere at the surface, which I have called the boundary of the atom. The layer is also acted upon by the difference between the mean elasticities at its two surfaces, and by the attraction towards the atomic centre; and these three forces must balance each other.

I have integrated the differential equation which results from this condition, for substances in the gaseous state, in which the forces that interfere with the centrifugal force and atmospheric elasticity are comparatively small; and the result is

$$P=b\frac{\mu}{M}D\left(\frac{w^2}{3gb}+1\right)(1-F)+f(D) \quad . \quad . \quad . \quad (VI.)$$

P is the entire elasticity of the gas, and D its mean density. M represents the total mass of an atom, measured by weight, and μ that of its atmospheric part; so that $\frac{\mu}{M}D$ is the mean density of the atomic atmospheres.

$f(D)$ denotes the effect of the mutual actions of separate atoms.

The first term represents the superficial-atomic elasticity. F denotes the effect of the attraction of the nucleus in modifying that elasticity, and can be represented approximately by a converging series, in terms of the negative powers of $\frac{w^2}{3gb}+1$, commencing with the inverse square, the coefficients being functions of the density D.

By using the first term of such a series, and determining its coefficient, and the quantity $f(D)$ empirically, I have obtained formulæ agreeing closely with the results of M. Regnault's experiments on the Expansion of Atmospheric Air, Carbonic Acid, and Hydrogen.

In a perfect gas, the above expression is reduced to

$$P=b\frac{\mu}{M}D\left(\frac{w^2}{3gb}+1\right) \quad . \quad . \quad . \quad . \quad (VII.)$$

Let n, as before, denote the number of atoms of a substance which, in the state of perfect gas, occupy unity of volume under unity of pressure at the temperature of melting ice, so that $n\,M$ is its specific gravity in that state: then

$$P=\frac{D}{n\,M}n\,\mu\,b\left(\frac{w^2}{3gb}+1\right) \quad . \quad . \quad . \quad . \quad (VIII.)$$

The factor by which $\frac{D}{n\,M}$ is here multiplied fulfils the condition of being a function of $\frac{w^2}{b}$, and of constants which are the same for all substances, and is therefore fitted for a measure of temperature. It obviously varies proportionally to the pressure of a perfect gas of a given density, or its volume under a given pressure.

Let τ, therefore, denote temperature, as measured from an *imaginary zero*, C degrees of the scale adopted, below the temperature of melting ice, at which

$$\frac{w^2}{3gb}+1=0;$$

Then for all substances

$$\tau = \mathrm{C}\, n \mu b \left(\frac{w^2}{3 g b} + 1 \right)$$

and in perfect gases

$$\mathrm{P} = \frac{\tau}{\mathrm{C}} \frac{\mathrm{D}}{n \mathrm{M}} \qquad \text{. . . (IX.)}$$

τ may be termed *absolute temperature*, and the point from which it is measured, the *absolute zero of temperature*. This, as I have observed, is an imaginary point, being lower than the absolute zero of heat by the quantity $\mathrm{C}\, n \mu b$, which is the same for all substances.

The value of C, or the absolute temperature of melting ice, as determined from M. REGNAULT'S experiments, is

274°·6 centigrade,

being the reciprocal of

0·00364166 per centigrade degree,

the value to which the coefficients of dilatation of gases at the temperature of melting ice approximate as they are rarefied.

For FAHRENHEIT'S scale C = 494°·28.

In the sequel I shall represent temperatures measured from that of melting ice by

$$\mathrm{T} = \tau - \mathrm{C}$$

We have now to consider the absolute quantity of heat, or of molecular *vis viva*, which corresponds to a given temperature in a given substance. It is obvious that

$$\frac{\mu w^2}{2 g}$$

represents, in terms of gravity, the portion of *vis viva*, in one atom, due to the molecular vortices; but besides the vortical motion, there may be oscillations of expansion and contraction, or of rectilinear vibration about a position of equilibrium. The velocity with which these additional motions are performed will be in a permanent condition, when the mean value of its square, independent of small periodic changes, is equal throughout the atomic atmosphere. We may therefore represent by

$$\frac{\mu v^2}{2 g} = k \frac{\mu w^2}{2 g} \qquad \text{. . . . (X.)}$$

the total *vis viva* of the atomic atmosphere. To this we have to add that of the nucleus, raising the quantity of heat in one atom to

$$\frac{\mathrm{M} v^2}{2 g} = q \qquad \text{. . . . .}$$

While the quantity of heat in unity of weight is

$$\frac{v^2}{2 g} = \mathrm{Q} \qquad \text{. . . . .} \qquad \text{(XI.)}$$

The coefficient k (which enters into the value of specific heat) being the ratio of the *vis viva* of the entire motion impressed on the atomic atmospheres by the action of their nuclei, to the *vis viva* of a peculiar kind of motion, may be conjectured to have a specific value for each substance depending in a manner as yet unknown on some circumstance in the constitution of its atoms. Although it varies in some cases for the same substance in the solid, liquid, and gaseous states, there is no experimental evidence that it varies for the same substance in the same condition. In the investigation which follows, therefore, I have treated it as sensibly constant.

The following, then, are the expressions for quantity of heat in terms of temperature. In one atom :—

$$q = \frac{v^2}{2g} \mathrm{M} = \frac{3k\mathrm{M}}{2\mathrm{C}n\mu}(\tau - \mathrm{C}n\mu b)$$

In unity of weight :—

$$\mathrm{Q} = \frac{v^2}{2g} = \frac{3k}{2\mathrm{C}n\mu}(\tau - \mathrm{C}n\mu b) \qquad \text{(XII.)}$$

Real specific heat is consequently expressed by the following equations :—

For one atom :—

$$\frac{dq}{d\tau} = \frac{3k\mathrm{M}}{2\mathrm{C}n\mu} \quad . \quad . \quad . \quad . \quad .$$

For unity of weight :—

$$\frac{d\mathrm{Q}}{d\tau} = \frac{3k}{2\mathrm{C}n\mu} \quad . \quad . \quad . \quad . \quad . \qquad \text{(XIII.)}$$

For so much of a perfect gas as occupies unity of volume under unity of pressure at the temperature of melting ice :—

$$n\frac{dq}{d\tau} = \frac{3k\mathrm{M}}{2\mathrm{C}\mu} \quad . \quad . \quad . \quad . \quad . \quad .$$

The laws established experimentally by DULONG, that the specific heats of simple atoms, and of certain groups of compound atoms, bear to each other simple ratios, generally that of equality, and that the specific heats of equal volumes of all simple gases are equal, shew that the specific factor $\frac{k\mathrm{M}}{\mu}$ depends on the chemical constitution of the atom, and thus confirm the conjecture I have stated respecting the coefficient k.

As I shall have occasion, in the investigation which follows, to refer to and to use the equation for the elasticity of vapours in contact with their liquids, which I published in the *Edinburgh New Philosophical Journal for July* 1849, I shall here state generally the nature of the reasoning from which it was deduced.

The equilibrium of a vapour in contact with its liquid depends on three conditions.

First, The total elasticity of the substance in the two states must be the same.

Secondly, The superficial atomic elasticity must vary continuously; so that if at the surface which reflects light there is an abrupt change of density (which seems almost certain), there must there be two densities corresponding to the same superficial-atomic elasticity.

Thirdly, The two forces, which act on each stratum of vapour parallel to the surface of the liquid, namely, the preponderance of molecular attraction towards the liquid, and the difference of the superficial-atomic elasticities at the two sides of the stratum, must be in equilibrio.

Close to the surface of the liquid, therefore, the vapour is highly condensed. The density diminishes rapidly as the distance from the liquid increases, and at all appreciable distances has a sensibly uniform value, which is a function of the temperature and of certain unknown molecular forces.

The integration of a differential equation representing the third condition of equilibrium, indicates the *form* of the approximate equation.

$$\text{Log P} = \alpha - \frac{\beta}{\tau} - \frac{\gamma}{\tau^2} \quad . \quad . \quad . \quad \text{(XIV.)}$$

The coefficients of which have been determined empirically by three experimental data for each fluid. For proofs of the extreme closeness with which the formulæ thus obtained agree with experiment, I refer to the Journal in which they first appeared.

I annex a table of the coefficients for water, alcohol, ether, turpentine, petroleum, and mercury, in the direct equation, and also in the inverse formula,

$$\frac{1}{\tau} = \sqrt{\frac{\alpha - \log \text{P}}{\gamma} + \frac{\beta^2}{4\gamma^2}} - \frac{\beta}{2\gamma} \quad . \quad . \quad \text{(XV.)}$$

by which the temperature of vapour at saturation may be calculated from the pressure. In the ninth and tenth columns are stated the limits between which the formulæ have been compared with experiment.

For turpentine, petroleum, and mercury, the formula consists of two terms only,

$$\text{Log P} = \alpha - \frac{\beta}{\tau} \quad . \quad . \quad . \quad \text{(XVI.)}$$

the small range of the experiments rendering the determination of γ impossible.

TABLE *of the Constants in the Formulæ for the Elasticities of Vapours in contact with their Liquids.*

(1.) Names of the Fluids.	(2.) Scale of Pressures.	(3.) Scale of Temperatures.	(4.) α.	(5.) Log. β.	(6.) Log. γ.	(7.) $\frac{\beta}{2\gamma}$	(8.) $\frac{\beta^2}{4\gamma^2}$	(9.) Range of Temperatures.	(10.) Range of Pressures.
Water	Millimètres of Mercury.	Centigrade.	7·831247	3·1851091	5·0827176	·0063294	·00004006	Centigrade. −30° to +230°	Millimètres. 0·35 to 20945
Do.	Inches of Mercury.	Fahrenheit.	6·426421	3·4403816	5·5932626	·0035163	·000012364	Fahrenheit. −22° to +446°	Inches. 0·014 to 824·63
Alcohol ..... sp. gr. 0·813...	do.	do.	6·16620	3·3165220	5·7602709	·0017998	·000003239	+32° to 264°	0·41 to 165·58
Ether ..... boiling at 105° F.	do.	do.	5·33590	3·2084573	5·5119893	·0024856	·000006178	105° to 210°	30·00 to 163·27
Ether ... . boiling at 104° F.	do.	do.	5·44580	3·2571312	5·3962460	·0036296	·000013174	34° to 104°	6·20 to 30·00
Turpentine	do.	do.	5·98187	3·5380701	.....	.....	.....	304° to 362°	30·00 to 62·24
Petroleum	do.	do.	6·19451	3·5648490	.....	.....	.....	316° to 375°	30·00 to 64·50
Mercury..... ..	Millimètres of Mercury.	Centigrade.	7·5305	3·4685511	.....	.....	.....	Centigrade. 72°·74 to 358°	Millimètres. 0·115 to 760
Do.	Inches of Mercury.	Fahrenheit.	6·1259	3·7238236	.....	.....	.....	Fahrenheit. 162°·93 to 676°·4	Inches. ·0046 to 29·92

The following are some additional values of the constant α for steam, corresponding to various units of pressure used in practice.

Units of Pressure.	Values of α.
Atmospheres of 760 millimètres of mercury, =29·922 inches of mercury, =14·7 lb. on the square inch, =1·0333 kilogrammes on the square centimètre, . .	4·950433
Atmospheres of 30 inches of mercury, =761·99 millimètres, =14·74 lb. on the square inch, =1·036 kilogrammes on the square centimètre, . .	4·949300
Kilogrammes on the square centimètre, ...	4·964658
Kilogrammes on the circular centimètre, ...	4·859748
Pounds avoirdupois on the square inch, ...	6·117662
Pounds avoirdupois on the circular inch, ...	6·012752
Pounds avoirdupois on the square foot, ...	8·276025

All the numerical values of the constants are for common logarithms.

Section I.—Of the Mutual Conversion of Heat and Expansive Power.

(1.) The quantity of heat in a given mass of matter, according to the hypothesis of molecular vortices, as well as every other hypothesis which ascribes the phenomena of heat to motion, is measured by the mechanical power to which that motion is equivalent, that being a quantity the total amount of which in a given system of bodies cannot be altered by their mutual actions, although its distribution and form may be altered. This is expressed in Equation XII. of the Introduction, where the quantity of heat in unity of weight, Q, is represented by the height $\frac{v^2}{2g}$, from which a body must fall in order to acquire the velocity of the molecular oscillations. This height, being multiplied by the weight of a body, gives the mechanical power to which the oscillations constituting its heat are equivalent. The real specific heat of unity of weight, as given in Equation XIII. of the Introduction,

$$\frac{dQ}{d\tau} = \frac{3k}{2Cn\mu}$$

represents the *depth of fall*, which is equivalent to *one degree of rise of temperature* in any given weight of the substance under consideration.

We know, to a greater or less degree of precision, the ratios of the specific heats of many substances to each other, and they are commonly expressed by taking that of water at the temperature of melting ice as unity; but their actual mechanical values have as yet been very imperfectly ascertained, and, in fact, the data necessary for their determination are incomplete.

(2.) Mr Joule, indeed, has made several very interesting series of experiments, in order to ascertain the quantity of heat developed in various substances

by mechanical power employed in different ways, viz., by electric currents excited by the rotation of a magnet, by the forcing of water through narrow tubes, by the agitation of water and oil with a paddle, by the compression of air, and by the friction of air rushing through a narrow orifice. The value of the depth of fall equivalent to a rise of one degree of FAHRENHEIT's scale in the temperature of a mass of water, as determined by that gentleman, varies, in the different series of experiments, between the limits of 760 feet and 890 feet, the value in which Mr JOULE appears to place the greatest confidence being about 780 feet.

Although the smallness of the differences of temperature measured in those experiments renders the numerical results somewhat uncertain, it appears to me that, as evidence of the convertibility of heat and mechanical power, they are unexceptionable. Nevertheless, there is reason to believe that the true mechanical equivalent of heat is considerably less than any of the values deduced from Mr JOULE's experiments; for in all of them there are causes of loss of power, the effect of which it is impossible to calculate. In all machinery, a portion of the power which disappears is carried off by waves of condensation and expansion, along the supports of the machine, and through the surrounding air: this portion cannot be estimated, and is, of course, not operative in producing heat within the machine. It is also impossible to calculate, where friction is employed to produce heat, what amount of it has been lost in the production of electricity, a power which is, no doubt, convertible into heat, but which, in such experiments, probably escapes without undergoing that conversion. To make the determination of the mechanical equivalent of heat by electro-magnetic experiments correct, it is necessary that the whole of the mechanical power should be converted into magnetic power, the whole of the magnetic power into what are called electric currents, and the whole of the power of the electric currents into heat, not one of which conditions is likely to be exactly fulfilled. Even in producing heat by the compression of air, it must not be assumed that the whole of the mechanical power is expended in raising the temperature.

(3.) The best means of determining the mechanical equivalent of heat are furnished by those experiments in which no machinery is employed. Of this kind are experiments on the velocity of sound in air and other gases, which, according to the received and well-known theory of LAPLACE, is accelerated by the heat developed by the compression of the medium.

The accuracy of this theory has lately been called in question. There can be no doubt that it deviates from absolute exactness, in so far that the magnitude of the displacements of the particles of air is neglected in comparison with the length of a wave. It appears to me, however, that the Astronomer-Royal, in his remarks on the subject in the *London and Edinburgh Philosophical Magazine for July* 1849, has shewn, in a satisfactory manner, that although the effect of the appreciable magnitude of those displacements, as compared with the length of a

wave of sound, is to alter slowly the form of the function representing the wave, still that effect is not sufficiently great to make LAPLACE's theory practically erroneous. I have, therefore, in the sequel, adhered to the experiments of DULONG, and to those quoted by POISSON, on the velocity of sound, as the best data for determining the mechanical equivalent of heat.

(4.) The expression already given for the real specific heat of unity of weight of a given substance may be resolved into two factors, thus:—

$$\frac{d\,\mathrm{Q}}{d\,\tau} = \frac{1}{\mathrm{C}\,n\,\mathrm{M}} \times \frac{3\,k\,\mathrm{M}}{2\,\mu} \quad . \quad . \quad . \quad (1.)$$

The first factor, $\frac{1}{\mathrm{C}\,n\,\mathrm{M}}$, may be considered in general as a known quantity; for C represents, as already stated, 274·6 centigrade degrees, the absolute temperature of melting ice, and $n\,\mathrm{M}$ the theoretical weight, in the perfectly gaseous state, of unity of volume of the substance, under unity of pressure, at that temperature; or what is the same thing, $\frac{1}{n\,\mathrm{M}}$ is the height of an imaginary column of the substance, of uniform density, and at the temperature of melting ice, whose pressure by weight upon a given area of base is equal to its pressure by elasticity, supposing it to be perfectly gaseous. The determination of the ratio $\frac{3\,k\,\mathrm{M}}{2\,\mu}$ is necessary, to complete the solution of the problem.

(5.) The relation now to be investigated between heat and mechanical power, is that which exists between the power expended in compressing a body into a smaller volume, and the increase of heat in consequence of such a compression, and conversely, between the heat which disappears, or, as it is said, becomes *latent*, during the expansion of a body to a greater volume, and the mechanical power gained or developed by that expansion. Those phenomena, according to the hypothesis now under consideration, as well as every hypothesis which ascribes heat to motion, are simply the transformation of mechanical power from one shape into another.

It is obvious, in the first place, without the aid of algebraical symbols, that the general effect of the compression of an oscillating atomic atmosphere, or molecular vortex, must be to accelerate its motion, and of its dilatation, to retard its motion; for every portion of such an atmosphere is urged towards the nucleus or atomic centre by a centripetal force equal to the centrifugal force arising from the oscillation; so that when, by compression, each portion of the atmosphere is made to *approach* the centre by a given distance, the *vis viva* of its motion will be *increased* by the amount corresponding to the centripetal force acting through that distance; and conversely, when by expansion each portion of the atmosphere is made to *retreat* from the centre, the *vis viva* of its motion will be *diminished* by a similar amount.

It is not, however, to be taken for granted, that *all* the power expended in

compressing a body appears in the form of heat. More or less power may be consumed or developed by changes of molecular arrangement, or of the internal distribution of the density of the atomic atmospheres; and changes of molecular arrangement or distribution may develope or consume heat, independently of changes of volume.

(6.) We shall now investigate, according to the hypothesis of molecular vortices, the amount of heat produced by an indefinitely small compression of one atom of a body in that state of perfect fluidity which admits of the bounding surface of the atom being treated as if it were spherical: its radius being denoted by R, and the radius of any internal spherical layer of the atmosphere by multiplying R by a fraction u.

I shall denote by the ordinary symbol of differentiation d, such changes as depend on the various positions of portions of the atomic atmosphere relatively to each other, when changes of volume and temperature are not taken into consideration; while by the symbol δ of the calculus of variations, I shall represent such changes as arise from the variations of volume and temperature.

Let us consider the case of an indefinitely thin spherical layer of the atomic atmosphere, whose distance from the nucleus is $\mathrm{R}u$, its thickness $\mathrm{R}\,du$, its area $4\pi\mathrm{R}^2u^2$, and its density $\frac{\mu}{\mathrm{M}}\mathrm{D}\,\psi(u, \mathrm{D}, \tau)$.

The weight, then, of this layer is

$$4\pi\mathrm{R}^3\frac{\mu}{\mathrm{M}}\mathrm{D}\,u^2\,\psi(u, \mathrm{D}, \tau)\,du.$$

Its velocity of oscillation is v, and having, in virtue of that velocity, a mean centrifugal force, as explained in the Introduction (Equation V.), equal to

$$\text{its weight} \times \left(\frac{v^2}{g\,k\,\mathrm{R}\,u} = \frac{2\,\mathrm{Q}}{k\,\mathrm{R}\,u}\right)$$

it is kept in equilibrio by an equal and opposite centripetal force, arising from attraction and elastic pressure, which is consequently represented by

$$4\pi\mathrm{R}^2\frac{\mu}{\mathrm{M}}\frac{v^2}{g\,k}\mathrm{D}\,u\,\psi(u, \mathrm{D}, \tau)\,du$$

$$= 8\pi\mathrm{R}^2\frac{\mu}{k\,\mathrm{M}}\mathrm{Q}\,\mathrm{D}\,u\,\psi(u, \mathrm{D}, \tau)\,du.$$

Let the mean density of the atom now be increased by the indefinitely small quantity $\delta\mathrm{D}$. Then the layer will approach the nucleus through the distance $-\delta(\mathrm{R}u) = -u\,\delta\mathrm{R} - \mathrm{R}\,\delta u$, and being acted upon through that distance by the centripetal force already stated, the *vis viva* of oscillation will be increased by a quantity corresponding to the mechanical power (that is to say, the *heat*), represented by the product of that distance by that force, or by

$$-8\pi\mathrm{R}^2\frac{\mu}{k\,\mathrm{M}}\mathrm{Q}\,\mathrm{D}\,u\,\psi(u, \mathrm{D}, \tau)\,du \times \delta(\mathrm{R}u)$$

$$= -8\pi \mathrm{R}^3 \frac{\mu}{k\mathrm{M}} \mathrm{Q}\,\mathrm{D}\,\psi(u, \mathrm{D}, \tau)\, u^2 \left(\frac{\delta \mathrm{R}}{\mathrm{R}} + \frac{\delta u}{u}\right) du$$

which, because $\frac{\delta \mathrm{R}}{\mathrm{R}} = -\frac{1}{3} \cdot \frac{\delta \mathrm{D}}{\mathrm{D}}$, and $\frac{4\pi \mathrm{R}^3 \mathrm{D}}{3} = \mathrm{M}$ is equal to

$$+\mathrm{Q}\mathrm{M} \cdot \frac{2\mu}{k\mathrm{M}} \psi(u, \mathrm{D}, \tau)\, u^2 \left(\frac{\delta \mathrm{D}}{\mathrm{D}} - 3\frac{\delta u}{u}\right) du.$$

We must suppose that the velocity of oscillation is equalised throughout the atomic atmosphere, by a propagation of motion so rapid as to be practically instantaneous.

Then if the above expression be integrated with respect to du, from $u=0$ to $u=1$, the result will give the whole increase of heat in the atom arising from the condensation $\delta \mathrm{D}$; and dividing that integral by the atomic weight M, we shall obtain the corresponding development of heat in unity of weight. This is expressed by the following equation :—

$$\delta Q' = 2\,\mathrm{Q} \frac{\mu}{k\mathrm{M}} \left\{ \frac{\delta \mathrm{D}}{\mathrm{D}} \int_0^1 du \cdot u^2 \psi(u, \mathrm{D}, \tau) - 3\int_0^1 du \cdot u\, \delta u\, \psi(u, \mathrm{D}, \tau) \right\} \quad . \quad . \quad (2.)$$

The letter Q′ is here introduced to denote, when negative, that heat which is consumed in producing changes of volume and of molecular arrangement, and when positive, as in the above equation, the heat which is produced by such changes.

The following substitutions have to be made in Equation (1.) of this Section.

For Q is to be substituted its value, according to Equation XII. of the Introduction; or abbreviating $\mathrm{C}n\mu b$ into κ:—

$$\mathrm{Q} = \frac{3k}{2\,\mathrm{C}n\mu}(\tau - \kappa) \quad . \quad . \quad . \quad (3.)$$

The value of the first integral in Equation (2.) of this Section is

$$\int_0^1 du \cdot u^2 \psi(u, \mathrm{D}, \tau) = \frac{1}{3}$$

The value of the second integral

$$-3\int_0^1 du \cdot u\, \delta u\, \psi(u, \mathrm{D}, \tau)$$

remains to be investigated. The first step in this inquiry is given by the condition, that whatsoever changes of magnitude a given spherical layer undergoes, the portion of atmosphere between it and the nucleus is invariable. This condition is expressed by the equation

$$0 = \left(\delta u \frac{d}{du} + \delta\tau \frac{d}{d\tau} + \delta \mathrm{D} \frac{d}{d\mathrm{D}}\right) \int_0^u du \cdot u^2 \psi(u, \mathrm{D}, \tau) \quad . \quad . \quad . \quad (4.)$$

from which it follows that

$$\delta u = -\frac{1}{u^2 \psi(u, \mathrm{D}, \tau)} \left(\delta\tau \frac{d}{d\tau} + \delta \mathrm{D} \frac{d}{d\mathrm{D}}\right) \int_0^u du \cdot u^2 \psi(u, \mathrm{D}, \tau)$$

and consequently that

$$-3\int_0^1 du \,.\, u\,\delta u\,\psi(u,\mathrm{D},\tau) =$$
$$+\left(\delta\tau\,.\,\frac{d}{d\tau}+\delta\mathrm{D}\,\frac{d}{d\mathrm{D}}\right)3\int_0^1\frac{du}{u}\int_0^u du\,.\,u^2\,\psi(u,\mathrm{D},\tau)$$

Hence, making

$$9\int_0^1\frac{du}{u}\int_0^u du\,.\,u^2\,\psi(u,\mathrm{D},\tau)=\mathrm{U}\quad .\quad .\quad .\quad (5.)$$

The second integral in Equation (2.) is transformed into

$$+\frac{1}{3}\left(\delta\tau\,\frac{d}{d\tau}+\delta\mathrm{D}\,\frac{d}{d\mathrm{D}}\right)\mathrm{U}.$$

By means of those substitutions we obtain, for the mechanical value of the heat developed in unity of weight of a fluid by any indefinitely small change of volume or of molecular distribution—

$$\left.\begin{aligned}\delta Q&=\frac{\tau-\kappa}{\mathrm{C}n\mathrm{M}}\left(\delta\mathrm{D}\,\left(\frac{1}{\mathrm{D}}+\frac{d\mathrm{U}}{d\mathrm{D}}\right)+\delta\tau\frac{d\mathrm{U}}{d\tau}\right)\\ \text{or taking } \mathrm{V}&=\frac{1}{\mathrm{D}} \text{ to denote the volume of unity of weight of the substance,}\\ \delta Q&=-\frac{\tau-\kappa}{\mathrm{C}n\mathrm{M}}\left(\delta\mathrm{V}\,\left(\frac{1}{\mathrm{V}}-\frac{d\mathrm{U}}{d\mathrm{V}}\right)-\delta\tau\frac{d\mathrm{U}}{d\tau}\right)\end{aligned}\right\}\quad (6.)$$

Of this expression, the portion $\frac{\tau-\kappa}{\mathrm{C}n\mathrm{M}}\cdot\frac{\delta\mathrm{D}}{\mathrm{D}}=-\frac{\tau-\kappa}{\mathrm{C}n\mathrm{M}}\cdot\frac{\delta\mathrm{V}}{\mathrm{V}}$ represents the variation of heat arising from mere change of volume.

$\frac{\tau-\kappa}{\mathrm{C}n\mathrm{M}}\delta\mathrm{V}\frac{d\mathrm{U}}{d\mathrm{V}}=\frac{\tau-\kappa}{\mathrm{C}n\mathrm{M}}\delta\mathrm{D}\frac{d\mathrm{U}}{d\mathrm{D}}$, denotes the variation of heat produced by change of molecular distribution dependent on change of volume.

$\frac{\tau-\kappa}{\mathrm{C}n\mathrm{M}}\delta\tau\frac{d\mathrm{U}}{d\tau}$ expresses the variation of heat due to change of molecular distribution dependent on change of temperature.

(7.) The function U is one depending on molecular forces, the nature of which is as yet unknown. The only case in which it can be calculated directly is that of a perfect gas. Without giving the details of the integration, it may be sufficient to state, that in this case

$$\left.\begin{aligned}\mathrm{U}&=\frac{\kappa}{\tau}\\ \text{and therefore that}\\ \frac{d\mathrm{U}}{d\tau}&=-\frac{\kappa}{\tau^2}\,;\quad \frac{d\mathrm{U}}{d\mathrm{V}}=0\end{aligned}\right\}\quad .\quad .\quad .\quad (7.)$$

In all other cases, however, the value of this function can be determined indirectly, by introducing into the investigation the principle of the *conservation of vis viva*.

Suppose a portion of any substance, of the weight *unity*, to pass through a variety of changes of temperature and volume, and at length to be brought back to its primitive volume and temperature. Then the absolute quantity of heat in the substance, and the molecular arrangement and distribution, being the same as at first, the effect of their changes is eliminated; *and the algebraical sum of the vis viva expended and produced, whether in the shape of expansion and compression, or in that of heat, must be equal to zero*:—that is to say, if, on the whole, any mechanical power has appeared, and been given out from the body, in the form of expansion, an equal amount must have been communicated to the body, and must have disappeared in the form of heat; and if any mechanical power has appeared and been given out from the body in the form of heat, an equal amount must have been communicated to the body, and must have disappeared in the form of compression. This principle expressed symbolically is

$$\Delta \Pi + \Delta Q' = 0 \quad . \quad . \quad . \quad . \quad . \quad (8.)$$

Where Π, when positive, represents expansive power given out, when negative, compressive power absorbed; and Q' represents, when positive, heat given out, when negative, heat absorbed.

To take the simplest case possible, let the changes of temperature and of volume be supposed to be indefinitely small, and to occur during distinct intervals of time, so that τ and V are independent variables. Let the initial absolute temperature be τ, the initial volume V, and the initial total elasticity P; and let the substance go through the following four changes.

First, Let its temperature be raised from τ to $\tau+\delta\tau$, the volume remaining unchanged. Then the quantity of heat absorbed is

$$-\delta\tau\left(\frac{d\,Q}{d\,\tau}-\frac{\tau-\kappa}{C\,n\,M}\,\frac{d\,U}{d\,\tau}\right)$$

and there is no expansion nor compression.

Secondly, Let the body expand, without change of temperature, from the volume V to the volume $V+\delta V$. Then the quantity of heat absorbed is

$$-\delta V\,.\,\frac{\tau+\delta\tau-\kappa}{C\,n\,M}\left(\frac{1}{V}-\frac{d}{d\,V}\,.\,(U+\frac{d\,U}{d\,\tau}\,\delta\,\tau)\right)$$

while the power given out by expansion is

$$\delta V\,(P+\frac{d\,P}{d\,\tau}\,\delta\,\tau)$$

Thirdly, Let the temperature fall from $\tau+\delta\tau$ to its original value τ, the volume $V+\delta V$ continuing unchanged; then the heat given out is

$$+\delta\tau\left(\frac{d\,Q}{d\,\tau}-\frac{\tau-\kappa}{C\,n\,M}\,\frac{d}{d\,\tau}(U+\frac{d\,U}{d\,V}\,\delta\,V)\right)$$

and there is no expansion nor compression.

Fourthly, Let the body be compressed, without change of temperature, to its original volume V; then the heat given out is

$$+\delta V \frac{\tau-\kappa}{C n M}\left(\frac{1}{V}-\frac{dU}{dV}\right)$$

while the power absorbed in compression is

$$-\delta V \,.\, P$$

The body being now restored in all respects to its primitive state, the sum of the two portions of power connected with change of volume, must, in virtue of the principle of *vis viva*, be equal to the sum of the four quantities of heat with their signs reversed. Those additions being made, and the sums divided by the common factor $\delta V \delta \tau$, the following equation is obtained:—

$$\frac{dP}{d\tau}=\frac{1}{C n M}\left(\frac{1}{V}-\frac{dU}{dV}\right) \quad . \quad . \quad . \quad (9.)$$

The integral of this partial differential equation is

$$U=\phi \,.\, \tau+\int dV \left(\frac{1}{V}-C n M \frac{dP}{d\tau}\right) \quad . \quad . \quad . \quad (10.)$$

Now $\phi \,.\, \tau$ being the same for all densities, is the value of U for the perfectly gaseous state, or $\frac{\kappa}{\tau}$; for in that state, the integral = 0.

The values of the partial differential coefficients are accordingly—

$$\left.\begin{aligned}\frac{dU}{dV}&=\frac{1}{V}-C n M \frac{dP}{d\tau}\\ \frac{dU}{d\tau}&=-\frac{\kappa}{\tau^2}-C n M \int dV \,.\, \frac{d^2 P}{d\tau^2}\end{aligned}\right\} \quad . \quad . \quad . \quad (11.)$$

and they can, therefore, be determined in all cases in which the quantity $\kappa=C n \mu b$, and the law of variation of the total elasticity with the volume and temperature are known, so as to complete the data required in order to apply equation 6 of this section to the calculation of the mechanical value of the variations of heat due to changes of volume and molecular arrangement.

The total elasticity of an imperfect gas, according to Equations VI. and XII. of the introduction, being

$$P=\frac{\tau}{C n M V}\left(1-F\left(D,\frac{\tau}{\kappa}\right)\right)+f(D)$$

its first and second partial differential coefficients with respect to the temperature are,—

$$\frac{dP}{d\tau}=\frac{1}{C n M V}\left(1-\left(1+\tau\frac{d}{d\tau}\right) F\left(D,\frac{\tau}{\kappa}\right)\right)$$

$$\frac{d^2 P}{d\tau^2}=-\frac{1}{C n M V}\left(2\frac{d}{d\tau}+\tau\frac{d^2}{d\tau^2}\right) F\left(D,\frac{\tau}{\kappa}\right)$$

Consequently, for the imperfectly gaseous state,

$$\left.\begin{aligned} \mathrm{U} &= \frac{\kappa}{\tau} + \left(1+\tau\frac{d}{d\tau}\right)\int d\mathrm{V}\,.\,\frac{\mathrm{F}\left(\mathrm{D},\frac{\tau}{\kappa}\right)}{\mathrm{V}} \\ \frac{d\,\mathrm{U}}{d\,\mathrm{V}} &= \frac{1}{\mathrm{V}}\cdot\left(1+\tau\frac{d}{d\tau}\right)\mathrm{F}\left(\mathrm{D},\frac{\tau}{\kappa}\right) \\ \frac{d\,\mathrm{U}}{d\,\tau} &= -\frac{\kappa}{\tau^2} + \left(2\frac{d}{d\tau}+\tau\frac{d^2}{d\tau^2}\right)\int d\mathrm{V}\,.\,\frac{\mathrm{F}\left(\mathrm{D},\frac{\tau}{\kappa}\right)}{\mathrm{V}} \end{aligned}\right\}\quad .\quad .\quad (12.)$$

(8.) It is to be observed that the process followed in ascertaining the nature of the function U is analogous to that employed by M. Carnôt in his theory of the motive power of heat, although founded on contrary principles, and leading to different results.

Carnôt, in fact, considers heat to be something of a peculiar kind, whether a condition or a substance, the total amount of which in nature is incapable of increase or of diminution. It is not, therefore, according to his theory, convertible into mechanical power; but is capable, by its transmission through substances under particular circumstances, of causing mechanical power to be developed. He supposes a body to go through certain changes of temperature and volume, and to return at last to its primitive volume and temperature, and conceives, in accordance with his view of the nature of heat, that it must have given out exactly the same quantity of heat that it has absorbed. The transmission of this heat he regards as the cause of the production of an amount of mechanical power, depending on the quantity of heat transmitted and on the temperature at which the transmission has taken place. According to these principles, a body, having received a certain quantity of heat, is capable of giving out not only all the heat it has received, but also a quantity of mechanical power which did not before exist.

According to the theory of this Essay, on the contrary, and to every conceivable theory which regards heat as a modification of motion, no mechanical power can be given out in the shape of expansion unless the quantity of heat emitted by the body in returning to its primitive temperature and volume is *less* than the quantity of heat originally received: the excess of the latter quantity above the former disappearing as heat, to appear as expansive power, so that the sum of the *vis viva* in those two forms continues unchanged.

Section II.—Of Real and Apparent Specific Heat, especially in the State of Perfect Gas.

(9.) The apparent specific heat of a given substance is found by adding to the real specific heat (or the heat which retains its form in producing an elevation of one degree of temperature in unity of weight) that additional heat which disappears in producing changes of volume and of molecular arrangement, and which is determined by reversing the sign of Q^1 in equation 6 of Section I. (so as to transform it from heat evolved to heat absorbed), and taking its *total* differential coefficient with respect to the temperature. Hence, denoting total apparent specific heat by K,—

$$K = \frac{dQ}{d\tau} - \frac{d.Q^1}{d\tau} = \frac{dQ}{d\tau} - \frac{dQ^1}{d\tau} - \frac{dQ^1}{dV} \cdot \frac{dV}{d\tau}$$

$$= \frac{1}{C n M}\left\{\frac{3kM}{2\mu} + (\tau - \kappa)\left(\frac{dV}{d\tau}\left(\frac{1}{V} - \frac{dU}{dV}\right) - \frac{dU}{d\tau}\right)\right\} \quad (13.)$$

Another mode of expressing this coefficient is the following:—

Denote the ratio $\frac{2\mu}{3kM}$ by N,

and the real specific heat by $\mathfrak{k}$

$$= \frac{1}{C n M N} \quad \ldots \quad (14.)$$

Then

$$K = \mathfrak{k}\left\{1 + N(\tau - \kappa)\left(\frac{dV}{d\tau}\left(\frac{1}{V} - \frac{dU}{dV}\right) - \frac{dU}{d\tau}\right)\right\} \quad (15.)$$

The value of $\frac{dV}{d\tau}$ is to be determined from the conditions of each particular case; so that each substance may have a variety of apparent specific heats, according to the manner in which the volume varies with the temperature.

If the volume is not permitted to vary, so that $\frac{dV}{d\tau} = 0$, there is obtained the following result, being the apparent specific heat at constant volume:—

$$K_v = \frac{1}{C n M}\left(\frac{1}{N} - (\tau - \kappa)\frac{dU}{d\tau}\right)$$

$$= \mathfrak{k}\left(1 - N(\tau - \kappa)\frac{dU}{d\tau}\right) \quad \ldots \quad (16.)$$

(10.) When the substance under consideration is a perfect gas, it has already been stated (Eq. 7), that $\frac{dU}{d\tau} = -\frac{\kappa}{\tau^2}, \frac{dU}{dV} = 0$; and because the volume of unity of weight is directly as the absolute temperature and inversely as the pressure,

$$\frac{1}{V}\frac{dV}{d\tau} = \frac{1}{\tau} - \frac{1}{P}\frac{dP}{d\tau} \quad \ldots \quad (17.)$$

Hence the following are the values of the apparent specific heats of unity of weight of a theoretically perfect gas under different circumstances:—

General value of the total apparent specific heat:—

$$\left.\begin{aligned} \mathrm{K} &= \frac{1}{\mathrm{C}\,n\,\mathrm{M}}\left\{\frac{1}{\mathrm{N}} + (\tau-\kappa)\left(\frac{\kappa}{\tau^2} + \frac{d\,\mathrm{V}}{\mathrm{V}\,d\,\tau}\right)\right\} \\ &= \frac{1}{\mathrm{C}\,n\,\mathrm{M}}\left\{\frac{1}{\mathrm{N}} + (\tau-\kappa)\left(\frac{\kappa}{\tau^2} + \frac{1}{\tau} - \frac{d\,\mathrm{P}}{\mathrm{P}\,d\,\tau}\right)\right\} \end{aligned}\right\}$$

Apparent specific heat at constant volume:—

$$\left.\begin{aligned} \mathrm{K_V} &= \frac{1}{\mathrm{C}\,n\,\mathrm{M}}\left\{\frac{1}{\mathrm{N}} + \frac{\kappa}{\tau} - \frac{\kappa^2}{\tau^2}\right\} \\ &= \mathfrak{k}\left(1+\mathrm{N}\left(\frac{\kappa}{\tau} - \frac{\kappa^2}{\tau^2}\right)\right) \end{aligned}\right\}$$

Apparent specific heat under constant pressure:—

$$\left.\begin{aligned} \mathrm{K_P} &= \frac{1}{\mathrm{C}\,n\,\mathrm{M}}\left(\frac{1}{\mathrm{N}} + 1 - \frac{\kappa^2}{\tau^2}\right) \\ &= \mathfrak{k}\left\{1+\mathrm{N}\left(1-\frac{\kappa^2}{\tau^2}\right)\right\} \end{aligned}\right\} \quad \ldots \quad (18.)$$

The ratio of the apparent specific heat under constant pressure to the apparent specific heat at constant volume is the following:—

$$\frac{\mathrm{K_P}}{\mathrm{K_V}} = \frac{1+\mathrm{N}\left(1-\frac{\kappa^2}{\tau^2}\right)}{1+\mathrm{N}\left(\frac{\kappa}{\tau} - \frac{\kappa^2}{\tau^2}\right)} = 1+\mathrm{N}\frac{1-\frac{\kappa}{\tau}}{1+\mathrm{N}\left(\frac{\kappa}{\tau} - \frac{\kappa^2}{\tau^2}\right)} \quad \ldots \quad (19.)$$

The value of κ is unknown; and, as yet, no experimental data exist from which it can be determined. I have found, however, that practically, results of sufficient accuracy are obtained by regarding κ as so small in comparison with τ, that $\frac{\kappa}{\tau}$, and *à fortiori* $\frac{\kappa^2}{\tau^2}$, may be neglected in calculation.

Thus are obtained the following *approximate* results, for perfect gases, and gases which may without material error be treated as perfect.

General value of the total apparent specific heat:—

$$\left.\begin{aligned} \mathrm{K} &= \frac{1}{\mathrm{C}\,n\,\mathrm{M}}\left(\frac{1}{\mathrm{N}} + \frac{\tau}{\mathrm{V}}\frac{d\,\mathrm{V}}{d\,\tau}\right) = \mathfrak{k} + \mathrm{P}\frac{d\,\mathrm{V}}{d\,\tau} \\ &= \frac{1}{\mathrm{C}\,n\,\mathrm{M}}\left(\frac{1}{\mathrm{N}} + 1 - \frac{\tau}{\mathrm{P}}\frac{d\,\mathrm{P}}{d\,\tau}\right) \end{aligned}\right\}$$

Apparent specific heat at constant volume:—

$$\mathrm{K_V} = \frac{1}{\mathrm{C}\,n\,\mathrm{M}\,\mathrm{N}} = \mathfrak{k}$$

being equal to the real specific heat.

Apparent specific heat under constant pressure:—

$$\mathrm{K_P} = \frac{1}{\mathrm{C}\,n\,\mathrm{M}}\left(\frac{1}{\mathrm{N}} + 1\right) = \mathfrak{k}\,(1+\mathrm{N})$$

$$\left.\begin{aligned} &\\ &\\ &\\

Ratio of those two specific heats :—

$$\frac{K_P}{K_V} = 1 + N \quad . \quad . \quad . \quad . \quad (21.)$$

This ratio is the quantity called by Poisson γ, in his researches on the propagation of sound.

(11.) It is unnecessary to do more than to refer to the researches of Poisson, and to those of Laplace, for the proof that the effect of the production of heat by the compression of air is the same as if the elasticity varied in proportion to that power of the density whose index is the ratio of the two specific heats; so that the actual velocity of sound is greater than that which it would have if there were no such development of heat, in the proportion of the square root of that ratio.

The following is the value of the velocity of sound in a gas, as given by Poisson, in the second volume of his *Traité de Mécanique* :—

$$a = \sqrt{g \cdot \gamma \cdot (1 + E T) \frac{m h}{\Delta}} \quad . \quad . \quad . \quad . \quad (22.)$$

where a denotes the velocity of sound, g the velocity generated by gravity in unity of time, E the coefficient of increase of elasticity with temperature, at the freezing point of water, T the temperature measured from that point, m the specific gravity of mercury, Δ that of the gas at the temperature of melting ice, and pressure corresponding to a column of mercury of the height h. It follows that the ratio γ is given by the formula

$$\gamma = 1 + N \text{ nearly} = \frac{a^2 \Delta}{g m h (1 + E T)} \quad . \quad . \quad . \quad (23.)$$

Calculations have been made to determine the ratio γ from the velocity of sound; but as many of them involve erroneous values of the coefficient of elasticity E, the experiments have to be reduced anew.

The following calculation is founded on an experiment quoted by Poisson on the velocity of sound in atmospheric air, the values of E, m, and Δ being taken from the experiments of M. Regnault.

$$a = 340{\cdot}89 \text{ mètres per second.}$$

$$g = 9^{m}{\cdot}80896. \qquad h = 0^{m}{\cdot}76. \qquad T = 15^{\circ}{\cdot}9 \text{ Centigrade.}$$

$$E = 0{\cdot}003665\,; \quad \frac{m}{\Delta} = 10513.$$

Consequently, for atmospheric air,

$$\gamma = 1{\cdot}401.$$

The results of a reduction, according to correct data, of the experiments of Dulong upon the velocity of sound in atmospheric air, oxygen, and hydrogen, are as follows :—

Atmospheric air, ...	$\gamma = 1{\cdot}410$
Oxygen, ...	1·426
Hydrogen, ...	1·426

Thus it appears, that for the simple substances, oxygen and hydrogen, the ratio N is the same, while for atmospheric air it is somewhat smaller.*

(12.) The ordinary mode of expressing the specific heats of gases is to state their ratios to that of an *equal volume* of atmospheric air at the same pressure and temperature.

When $\frac{\kappa}{\tau}$ is a very small fraction, specific heats of *unity of volume* of a perfect gas are given by the equations

$$\left.\begin{aligned} n\,\mathrm{M}\,\mathrm{K}_v &= \frac{1}{\mathrm{C}\,\mathrm{N}} \\ n\,\mathrm{M}\,\mathrm{K}_p &= \frac{1}{\mathrm{C}}\left(\frac{1}{\mathrm{N}}+1\right) \end{aligned}\right\} \quad . \quad . \quad . \quad (24.)$$

That is to say, the specific heat of unity of volume at constant volume is inversely proportional to the fraction by which the ratio of the two specific heats exceeds unity; a conclusion already deduced from experiment by Dulong.

The following is a comparison of the ratios of the apparent specific heats under constant pressure, of unity of volume of oxygen and hydrogen respectively, to that of atmospheric air, as deduced from Equation (24.), with those determined experimentally by De la Roche and Bérard.

Gas.	Ratio $\frac{n\,\mathrm{M}\,\mathrm{K}_p\ (\mathrm{Gas})}{n\,\mathrm{M}\,\mathrm{K}_p\ (\mathrm{Atmos.\ air})}$ By Theory.	By Experiment.
Oxygen, ...	0·973	0·9765
Hydrogen, ...	0·973	0·9033

This comparison exhibits a much more close agreement between theory and experiment than has been hitherto supposed to exist, the errors in the constants employed having had the effect of making the ratio 1 + N seem greater for atmospheric air than for oxygen and hydrogen, while in fact it is smaller.

To treat the other substances on which both M. Dulong and MM. De la

* The following are some additional determinations of the value of γ for atmospheric air, founded upon experiments on the velocity of sound:—

Observers.	T Centigrade.	a Mètres per second.	γ
Bravais and Martins: mean of several experiments at temperatures varying from 5° to 11° centigrade, reduced to 0° (Comptes Rendus, xix.) . .	0°	332·37	1·40955
Moll and Van Beek: reduced to	0°	332·25	1·40853
Stampfer and Myrbach: reduced to 0° (not corrected for moisture)	0°	332·96	1·41456
Académie des Sciences, 1738: (not corrected for moisture)	6°·1	337·10	1·418

A variation of one mètre per second in the velocity of sound at 0° corresponds to a variation of ·0085 in the value of γ.

ROCHE and BÉRARD made experiments as perfect gases, would lead to sensible errors. I have, therefore, confined my calculations for the present to oxygen, hydrogen, and atmospheric air.

(13.) The heat produced by compressing so much of a perfect gas as would occupy unity of volume under the pressure unity, at the temperature 0° centigrade, from its actual volume $n\,\mathrm{M}\,\mathrm{V}_1 = \frac{\tau}{\mathrm{P}\,\mathrm{C}}$ into a volume which is less in a given ratio s (when κ is neglected as compared with τ), is expressed by the following motion :—

$$n\,\mathrm{M}\,\mathrm{Q}' = -\frac{1}{\mathrm{C}}\int_{\mathrm{V}_1}^{s\mathrm{V}_1} d\mathrm{V} \;.\; \frac{\tau}{\mathrm{V}} = -n\,\mathrm{M}\,\mathrm{V}_1\int_1^s \mathrm{P}\,ds \quad . \quad . \quad . \quad (25.)$$

being, in fact, equal to the mechanical power used in the compression. When the temperature is maintained constant, this becomes

$$n\,\mathrm{M}\,\mathrm{Q}'_{(\tau)} = \frac{\tau}{\mathrm{C}}\log_e \;.\; \frac{1}{s} \quad . \quad . \quad . \quad . \quad (26.)$$

which is obviously independent of the nature of the gas.

Hence *equal volumes of all substances in the state of perfect gas, at the same pressure and at equal and constant temperatures, being compressed by the same amount, disengage equal quantities of heat;* a law already deduced from experiment by DULONG.

(14.) The determination of the fraction N affords the means of calculating the mechanical or absolute value of specific heat, as defined by Equation 1, Section First. The data for atmospheric air being taken as follows :—

$$\mathrm{N} = 0{\cdot}4, \qquad \mathrm{C} = 274°{\cdot}6 \text{ centigrade},$$

$\frac{1}{n\,\mathrm{M}}$ = height of an imaginary column of air of uniform density, at the temperature 0° cent., whose pressure by weight on a given base is equal to its pressure by elasticity, ... = 7990 mètres,
= 26214 feet :—

the real specific heat of atmospheric air, or the depth of fall equivalent to one centigrade degree of temperature in that gas, is found to be

$$\mathfrak{k} = \frac{1}{\mathrm{C}\,n\,\mathrm{M}\,\mathrm{N}} = 72{\cdot}74 \text{ mètres} = 238{\cdot}66 \text{ feet} \quad . \quad . \quad . \quad (27.)$$

The apparent specific heat of atmospheric air, under constant pressure, according to DE LA ROCHE and BÉRARD, is equal to that of liquid water at 0° centigrade × 0·2669. The ratio of its real specific heat to the apparent specific heat of water at 0° centigrade, is, therefore,

$$\cdot 2669 \times \frac{1{\cdot}0}{1{\cdot}4} = \cdot 1906,$$

And, consequently, the mechanical value of the apparent specific heat of liquid water, at the temperature of melting ice, is

$$\left.\begin{array}{l}\dfrac{k\,(\text{at. air})}{\cdot 1906} = 381{\cdot}64 \text{ mètres} = 1252 \text{ feet per centigrade degree,} \\ \qquad\qquad \text{or } 695{\cdot}6 \text{ feet per degree of Fahrenheit's scale,}\end{array}\right\} \quad . \quad . \quad . \quad (28.)$$

This quantity we shall denote by K_w. It is the mechanical equivalent of the ordinary *thermal unit*.

I have already pointed out (in Article 2. of the First Section) the causes which tend to make the apparent value of the mechanical equivalent of heat, in Mr JOULE's experiments, greater than the true value. The differences between the result I have just stated, and those at which he has arrived, do not seem greater than those causes are capable of producing, when combined with the uncertainty of experiments, like those of Mr JOULE, on extremely small variations of temperature.

(15.) Besides the conditions of constant volume and constant pressure, there is a third condition in which it is of importance to know the apparent specific heat of an elastic fluid, namely, the condition of vapour at saturation, or in contact with its liquid.

The apparent specific heat of a vapour at saturation, is the quantity of heat which unity of weight of that vapour receives or gives out, while its temperature is increased by one degree, its volume being at the same time compressed so as to bring it to the maximum pressure corresponding to the increased temperature.

It has been usually taken for granted, that this quantity is the same with the variation for one degree of temperature, of what is called the *total heat* of evaporation. Such is, indeed, the case according to the theory of CARNÔT; but I shall shew that, according to the mechanical theory of heat, these two quantities are not only distinct, but in general of contrary signs.

I shall, for the present, consider such vapours only as may be treated in practice as perfect gases, so as to make the first of the Equations (20.) applicable.

It has been shewn that the logarithm of the maximum elasticity of a vapour in contact with its liquid may be represented by the expression

$$\log \mathrm{P} = a - \frac{\beta}{\tau} - \frac{\gamma}{\tau^2}$$

The coefficients a, β, γ, being those adapted for calculating the common logarithm of the pressure, I shall use the accented letters a', β', γ', to denote those suited to calculate the hyperbolic logarithm, being equal respectively to the former coefficients $\times 2{\cdot}3025851$.

Then for vapour at saturation,

$$\frac{d\,\mathrm{P}}{\mathrm{P}\,d\tau} = \frac{\beta'}{\tau^2} + \frac{2\gamma'}{\tau^3} \quad . \quad . \quad . \quad . \quad (29.)$$

Making this substitution in the general Equation (21.), we find the following value for the apparent specific heat of perfectly gaseous vapour at saturation :—

$$\left.\begin{aligned} K_s &= k + P\frac{dV}{d\tau} = k\left(1 + N \cdot \frac{\tau}{V}\frac{dV}{d\tau}\right) \\ &= k\left\{1 + N\left(1 - \frac{\tau}{P}\frac{dP}{d\tau}\right)\right\} \\ &= \frac{1}{C\,n\,M}\left(\frac{1}{N} + 1 - \frac{\beta'}{\tau} - \frac{2\gamma'}{\tau^2}\right) \end{aligned}\right\} \quad . \quad . \quad . \quad (30.)$$

(16.) For the vapours of which the properties are known, the negative terms of this expression exceed the positive at all ordinary temperatures, so that the kind of apparent specific heat now under consideration is a negative quantity:—that is to say, that if a given weight of vapour at saturation is increased in temperature, and at the same time maintained by compression at the maximum elasticity, the heat generated by the compression is greater than that which is required to produce the elevation of temperature, and a surplus of heat is given out; and on the other hand, if vapour at saturation is allowed to expand, and at the same time maintained at the temperature of saturation, the heat which disappears in producing the expansion is greater than that set free by the fall of temperature, and the deficiency of heat must be supplied from without, *otherwise a portion of the vapour will be liquefied, in order to supply the heat necessary for the expansion of the rest.*

This circumstance is obviously of great importance in meteorology, and in the theory of the steam-engine. There is as yet no experimental proof of it. It is true, that, in the working of non-condensing engines, it has been found that the steam which escapes is always at the temperature of saturation corresponding to its pressure, and carries along with it a portion of water in the liquid state; but it is impossible to distinguish between the water which has been liquefied by the expansion of the steam, and that which has been carried over mechanically from the boiler.

The calculation of the proportion of vapour liquefied by a given expansion, requires the knowledge of the latent heat of evaporation, which forms the subject of the next section.

Section III.—Of the Latent and Total Heat of Evaporation, especially for Water.

(17.) The latent heat of evaporation of a given substance at a given temperature, is the amount of heat which disappears in transforming unity of weight of the substance from the liquid state, to that of vapour of the maximum density for the given temperature, being consumed in producing an increase of volume, and an unknown change of molecular arrangement.

It is obvious, that if the vapour thus produced is reconverted into the liquid state *at the same temperature*, the heat given out during the liquefaction must be

equal to that consumed during the evaporation; for as the sum of the expansive and compressive powers, and of those dependent on molecular arrangement during the whole process, is equal to zero, so must the sum of the quantities of heat absorbed and evolved.

The heat of liquefaction, at a given temperature, is therefore equal to that of evaporation, with the sign reversed.

(18.) If to the latent heat of evaporation at a given temperature, is added the quantity of heat necessary to raise unity of weight of the liquid from a certain fixed temperature (usually that of melting ice) to the temperature at which the evaporation takes place, the result is called the total heat of evaporation from the fixed temperature chosen.

According to the theory of CARNÔT, this quantity is called the constituent heat of vapour; and it is conceived, that if liquid at the temperature of melting ice be raised to any temperature and evaporated, and finally brought in the state of vapour to a certain given temperature, the whole heat expended will be equal to the constituent heat corresponding to that given temperature, and will be the same, whatsoever may have been the intermediate changes of volume, or the temperature of actual evaporation.

According to the mechanical theory of heat, on the other hand, the quantity of heat expended must vary with the intermediate circumstances; for otherwise no power could be gained by the alternate evaporation and liquefaction of a fluid at different temperatures.

(19.) The law of the latent and total heat of evaporation is immediately deducible from the principle of the constancy of the total *vis viva* in the two forms of heat and expansive power, when the body has returned to its primitive density and temperature, as already laid down in Article 7.

That principle, when applied to evaporation and liquefaction, may be stated as follows:—

Let a portion of fluid in the liquid state be raised from a certain temperature to a higher temperature: let it be evaporated at the higher temperature: let the vapour then be allowed to expand, being maintained always at the temperature of saturation for its density, until it is restored to the original temperature, at which temperature let it be liquefied:—then *the excess of the heat absorbed by the fluid above the heat given out, will be equal to the expansive power generated.*

To represent those operations algebraically,—let the lower absolute temperature be τ_0:—the volume of unity of weight of liquid at that temperature, v_0, and that of vapour at saturation, V_0: let the pressure of that vapour be P_0: the latent heat of evaporation of unity of weight, L_0; and let the corresponding quantities for the higher absolute temperature τ_1, be v_1, V_1, P_1, L_1. Let K_L represent the mean apparent specific heat of the substance in the liquid form between the temperatures τ_0 and τ_1. Then,—

First, Unity of weight of liquid being raised from the temperature τ_0 to the temperature τ_1, absorbs the heat,

$$K_L (\tau_1 - \tau_0)$$

and produces the expansive power,

$$\int_{v_0}^{v_1} dv \,.\, P$$

Secondly, It is evaporated at the temperature τ_1, absorbing the heat

$$L_1$$

and producing the expansive power,

$$P_1 (V_1 - v_1)$$

Thirdly, The vapour expands, at saturation, until it is restored to the original temperature τ. In this process it absorbs the heat,

$$-\int_{\tau_0}^{\tau_1} d\tau \,.\, K_s$$

and produces the expansive power,

$$\int_{V_1}^{V_0} dV \,.\, P$$

Fourthly, It is liquefied at the original temperature, giving out the heat

$$L_0$$

and consuming the compressive power,

$$P_0 (V_0 - v_0).$$

The equation between the heat which has disappeared, and the expansive power which has been produced, is as follows :—

$$\left.\begin{aligned} &L_1 - L_0 + K_L (\tau_1 - \tau_0) - \int_{\tau_0}^{\tau_1} d\tau \,.\, K_s \\ &= P_1 (V_1 - v_1) - P_0 (V_0 - v_0) + \int_{v_0}^{v_1} dv \,.\, P + \int_{V_1}^{V_0} dV \cdot P \end{aligned}\right\} \quad . \quad . \quad . \quad (31.)$$

If the vapour be such that it can be regarded as a perfect gas without sensible error, the substitution of $k + P\frac{dV}{d\tau}$ for K_s, and of $\frac{\tau}{C\,n\,M} = k\,N\,\tau$ for $P\,V$, transforms the above to

$$\left.\begin{aligned} &L_1 - L_0 + \{K_L - k(1+N)\}(\tau_1 - \tau_0) \\ &= -P_1 v_1 + P_0 v_0 + \int_{v_0}^{v_1} dv \,.\, P = -\int_{P_0}^{P_1} dP \,.\, v \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (32.)$$

In almost all cases which occur in practice, v is so small as compared with V, that $-\int dP \,.\, v$ may be considered as sensibly $= 0$; and therefore (sensibly)

$$L_1 + K_L (\tau_1 - \tau_0) = L_0 + k(1+N)(\tau_1 - \tau_0) \quad . \quad . \quad . \quad (33.)$$

Now this quantity, which I shall denote by H, is the total heat required to raise unity of weight of liquid from τ_0 to τ_1 of absolute temperature, and to evaporate it at the latter temperature. Therefore *the total heat of evaporation, where the vapour may be treated as a perfect gas, increases sensibly at an uniform rate with the temperature of evaporation; and the coefficient of its increase with temperature is equal to the apparent specific heat of the vapour at constant pressure*, $k(1+N)$.

(20.) There have never been any experiments from which the apparent specific heat of steam under constant pressure can be deduced in the manner in which that of permanent gases has been ascertained.

The experiments of M. Regnault, however, prove that the total heat of evaporation of water increases uniformly with the temperature from 0° to 200° centigrade, and thus far fully confirm the results of this theory.

The coefficient of increase is equal to

$$K_w \times 0{\cdot}305$$

Its mechanical value is consequently

116·4 metres = 382 feet per centigrade degree, or
212 feet per degree of Fahrenheit. . . . (34.)

Although the principle of the conservation of *vis viva* has thus enabled us to ascertain the *law of increase* of the total heat of evaporation, it does not enable us to calculate *à priori* the constant L_0 of the formula, being the latent heat of evaporation at the fixed temperature from which the total heat is measured; for the changes of molecular arrangement which constitute evaporation are unknown.

When the fixed temperature is that of melting ice, M. Regnault's experiments give 606·5 centigrade degrees, applied to liquid water as the value of this constant; so that

$$H = K_w\,(606°{\cdot}5 + {\cdot}305\,T^0)$$
For the centigrade scale,
$$H = K_w\,(1091°{\cdot}7 + {\cdot}305\,(T^0 - 32°))$$
For Fahrenheit's scale. . . . (35.)

is the complete expression for the heat required to raise unity of weight of water from the temperature of melting ice to T^0 above the ordinary zero, and to evaporate it at the latter temperature. This formula has been given by M. Regnault as merely empirical; but we have seen that it closely represents the physical law, when quantities depending on the expansion of water are neglected.

It must be remarked, that the unit of heat in M. Regnault's tables is not precisely the specific heat of water at 0° centigrade, but its mean specific heat between the initial and final temperatures of the water in the calorimeter. The utmost error, however, which can arise from this circumstance, is less than $\frac{1}{1200}$ of the total heat of evaporation, so that it may safely be neglected.

The coefficient ·305 K_w=382 feet per centigrade degree is the apparent specific heat of steam at constant pressure; that is to say, for steam,—

$$\left.\begin{aligned} &\text{k}+\frac{1}{C\,n\,M}=382 \text{ feet per centigrade degree,}\\ &\text{but } \frac{1}{C\,n\,M}=153 \text{ feet.}\end{aligned}\right\}$$

Therefore the real specific heat of steam is

$$\left.\begin{aligned} \text{k}&=\frac{1}{C\,n\,M\,N}=229 \text{ feet per centigrade degree,}\\ &=127\cdot4 \text{ feet per deg. of Fahrenheit,}\\ &=K_w\times\cdot183\\ \text{and } N&=\frac{153}{229}=\frac{2}{3}.\end{aligned}\right\} \quad . \; . \; . \quad (36.)$$

The quantity $-\int_{P_0}^{P_1} dP \,.\, v$ has been neglected, as already explained, in these calculations, on account of its smallness. When $\tau_0=C$, or the fixed point is 0° centigrade, this integral is nearly equal to

$$-v\,P_1=-\frac{v}{V_1}\cdot\frac{\tau_1}{C\,n\,M}=-\text{k}\,N\,.\,\frac{v}{V_1}\tau_1 \quad . \; . \; . \quad (37.)$$

which, for steam, is equal to

$$-K_w\times\cdot122\,\frac{v}{V_1}\tau_1.$$

For a pressure of eight atmospheres,

$$\frac{v}{V_1}=\frac{1}{252} \text{ nearly, } \tau_1=445°\cdot5 \text{ (T=170°·9 cent.)}$$

consequently, $$-v\,P_1=-K_w\times0°\cdot22 \text{ cent.}$$

a quantity much less than the limit of errors of observation in experiments on latent heat.

This shews that in practice we are justified in overlooking the influence of the volume of the liquid water on the heat of evaporation.

Section IV.—Of the Mechanical Action of Steam, treated as a Perfect Gas, and the Power of the Steam-Engine.

(21.) In the present limited state of our experimental knowledge of the density of steam at pressures differing much from that of the atmosphere, it is desirable to ascertain whether any material error is likely to arise from treating it as a perfect gas. For this purpose the ratio of the volume of steam at 100° centigrade, under the pressure of one atmosphere, to that of the water which produces it at 4°·1 centigrade, as calculated theoretically on the supposition of steam being a perfect gas, is to be compared with the actual ratio.

The weight of one volume of water at 4·1 centigrade being taken as unity, that of half a volume of oxygen at 0° centigrade, under the pressure of one atmosphere, according to the experiments of M. REGNAULT, is 0·000714900

That of one volume of hydrogen, ... 0·000089578

The sum being . . . . . 0·000804478

The reciprocal of this sum being multiplied by $\frac{374{\cdot}6}{274{\cdot}6} = 1{\cdot}364166$ the ratio of dilatation of a perfect gas from 0° to 100° centigrade) the result gives, for the volume of steam of saturation at 100° centigrade as compared with that of water at

4·°1 . . . . . 1695·72

And for its density, . . 0·00058972

The agreement of those results with the known volume and density of steam is sufficiently close to shew, that at pressures less than one atmosphere, it may be regarded as a gas sensibly perfect; from which it may be concluded, that in the absence of more precise data, the errors arising from treating it as a perfect gas at such higher pressures as occur in practice, will not be of much importance.

Representing, then, by v the volume of unity of weight of water at 4·1 centigrade, that of unity of weight of steam at any pressure and temperature will be given by the formula

$$\mathrm{V} = \frac{1696\, v\, \varpi}{(\tau)} \cdot \frac{\tau}{\mathrm{P}} \quad . \quad . \quad . \quad (38.)$$

ϖ representing the number of units of weight per unit of area in the pressure of one atmosphere, and (τ) the absolute temperature at which the pressure of saturation is one atmosphere; being for the centigrade scale 374·6, and for Fahrenheit's scale 674°·28.

The mechanical action of unity of weight of steam at the temperature τ and pressure P, during its entrance into a cylinder, before it is permitted to expand, is represented by the product of its pressure and volume, or by

$$\mathrm{PV} = \frac{1696\, v\, \varpi}{(\tau)} \cdot \tau \quad . \quad . \quad . \quad (39.)$$

The coefficient $\frac{1696\, v\, \varpi}{(\tau)}$ represents a certain depth of fall per degree of absolute temperature, and is the same with the coefficient $\frac{1}{\mathrm{C}\, n\, \mathrm{M}}$ already referred to. By taking the following values of the factors:—

$v = 0{\cdot}016$ cubic foot per pound avoirdupois,
$\varpi = 2117$ pounds avoirdupois per square foot,

we find this coefficient to be

153·35 feet = 46·74 mètres per centigrade degree,
85·19 feet per degree of Fahrenheit; } . . (40.)

this determination may be considered correct to about $\frac{1}{1200}$ part. When French measures are used in the calculation, the following is the result:—

$$\left.\begin{array}{l} v = 1 \text{ cubic centimètre per gramme,} \\ \varpi = 1033{\cdot}3 \text{ grammes per square centimètre,} \\ \dfrac{1}{C\,n\,M} = 46{\cdot}78 \text{ mètres per centigrade degree,} \\ \qquad = 153{\cdot}48 \text{ feet} \quad \ldots \quad \ldots \quad \ldots \\ \qquad \text{or } 85{\cdot}27 \text{ feet per degree of Fahrenheit.} \end{array}\right\} \quad . \ . \ . \quad (41.)$$

The difference, which is of no practical importance in calculating the power of the steam-engine, arises in the estimation of the density of liquid water.

(22.) Unit of weight of steam at saturation, of the elasticity P_1 and volume V_1 corresponding to the absolute temperature τ_1, being cut off from external sources of heat, it is now to be investigated what amount of power it will produce in expanding to a lower pressure P_2 and temperature τ_2.

It has already been shewn, at the end of the second section, that if vapour at saturation is allowed to expand, it requires a supply of heat from without to maintain it at the temperature of saturation, otherwise a portion of it must be liquefied to supply the heat required to expand the rest. Hence, when unity of weight of steam at saturation, at the pressure P_1 and volume V_1, expands to a lower pressure P, being cut off from external sources of heat, it will not occupy the entire volume V corresponding to that pressure, according to Equation (38.), but a less volume

$$S = m\,V,$$

where m represents the weight of water remaining in the gaseous state, the portion $1-m$ having been liquefied during the expansion of the remainder. The expansive action of the steam will therefore be represented by

$$\int_{V_1}^{S_2} d\,S \,.\, P \quad . \quad . \quad . \quad (42.)$$

The law of variation of the fraction m flows from the following considerations:—

Let δm represent the indefinitely small variation of m corresponding to the indefinitely small change of temperature $\delta\tau$; L, the latent heat of evaporation of unity of weight; K_s, as in Equation (30.), the specific heat of vapour at saturation, which is a negative coefficient varying with the temperature; then we must have

$$-L\,\delta m = m\,K_s\,\delta\tau, \quad \text{or} \quad \frac{\delta m}{m} = -\frac{K_s}{L}\,\delta\tau,$$

in order that the heat produced by the liquefaction of δm may be equal to the heat required to expand m. Hence making, according to Equation (30.)—

$$K_s\,\delta\tau = k\,(\delta\tau + N\frac{\tau}{V}\,\delta V)$$

and
$$\delta\tau = -\frac{\tau}{\mathrm{V}}\,\delta\mathrm{V}\,\frac{1}{\frac{\beta'}{\tau}+\frac{2\gamma'}{\tau^2}-1}$$

we obtain
$$\frac{\delta m}{m} = -\frac{\mathfrak{k}\tau}{\mathrm{L}}\left(\mathrm{N}-\frac{1}{\frac{\beta'}{\tau}+\frac{2\gamma'}{\tau^2}-1}\right)\frac{\delta\mathrm{V}}{\mathrm{V}} \quad . \quad . \quad . \quad (43.)$$

and denoting the coefficient of $\frac{\delta\mathrm{V}}{\mathrm{V}}$ by $-\nu$,

and because
$$\left.\begin{aligned} &\frac{d\log m}{d\log \mathrm{V}} = -\nu\,;\ \frac{d\log \mathrm{S}}{d\log \mathrm{V}} = 1-\nu \\ &\frac{d\log \mathrm{V}}{d\log \mathrm{P}} = -1+\frac{1}{\frac{\beta'}{\tau}+\frac{2\gamma'}{\tau^2}} \\ &\frac{d\log m}{d\log \mathrm{P}} = +\nu\left(1-\frac{1}{\frac{\beta'}{\tau}+\frac{2\gamma'}{\tau^2}}\right) \\ &\frac{d\log \mathrm{S}}{d\log \mathrm{P}} = -(1-\nu)\left(1-\frac{1}{\frac{\beta'}{\tau}+\frac{2\gamma'}{\tau^2}}\right) = -\sigma \end{aligned}\right\} \quad . \quad . \quad . \quad (44.)$$

As the mean temperature of the liquid thus produced more or less exceeds that of the remaining vapour, a small fraction of it will be reconverted into vapour, if the expansion is carried on slowly enough; but its amount is so small, that to take it into account would needlessly complicate the calculation, without making it to any material extent more accurate.

(23.) The extreme complexity of the exponent σ, considered as a function of the pressure P, would render a general formula for the expansive action $\int \mathrm{P}\,d\,\mathrm{S}$ very cumbrous in its application. For practical purposes, it is sufficient to consider the exponent σ as constant during the expansion which takes place in any given engine, assigning it an average value suitable to the part of the scale of pressures in which the expansion takes place. For engines in which the steam is introduced at pressures not exceeding four atmospheres, I conceive that it will be sufficiently accurate to make
$$\sigma = \frac{6}{7};$$
while for engines in which the initial pressure lies between four and eight atmospheres, the suitable value is
$$\sigma = \frac{5}{6}.$$

The utmost error which can arise from using these exponents is about $\frac{1}{100}$ of the whole power of the engine, and that only in extreme cases.

Making, therefore,
$$\mathrm{P} = \mathrm{P}_1\left(\frac{\mathrm{S}}{\mathrm{V}_1}\right)^{-\frac{1}{\sigma}}$$

we obtain for the value of the expansive action of unity of weight of steam,

$$\left.\begin{aligned}\int_{V_1}^{S_2} d\,S \;.\; P &= P_1 V_1 \frac{\sigma}{1-\sigma}\left(1-\left(\frac{S_2}{V_1}\right)^{1-\frac{1}{\sigma}}\right)\\ &= P_1 V_1 \frac{\sigma}{1-\sigma}\left(1-s^{1-\frac{1}{\sigma}}\right)\end{aligned}\right\} \quad . \; . \; . \quad (45.)$$

s being used to denote $\frac{S_2}{V_1}$, or the ratio of the volumes occupied by steam at the end and at the beginning of the expansion respectively.

A table to facilitate the computation is given in the Appendix.

The gross mechanical action of unity of weight of steam on one side of the piston is found by adding to the above quantity the action of the steam before it begins to expand, or $P_1 V_1$, and is therefore

$$P_1 V_1 \left(\frac{1}{1-\sigma} - \frac{\sigma}{1-\sigma} s^{1-\frac{1}{\sigma}}\right) \quad . \; . \; . \quad (46.)$$

The values of the coefficients and exponent being

	$\frac{1}{1-\sigma}$	$\frac{\sigma}{1-\sigma}$	$1-\frac{1}{\sigma}$
For initial pressures between			
1 and 4 atmospheres, ...	7	6	$-\frac{1}{6}$
4 and 8 atmospheres, ...	6	5	$-\frac{1}{5}$

(24.) The following deductions have to be made from the gross action, in order to obtain the action effective in overcoming resistance.

First. For loss of power owing to a portion of the steam being employed in filling steam-passages, and the space called the *clearance* of the cylinder at one end. Let the bulk of steam so employed be the fraction $c\,S_2$ of the space filled by steam at the end of the expansion; then the loss of power from this cause is

$$P_1 c\, S_2 = c\, s\, P_1 V_1.$$

Secondly, For the pressure on the opposite side of the piston, of the steam which escapes into the condenser, or into the atmosphere, as the case may be. Let P_3 be the pressure of this steam; the deduction to be made for its action is

$$P_3 S_2 (1-c) = P_3 V_1 (1-c)\, s.$$

These deductions having been made, there is obtained for the effect of unity of weight of water evaporated,

$$V_1 \left\{ P_1 \left(\frac{1}{1-\sigma} - \frac{\sigma}{1-\sigma} s^{1-\frac{1}{\sigma}} - c\, s\right) - P_3 (1-c)\, s \right\} \quad . \; . \quad (47.)$$

(25.) The effect of the engine in unity of time is found by multiplying the

above quantity by the number of units of weight of water evaporated in unity of time.

If this number be denoted by W,

$$W S_2 (1-c) = W V_1 (1-c) s = A u \quad . \quad . \quad . \quad (48.)$$

will represent the cubical space traversed by the piston in unity of time, A denoting the area of the piston, and u its mean velocity.

Now let the whole resistance to be overcome by the engine be reduced by the principles of statics to a certain equivalent pressure per unit of area of piston, and let this pressure be denoted by R. Then,

$$R A u = R W V_1 (1-c) s \quad . \quad . \quad . \quad (49.)$$

expresses the effect of the engine in terms of the gross resistance.

We have now the means of calculating the circumstances attending the working of a steam-engine according to the principle of the conservation of *vis viva*, or, in other words, of the equality of power and effect, which regulates the action of all machines that move with an uniform or periodical velocity.

This principle was first applied to the steam-engine by the COUNT DE PAMBOUR; and accordingly, the formulæ which I am about to give only differ from those of his work in the expressions for the maximum pressure at a given temperature, and for the expansive action of the steam, which are results peculiar to the theory of this essay.

In the first place, the effect, as expressed in terms of the pressure, is to be equated to the effect as expressed in terms of the resistance, as follows:—

$$R A u = R W V_1 (1-c) s = W V_1 \left\{ P_1 \left(\frac{1}{1-\sigma} - \frac{\sigma}{1-\sigma} s^{1-\frac{1}{\sigma}} - c s \right) - P_3 (1-c) s \right\} \quad . \quad . \quad (50.)$$

This is the fundamental equation of the action of the steam-engine, and corresponds with Equation A. of M. DE PAMBOUR'S theory.

(26.) Dividing both sides of Equation (50.) by the space traversed by the piston in unity of time $W V_1 (1-c) s$, and transferring the pressure of the waste steam, P_3, to the first side, we obtain this equation:—

$$R + P_3 = P_1 \frac{\frac{1}{1-\sigma} - \frac{\sigma}{1-\sigma} s^{1-\frac{1}{\sigma}} - c s}{(1-c) s} \quad . \quad . \quad . \quad (51.)$$

which gives the means of determining the pressure P_1 at which the steam must enter the cylinder, in order to overcome a given resistance and counter-pressure with a given expansion; or supposing the expansion s to be variable at pleasure, and the initial pressure P_1 fixed, the equation gives the means of finding, by approximation, the expansion best adapted to overcome a given resistance and counter-pressure.

The next step is to determine, from Equations XV. of the Introduction and

(38.) of this section, the volume V_1 of unity of weight of steam corresponding to the maximum pressure P_1. Then Equation (48.) gives the space traversed by the piston in unity of time, which, being multiplied by the resistance R per unit of area of piston, gives the gross effect of the engine.

(27.) If, on the other hand, the space traversed by the piston in unity of time is fixed, Equation (48.) gives the means of determining, from the evaporating power of the boiler W, either the volume V_1 of unity of weight of steam required to work the engine at the given velocity with a given expansion, or the expansion s proper to enable steam of a given initial density to work the engine at the given velocity. The initial pressure P_1 being then determined from the volume V_1, the resistance which the engine is capable of overcoming with the given velocity is to be calculated by means of Equation (51.)

(28.) This calculation involves the determination of the pressure P_1 from the volume V_1 of unity of weight of steam at saturation, which can only be done by approximation. The following formula will be found useful for this purpose:—

$$P_1 = \varpi \left(\frac{V_0}{V_1}\right)^{\frac{12}{11}} \quad . \quad . \quad . \quad (52.)$$

where ϖ represents the pressure of one atmosphere, V_0 the volume of steam of saturation at that pressure (being 1696 times the volume of water at 4°·1 cent., or 27·136 cubic feet per pound avoirdupois), and V_1 the volume of steam of saturation at the pressure P_1. This formula is only applicable between the pressures of one and eight atmospheres: that is to say, when the volume of steam is not greater than 27 cubic feet per pound, nor less than 4, and the temperature not lower than 100° centigrade, nor higher than 171° centigrade (which correspond to 212° and 340° Fahrenheit).

The greatest error in computing the pressure by means of this formula is about $\frac{1}{50}$ of an atmosphere, and occurs at the pressure of four atmospheres, so that it is $\frac{1}{200}$ of the whole pressure. This is sufficiently accurate for practice, in calculating the power of steam-engines; but should a more accurate result be required, the approximate value of the pressure may be used to calculate the temperature by means of Equation XV.; and the temperature thus determined (which will be correct to $\frac{1}{5}$ of a centigrade degree) may then be used in conjunction with the volume to compute a corrected value of the pressure, according to Equation (38.) The pressure, as thus ascertained, will be correct to $\frac{1}{2000}$ of its amount, which may be considered the greatest degree of accuracy attainable.

The most convenient and expeditious mode, however, of computing the pressure from the volume, or *vice versâ*, is by interpolation from the table given in the Appendix to this paper.

(29.) The resistance denoted by R may be divided into two parts; that which arises from the *useful work performed*, and that which is independent of it, being,

in fact, the resistance of the engine when unloaded. Now it is evident, that the maximum *useful* effect of the steam has been attained, as soon as it has expanded to a pressure which is in equilibrio with the pressure of the waste steam added to the resistance of the engine when unloaded; for any further expansion, though increasing the total effect, diminishes the useful effect. Therefore if we make

$$R = R' + f,$$

R' being the resistance arising from the useful work, and f the resistance of the engine when unloaded, both expressed in the form of pressure on the piston, the expansion corresponding to the maximum of useful effect will take place when

$$P_2 = P_3 + f \quad . \quad . \quad . \quad .$$

the corresponding ratio of expansion being

$$s = \left(\frac{P_1}{P_3 + f}\right)^{\sigma} \quad . \quad . \quad . \quad . \qquad . \; . \; (53.)$$

The maximum useful effect with a given pressure on the safety-valve has been so fully discussed by M. DE PAMBOUR, that it is unnecessary to do more than to state that it takes place when the initial pressure in the cylinder is equal to that at the safety-valve: that is to say, when it and the useful resistance are the greatest that the safety-valve will permit.

(30.) Annexed is a table of the values of some of the quantities which enter into the preceding equations in the notation of the COUNT DE PAMBOUR'S works.

Expression in the Notation of this paper.	Equivalent Expression in M. DE PAMBOUR'S Notation.
$R = R' + f$	$(1+\delta)\, r + f$
$A\, u$	$a\, v$
W	S × weight of one cubic foot of water.
P_2	p
s	$\frac{l + c}{l' + c}$
c	$\frac{c}{l' + c}$

(31.) As an illustration, I shall calculate the maximum useful effect of one pound, and of one cubic foot of water, in a Cornish double-acting engine, in the circumstances taken by M. DE PAMBOUR as an example for that kind of engine: that is to say,—

Clearance one-twentieth of the stroke, or $c = \frac{1}{21}$

Resistance not depending on the useful load, $f =$ 72 lb. per square foot.
Pressure of condensation, . . . $P_3 =$ 576 lb. ... ...

Consequently to give the maximum useful effect,

$$P_2 = P_3 + f \quad = 648 \text{ lb. per square foot.}$$

Total pressure of the steam when first admitted, $P_1 = 7200$ lb. ... ...

Volume of 1 lb. of steam $V_1 = 8{\cdot}7825$ cubic feet.

Therefore $P_1 V_1 = 63234$ lbs. raised one foot.

$$\frac{P_1}{P_2} = \frac{7200}{648} \text{ and consequently,}$$

Expansion to produce the maximum useful effect $s = \left(\frac{P_1}{P_2}\right)^{\frac{6}{7}} = 7{\cdot}877$.

Space traversed by the piston during the action of one pound of steam,

$$= V_1 (1-c)\, s = 65{\cdot}886 \text{ cubic feet.}$$

Gross effect of one pound of steam, in pounds raised one foot high,

$$= P_1 V_1 \left(7 - 6s^{-\frac{1}{6}} - \frac{s}{21}\right) - P_3 V_1 (1-c)\, s \quad . \quad . \quad = 112004$$

Deduct for resistance of engine when unloaded $f V_1 (1-c)\, s$. = 4744

Effect of one pound of steam in overcoming resistance depending on useful load, ... 107260

This being multiplied by $62\frac{1}{2}$, gives for the effect of one cubic foot of water evaporated, in pounds raised one foot, ... 6,703,750

It is here necessary to observe, that M. DE PAMBOUR distinguishes the useful resistance into two parts, the resistance of the useful load independently of the engine, and the increase in the resistance of the engine, arising from the former resistance, and found by multiplying it by a constant fraction which he calls δ. In calculating the net useful effect, he takes into account the former portion of the resistance only; consequently,

$$\text{Net useful effect as defined by M. DE PAMBOUR} = \frac{\text{Gross effect} - f V_1 (1-c)\, s}{1+\delta} \quad . \quad (54.)$$

The value of δ, for double acting steam-engines generally, is considered by M. DE PAMBOUR to be $\frac{1}{7}$; consequently, to reduce the effect of one cubic foot of water as calculated above to that which corresponds with his definition, we must deduct $\frac{1}{8}$, which leaves,

5,865,781 lb. raised one foot.

M. DE PAMBOUR's own calculation gives,

6,277,560

being too large by about one-fifteenth.

(32.) In order to shew the limit of the effect which may be expected from the expenditure of a given quantity of heat in evaporating water, and also to verify the approximate method employed in calculating the expansive action of the steam, I shall now investigate the *maximum gross effect*, including resistance of all kinds, producible by evaporating unity of weight of water at a higher temperature and liquefying it at a lower, and compare, in two examples, the power produced, with the heat which disappears during the action of the steam, as calculated directly.

To obtain the maximum gross effect, the steam must continue to act expansively until it reaches the pressure of condensation, so that $P_2 = P_3$. The clearance must also be null, or $c=0$. Making those substitutions in the formula (47.), we find, for the maximum gross effect of unity of weight of water, evaporated under the pressure P_1 and liquefied under the pressure P_2,

$$P_1 V_1 \frac{1}{1-\sigma}\left(1 - s^{1-\frac{1}{\sigma}}\right) = P_1 V_1 \frac{1-\left(\frac{P_2}{P_1}\right)^{1-\sigma}}{1-\sigma} \quad . \quad . \quad . \quad (55.)$$

In order to calculate directly the heat which is converted into power in this operation, let τ_1, τ_2, respectively represent the absolute temperatures of evaporation and liquefaction, and L_2 the latent heat of evaporation at the lower temperature τ_2; then the total heat of evaporation at τ_1, starting from τ_2 as the fixed point, by Equation (33.), is

$$H_{2,1} = L_2 + \cdot 305\, K_w\, (\tau_1 - \tau_2).$$

This is the heat communicated to the water in raising it from τ_2 to τ_1 and evaporating it. Now a weight $1-m$ of the steam is liquefied during the expansion at temperatures varying from τ_1 to τ_2, so that it may be looked upon as forming a mass of liquid water approximately at the mean temperature $\frac{\tau_1+\tau_2}{2}$, and from which a quantity of heat, approximately represented by

$$K_w\,(1-m)\,\frac{\tau_1-\tau_2}{2}$$

must be abstracted, to reduce it to the primitive temperature τ_2.

Finally, the weight of steam remaining, m, has to be liquefied at the temperature τ_2, by the abstraction of the heat

$$m\, L_2.$$

The difference between the heat given to the water, and the heat abstracted from it, or

$$\left.\begin{aligned} &H_{2,1} - K_w\,(1-m)\,\frac{\tau_1-\tau_2}{2} - m\, L_2 \\ &= (1-m)\, L_2 + K_w \left(\cdot 305 - \frac{1-m}{2}\right)(\tau_1 - \tau_2) \end{aligned}\right\} \quad . \quad . \quad . \quad (56.)$$

is the heat which has disappeared, and ought to agree with the expression (55.) for the power produced, if the calculation has been conducted correctly.

As a first example, I shall suppose unity of weight of water to be evaporated under the pressure of four atmospheres, and liquefied under that of half an atmosphere; so that the proper values of the coefficients and exponent are

$$\frac{1}{1-\sigma} = 7 \qquad 1-\sigma = \frac{1}{7}$$

The data in this case for calculating the power, are,

$P_1 = 8468$ lb. per square foot.

$V_1 = 7{\cdot}584$ cubic feet for one lb. of steam.

$P_1 V_1 = 64221$ lb. raised one foot.

$\frac{P_2}{P_1} = \frac{1}{8}$, whence $s = 8^{\frac{6}{7}} = 5{\cdot}944$.

Maximum possible effect of one pound of water,

$$= P_1 V_1 \times 7 \left(1 - \left(\frac{1}{8}\right)^{\frac{1}{7}}\right) = 115600 \text{ lb. raised one foot.}$$

Being the mechanical equivalent of $92°{\cdot}3$ centigrade degrees applied to one pound of liquid water at 0° C. ; or,

$$92°{\cdot}3\ K_w$$

Maximum possible effect of one cubic foot of water, 7,225,000 lb. raised one foot.

In order to calculate directly the heat converted into power, we have,

$\tau_1 = C + 141°{\cdot}1$ cent. $\tau_2 = C + 81°{\cdot}7$

$L_2 = 549°{\cdot}7\ K_w$

$H_{2,1} = 568°{\cdot}7\ K_w$ = heat expended in the boiler.

$1 - m = {\cdot}14$ nearly = proportion of steam liquefied during the expansion.

The heat converted into mechanical power, as calculated from these data, is found to be,

$$91°{\cdot}6\ K_w$$

differing by only $0°{\cdot}7$ from the amount as calculated from the power produced.

The direct method, however, is much less precise than the other, and is to be regarded as only a verification of the general principle of calculation.

The heat rendered effective, in the above example, is $\frac{92{\cdot}3}{568{\cdot}7}$, or less than *one-sixth* of that expended in the boiler.

As a second example, I shall suppose the steam to be produced at a pressure of eight atmospheres, and to expand to that of one atmosphere. In this case,

$P_1 = 16936$ lbs. per square foot.

$V_1 = 4{\cdot}03$ cubic feet per lb. of steam.

$P_1 V_1 = 68252$ lbs. raised one foot.

$\frac{P_2}{P_1} = \frac{1}{8}$ $\therefore$ $s = 5{\cdot}657 = 8^{\frac{5}{6}}$

Maximum possible effect of one pound of water,

$$= P_1 V_1 \times 6 \left(1 - \left(\frac{1}{8}\right)^{\frac{1}{6}}\right) = 119{,}942 \text{ lb. raised one foot.}$$

Being the equivalent of $95°{\cdot}8\ K_w$ (Centigrade).

Maximum possible effect of one cubic foot of water = 7,496,375 lb. raised one foot.

The data for calculating directly the heat rendered effective are,

$\tau_1 = C + 170°{\cdot}9$ cent. $\tau_2 = C + 100°$

$L_2 = 537°\ K_w$

$H_{2,1} = 558°{\cdot}6\ K_w$ = heat expended in the boiler.

$1 - m = {\cdot}148$ nearly = steam liquefied during the expansion.

Whence, the heat converted in power, as calculated directly, is,

$$95°{\cdot}8\,K_w$$

agreeing with the calculation from the power produced.

In this example, the heat rendered effective is $\frac{95{\cdot}8}{558{\cdot}6}$, or somewhat more than one-sixth of that expended in the boiler.

(33.) The results of the calculations of maximum possible effect, of which examples have just been given, are *limits* which may be approached in practice by Cornish and similar engines, but which cannot be fully realised; and yet it has been shewn, that in those theoretical cases only about *one-sixth* of the heat expended in the boiler is rendered effective. In practice, of course, the proportion of heat rendered effective must be still smaller; and, in fact, in some unexpansive engines, it amounts to only one-twenty-fourth, or even less.

Dr Lyon Playfair, in a memoir on the Evaporating Power of Fuel, has taken notice of the great disproportion between the heat expended in the steam-engine and the work performed. It has now been shewn that this waste of heat is, to a great extent, a necessary consequence of the nature of the machine. It can only be reduced by increasing the initial pressure of the steam, and the extent of the expansive action; and to both of those resources there are practical limits, which have already in some instances been nearly attained.

APPENDIX TO THE FOURTH SECTION,

CONTAINING TABLES TO BE USED IN CALCULATING THE PRESSURE, VOLUME, AND MECHANICAL ACTION OF STEAM, TREATED AS A PERFECT GAS.

The object of the first of the annexed tables is to facilitate the calculation of the volume of steam of saturation at a given pressure, of the pressure of steam of saturation at a given volume, and of its mechanical action at full pressure.

The pressures are expressed in pounds avoirdupois per square foot, and the volumes by the number of cubic feet occupied by one pound avoirdupois of steam, when considered as a perfect gas; those denominations being the most convenient for mechanical calculations in this country.

The columns to be used in determining the pressure from the volume, and *vice versâ*, are the third, fourth, sixth, and seventh.

The third column contains the common logarithms of the pressures of steam of saturation for every fifth degree of the centigrade thermometer from $-30°$ to $+260°$: that is to say, for every ninth degree of Fahrenheit's thermometer from $-22°$ to $+500°$.

The fourth column gives the differences of the successive terms of the third column.

The sixth column contains the common logarithms of the volume of one pound of steam of saturation corresponding to the same temperatures.

The seventh column contains the differences of the successive terms of the sixth column, which are negative; for the volumes diminish as the pressures increase.

By the ordinary method of taking proportional parts of the differences, the logarithms of the volumes corresponding to intermediate pressures, or the logarithms of the pressures corresponding to intermediate volumes, can be calculated with great precision. Thus, let $X+h$ be the logarithm of a pressure not found in the table, X being the next less logarithm which is found in the table; let Y be the logarithm of the volume corresponding to X, and $Y-k$ the logarithm of the volume corresponding to $X+h$; let H be the difference between X and the next greater logarithm in the table, as given in the fourth column, and K the corresponding difference in the seventh column; then by the proportion

$$H : K :: h : k$$

either $Y-k$ may be found from $X+h$, or $X+h$ from $Y-k$.

In the fifth and eighth columns respectively, are given the actual pressures and volumes corresponding to the logarithms in the third and sixth columns, to five places of figures.

In the ninth column are given the values of the quantity denoted by $P_1 V_1$ in the formulæ, which represents the mechanical action of unity of weight of steam at full pressure, or before it has begun to expand, in raising an equal weight. Those values are expressed in feet, being the products of the pressures in the fifth column by the volumes in the eighth, and have been found by multiplying the absolute temperature in centigrade degrees by 153·48 feet. Intermediate terms in this column, for a given pressure or a given volume, may be approximated to by the method of differences, the constant difference for 5° centigrade being 767·4 feet; but it is more accurate to calculate them by taking the product of the pressure and volume.

When the pressure is given in other denominations, the following logarithms are to be added to its logarithm, in order to reduce it to pounds avoirdupois per square foot:—

For Millimètres of mercury,	0·44477
Inches of mercury,	1·84960
Atmospheres of 760 millimètres,	3·32559
Atmospheres of 30 inches,	3·32672
Kilogrammes on the square centimètre,	3·31136
Kilogrammes on the circular centimètre,	3·41627
Kilogrammes on the square mètre,	$\bar{1}$·31136
Pounds avoirdupois on the square inch,	2·15836
Pounds avoirdupois on the circular inch,	2·26327

To reduce the logarithm of the number of cubic mètres occupied by one kilogramme to that of the number of cubic feet occupied by one pound avoirdupois, add 1·20463.

The logarithms are given to five places of decimals only, as a greater degree of precision is not attainable in calculations of this kind.

The second table is for the purpose of calculating the mechanical action of steam in expansive engines.

The first column contains values of the fraction of the entire capacity of the cylinder which is filled with steam before the expansion commences (being the quantity $\frac{1}{s}$ of the formulæ), for every hundredth part, from 1·00, or the whole cylinder, down to 0·10, or one-tenth.

If l be the entire length of stroke, l' the portion performed at full pressure, and c the fraction of the entire capacity of the cylinder allowed for clearance, then

$$\frac{l'}{l}=\frac{\frac{1}{s}-c}{1-c}, \quad \text{and} \quad \frac{1}{s}=(1-c)\frac{l'}{l}+c.$$

The *entire capacity of the cylinder* is to be understood to include clearance at one end only.

The second column gives the reciprocals of the quantities in the first, or the values of the ratio of expansion s.

The third and fourth columns, headed Z, give the values of the quantity $\frac{1}{1-\sigma}-\frac{\sigma}{1-\sigma}\,s^{1-\frac{1}{\sigma}}$ of Article 23, which represents the ratio of the entire gross action of the steam to its action at full pressure, without allowing for clearance. The third column is to be used for initial pressures of from one to four atmospheres; and the fourth for initial pressures of from four to eight atmospheres.

The deduction to be made from the quantity Z for clearance is $c\,s$, or the product of the fraction of the cylinder allowed for clearance by the ratio of expansion. Hence, to calculate from the tables the net mechanical action of unity of weight of steam, allowing for the counter-pressure of the waste steam P_3, as well as for clearance, we have the formula

$$P_1 V_1 (Z-c\,s)-P_3 V_1 (1-c)\,s$$

being equivalent to the formula (47.) of this paper.

TABLE I.—*Pressure and Volume of Steam, and its Action at Full Pressure.*

(1.) Temperature Fahrenheit.	(2.) Temperature Centigrade.	(3.) Logarithm of pressure in lbs. per square foot.	(4.) Differences.	(5.) Pressure in lbs. per square foot.	(6.) Logarithm of volume of one lb. of Steam in cubic feet.	(7.) Differences.	(8.) Volume of one lb. of Steam in cubic feet.	(9.) Action of a given weight of Steam in raising an equal weight in feet, at full pressure $= P_1 V_1$
−22°	−30°	$\bar{1}$·99278	0·20563	0·9835	4·58173	0·19684	38171	37541
−13	−25	0·19841	0·19602	1·5791	4·38489	0·18741	24260	38309
− 4	−20	0·39443	0·18710	2·4799	4·19748	0·17865	15757	39076
+ 5	−15	0·58153	0·17864	3·8153	4·01883	0·17036	10443	39843
14	−10	0·76017	0·17085	5·7567	3·84847	0·16272	7054·6	40611
23	− 5	0·93102	0·16348	8·5314	3·68575	0·15550	4850·1	41378
32	0	1·09450	0·15661	12·431	3·53025	0·14877	3390·4	42146
41	+ 5	1·25111	0·15012	17·828	3·38148	0·14242	2407·0	42913
50	10	1·40123	0·14404	25·190	3·23906	0·13648	1734·0	43680
59	15	1·54527	0·13836	35·097	3·10258	0·13093	1266·4	44448
68	20	1·68363	0·13284	48·265	2·97165	0·12553	936·81	45215
77	25	1·81647	0·12780	65·535	2·84612	0·12061	701·65	45983
86	30	1·94427	0·12297	87·957	2·72551	0·11590	531·51	46750
95	35	2·06724	0·11842	116·75	2·60961	0·11146	407·01	47517
104	40	2·18566	0·11410	153·34	2·49815	0·10725	314·88	48285
113	45	2·29976	0·11002	199·42	2·39090	0·10328	245·98	49052
122	50	2·40978	0·10614	256·91	2·28762	0·09950	193·92	49820
131	55	2·51592	0·10247	328·04	2·18812	0·09593	154·21	50587
140	60	2·61839	0·09897	415·33	2·09219	0·09253	123·65	51354
149	65	2·71736	0·09566	521·63	1·99966	0·08931	99·922	52122
158	70	2·81302	0·09250	650·16	1·91035	0·08625	81·349	52889
167	75	2·90552	0·08953	804·49	1·82410	0·08336	66·696	53657
176	80	2·99505	0·08658	988·67	1·74074	0·08050	55·048	54424
185	85	3·08163	0·08388	1206·8	1·66024	0·07788	45·734	55191
194	90	3·16551	0·08129	1463·9	1·58236	0·07538	38·226	55959
203	95	3·24680	0·07879	1765·2	1·50698	0·07295	32·135	56726
212	100	3·32559	0·07640	2116·4	1·43403	0·07064	27·166	57494
221	105	3·40199	0·07415	2523·4	1·36339	0·06847	23·088	58261
230	110	3·47614	0·07196	2993·2	1·29492	0·06635	19·721	59028
239	115	3·54810	0·06988	3532·6	1·22857	0·06434	16·927	59796
248	120	3·61798	0·06788	4149·3	1·16423	0·06241	14·596	60563
257	125	3·68586	0·06597	4851·3	1·10182	0·06057	12·642	61331
266	130	3·75183	0·06414	5647·2	1·04125	0·05881	10·996	62098
275	135	3·81597	0·06238	6545·9	0·98244	0·05711	9·6037	62865
284	140	3·87835	0·06069	7557·0	0·92533	0·05548	8·4204	63633
293	145	3·93904	0·05907	8690·4	0·86985	0·05393	7·4105	64400
302	150	3·99811	0·05751	9956·6	0·81592	0·05242	6·5452	65168
311	155	4·05562	0·05601	11366	0·76350	0·05099	5·8010	65935
320	160	4·11163	0·05456	12931	0·71251	0·04959	5·1583	66702
329	165	4·16619	0·05319	14662	0·66292	0·04828	4·6017	67470
338	170	4·21938	0·05184	16572	0·61464	0·04698	4·1176	68237
347	175	4·27122	0·05056	18673	0·56766	0·04576	3·6954	69005
356	180	4·32178	0·04932	20979	0·52190	0·04457	3·3258	69772
365	185	4·37110	0·04812	23502	0·47733	0·04342	3·0014	70539
374	190	4·41922	0·04696	26256	0·43391	0·04231	2·7159	71307
383	195	4·46618	0·04586	29254	0·39160	0·04126	2·4638	72074
392	200	4·51204	0·04478	32512	0·35034	0·04023	2·2405	72842
401	205	4·55682	0·04375	36043	0·31011	0·03924	2·0423	73609
410	210	4·60057	0·04274	39863	0·27087	0·03829	1·8663	74376
419	215	4·64331	0·04176	43986	0·23258	0·03734	1·7084	75144
428	220	4·68507	0·04085	48425	0·19524	0·03649	1·5676	75911
437	225	4·72592	0·03994	53201	0·15875	0·03561	1·4413	76679
446	230	4·76586	0·03906	58326	0·12314	0·03478	1·3278	77446
455	235	4·80492	0·03819	63815	0·08836	0·03395	1·2256	78213
464	240	4·84311	0·03740	69680	0·05441	0·03320	1·1335	78981
473	245	4·88051	0·03660	75947	0·02121	0·03244	1·0501	79748
482	250	4·91711	0·03582	82625	$\bar{1}$·98877	0·03170	0·97447	80516
491	255	4·95293	0·03507	89728	$\bar{1}$·95707	0·03099	0·90588	81283
500	260	4·98800		97275	$\bar{1}$·92608		0·84349	82050

TABLE II.—*Expansive Action of Steam.*

(1.) Fraction of Cylinder filled with Steam at full pressure $=\frac{1}{s}$	(2.) Ratio of Expansion $=s$.	(3.) Coefficient of gross action $=z$. Initial Pressure one to four Atmospheres.	(4.) Coefficient of gross action $=z$. Initial Pressure four to eight Atmospheres.	(1.) Fraction of Cylinder filled with Steam at full pressure. $=\frac{1}{s}$	(2.) Ratio of Expansion. $=s$.	(3.) Coefficient of gross action $=z$. Initial Pressure one to four Atmospheres.	(4.) Coefficient of gross action $=z$. Initial Pressure four to eight Atmospheres.
1·00	1·000	1·000	1·000	·54	1·852	1·586	1·580
·99	1·010	1·010	1·010	·53	1·887	1·602	1·596
·98	1·020	1·020	1·020	·52	1·923	1·620	1·613
·97	1·031	1·030	1·030	·51	1·961	1·637	1·630
·96	1·042	1·041	1·041	·50	2·000	1·655	1·647
·95	1·053	1·051	1·051	·49	2·041	1·673	1·665
·94	1·064	1·062	1·062	·48	2·083	1·691	1·683
·93	1·075	1·072	1·072	·47	2·128	1·709	1·701
·92	1·087	1·083	1·083	·46	2·174	1·728	1·719
·91	1·099	1·094	1·093	·45	2·222	1·748	1·738
·90	1·111	1·104	1·104	·44	2·273	1·767	1·757
·89	1·124	1·115	1·115	·43	2·326	1·787	1·777
·88	1·136	1·126	1·126	·42	2·381	1·808	1·796
·87	1·149	1·138	1·137	·41	2·439	1·829	1·817
·86	1·163	1·149	1·149	·40	2·500	1·850	1·837
·85	1·176	1·160	1·160	·39	2·564	1·871	1·858
·84	1·190	1·172	1·171	·38	2·632	1·894	1·880
·83	1·205	1·183	1·183	·37	2·703	1·916	1·902
·82	1·220	1·195	1·195	·36	2·778	1·939	1·924
·81	1·235	1·207	1·206	·35	2·857	1·963	1·947
·80	1·250	1·219	1·218	·34	2·941	1·987	1·970
·79	1·266	1·231	1·230	·33	3·030	2·012	1·994
·78	1·282	1·243	1·242	·32	3·125	2·038	2·019
·77	1·299	1·256	1·255	·31	3·225	2·064	2·044
·76	1·316	1·268	1·267	·30	3·333	2·091	2·070
·75	1·333	1·281	1·280	·29	3·448	2·119	2·097
·74	1·351	1·294	1·292	·28	3·571	2·147	2·124
·73	1·370	1·307	1·305	·27	3·704	2·176	2·152
·72	1·389	1·320	1·318	·26	3·846	2·207	2·181
·71	1·408	1·333	1·331	·25	4·000	2·238	2·211
·70	1·429	1·346	1·344	·24	4·167	2·270	2·242
·69	1·449	1·360	1·358	·23	4·348	2·304	2·273
·68	1·471	1·374	1·371	·22	4·545	2·338	2·306
·67	1·493	1·387	1·385	·21	4·762	2·374	2·341
·66	1·515	1·401	1·399	·20	5·000	2·412	2·376
·65	1·538	1·416	1·413	·19	5·263	2·451	2·413
·64	1·563	1·430	1·427	·18	5·556	2·492	2·452
·63	1·587	1·445	1·441	·17	5·882	2·534	2·492
·62	1·613	1·459	1·456	·16	6·250	2·579	2·434
·61	1·640	1·474	1·471	·15	6·667	2·626	2·579
·60	1·667	1·490	1·486	·14	7·143	2·676	2·626
·59	1·695	1·505	1·501	·13	7·692	2·730	2·675
·58	1·724	1·521	1·516	·12	8·333	2·786	2·728
·57	1·754	1·537	1·532	·11	9·091	2·847	2·784
·56	1·786	1·553	1·547	·10	10·000	2·912	2·845
·55	1·818	1·569	1·563				

VIII.—*Note as to the Dynamical Equivalent of Temperature in Liquid Water, and the Specific Heat of Atmospheric Air and Steam*, being a Supplement to a Paper *On the Mechanical Action of Heat.* By WILLIAM JOHN MACQUORN RANKINE, Civil Engineer, F.R.S.E., F.R.S.S.A., &c.

(Read 2d December 1850.)

(33*.) In my paper on the Mechanical Action of Heat, published in the 1st Part of the 20th Volume of the *Transactions of the Royal Society of Edinburgh*, some of the numerical results depend upon the dynamical equivalent of a degree of temperature in liquid water. The value of that quantity which I then used, was calculated from the experiments of DE LA ROCHE and BÉRARD on the apparent specific heat of atmospheric air under constant pressure, as compared with liquid water.

The experiments of Mr JOULE on the production of heat by friction, give, for the specific heat of liquid water, an equivalent about one-ninth part greater than that which is determined from those of DE LA ROCHE and BÉRARD. I was formerly disposed to ascribe this discrepancy in a great measure to the smallness of the differences of temperature measured by Mr JOULE, and to unknown causes of loss of power in his apparatus, such as the production of sound and of electricity; but, subsequently to the publication of my paper, I have seen the detailed account of Mr JOULE's last experiments in the *Philosophical Transactions* for 1850, which has convinced me, that the uncertainty arising from the smallness of the elevations of temperature, is removed by the multitude of experiments (being forty on water, fifty on mercury, and twenty on cast iron); that the agreement amongst the results from substances so different, shews that the error by unknown losses of power is insensible, or nearly so; and that the necessary conclusion is, that the dynamical value assigned by Mr JOULE to the specific heat of liquid water, viz.:—772 feet per degree of Fahrenheit, does not err by more than two, or, at the utmost, three feet; and therefore, that the discrepancy originates chiefly in the experiments of DE LA ROCHE and BÉRARD.

I therefore take the earliest opportunity of correcting such of my calculations as require it, so as to correspond with Mr JOULE's equivalent. They relate to the specific heat of atmospheric air as compared with liquid water, and to that of steam, and are contained in the second and third Sections of my paper, Articles 14 and 20; Equations 28, 34, and 36.

SPECIFIC HEAT OF ATMOSPHERIC AIR AS COMPARED WITH LIQUID WATER.—(Section II Article 14.)

The dynamical values of the specific heat of atmospheric air are calculated independently from the velocity of sound, without reference to the specific heat of liquid water; and from the closeness of the agreement of the experiments of M.M. BRAVAIS and MARTINS, MOLL and VAN BEEK, STAMPFER and MYRBACH, WERTHEIM and others, it is clear that the limits of error are about $\frac{1}{300}$ for the velocity of sound, $\frac{1}{150}$ for the ratio, and from $\frac{1}{40}$ to $\frac{1}{50}$ for the dynamical values of the specific heat of air, at constant volume and constant pressure. Those values, as given by Equation 27, are—

Real specific heat,—

$$\mathfrak{k} = 238{\cdot}66 \text{ feet} = 72{\cdot}74 \text{ metres per centigrade degree.}$$
$$= 132{\cdot}6 \text{ feet per degree of Fahrenheit.}$$

Apparent specific heat under constant pressure,—

$$K_P = 334 \text{ feet} = 101{\cdot}8 \text{ metres per centigrade degree.}$$
$$= 185{\cdot}6 \text{ feet per degree of Fahrenheit.}$$

The ratio of these two quantities being taken as

$$\frac{K_P}{\mathfrak{k}} = 1 + N = 1{\cdot}4$$

The dynamical equivalent of the specific heat of liquid water, as determined by Mr JOULE, is

$$K_w = 1389{\cdot}6 \text{ feet} = 423{\cdot}54 \text{ metres per centigrade degree.}$$
$$= 772 \text{ feet per degree of Fahrenheit.}$$

The specific heat of air, that of liquid water being taken as unity, has therefore the following values:—

Real specific heat,—

$$\frac{\mathfrak{k}}{K_w} = \frac{132{\cdot}6}{772} = 0{\cdot}1717.$$

Apparent specific heat under constant pressure,—

$$\frac{K_P}{K_w} = \frac{185{\cdot}6}{772} = 0{\cdot}2404$$

This last quantity, according to DE LA ROCHE and BÉRARD, is

$$0{\cdot}2669$$

The discrepancy being, . . $0{\cdot}0265$

or *one-ninth* of the value according to Mr JOULE's equivalent.

SPECIFIC HEAT OF STEAM. (Section III. Art. 20.)

The apparent specific heat of steam (Eq. 34 and 36) as a gas under constant

pressure is equal to that of liquid water × 0·305. Its dynamical value is therefore

$$K_P = \mathfrak{k} + \frac{1}{C\,n\,M} = 1389{\cdot}6 \times 0{\cdot}305$$

$$= 422{\cdot}83 \text{ feet} = 129{\cdot}18 \text{ metres per centigr. degree.}$$

But $$\frac{1}{C\,n\,M} = 153{\cdot}48 \text{ feet} = 46{\cdot}78 \text{ metres per centigr. degree.}$$

Therefore the real specific heat is

$$\mathfrak{k} = 269{\cdot}35 \text{ feet} = 82{\cdot}40 \text{ metres per centigr. degree.}$$

Or, that of liquid water being taken as unity,

$$\frac{\mathfrak{k}}{K_w} = \frac{269{\cdot}35}{1389{\cdot}6} = 0{\cdot}194$$

The ratio of these two values of the specific heat of steam is

$$1 + N = 1{\cdot}57.$$

Their dynamical equivalents for FAHRENHEIT'S scale are

$$\mathfrak{k} = 149{\cdot}64 \text{ feet} \quad . \quad . \quad . \quad K_P = 235{\cdot}46 \text{ feet.}$$

Neither the formulæ in the fourth Section, respecting the working of the steam-engine, nor the tables at the end of the paper, require any alteration; for the action of steam at full pressure being calculated from data independent of its specific heat, is not at all affected by the discrepancy I have mentioned; and the expansive action is not affected to an extent appreciable in practice.

IX.—*On the Power and Economy of Single-Acting Expansive Steam-Engines*, being a Supplement to the Fourth Section of a Paper *On the Mechanical Action of Heat*. By William John Macquorn Rankine, Civil Engineer, F.R.S.E., F.R.S.S.A., &c.

(Read 21st April 1851.)

(34.) The objects of this paper are twofold: *First*, To compare the results of the formulæ and tables relative to the power of the steam-engine, which have been deduced from the Dynamical Theory of Heat, with those of experiments on the actual duty of a large Cornish engine at various rates of expansion; and, *Secondly*, To investigate and explain the method of determining the rate of expansion, and, consequently, the dimensions and proportions of a Cornish engine, which, with a given maximum pressure of steam in the cylinder, at a given velocity, shall perform a given amount of work at the least possible pecuniary cost, taking into account the expense of fuel, and the interest of the capital required for the construction of the engine.

This problem is solved with the aid of the tables already printed, by drawing two straight lines on a diagram annexed to this paper.

The merit of first proposing the question of the economy of expansive engines in this definite shape, belongs, I believe, to the Artizan Club, who have offered premiums for its solution; having done so (to use their own words), "with a view to enable those who, from their position, cannot take part in the discussions of the various scientific Societies to give the profession the benefit of their studies and experience." The 5th of April is the latest day fixed by them for receiving papers; and as this communication cannot possibly be read to a meeting before the 7th April, nor published until some months afterwards, I trust I may feel confident that it will not be considered as interfering with their design.

Formulæ applicable to the Cornish Engine.

(35.) The equations of motion of the steam-engine, in this and the original paper, are the same in their general form with those of M. de Pambour. The differences consist in the expressions for the pressure and volume of steam, and for the mechanical effect of its expansion; the former of which were deduced from a formula suggested by peculiar hypothetical views, and the latter from the dynamical theory of heat.

Those equations are Nos. (50) and (51) of the original paper. I shall now

express them in a form more convenient for practical use, the notation being as follows:—

Let A be the area of the piston.

l, the length of stroke.

n, the number of double strokes in unity of time.

c, the fraction of the total bulk of steam above the piston when down, allowed for clearance, and for filling steam-passages; so that the total bulk of steam at the end of the effective stroke is

$$\frac{l\,\mathrm{A}}{1-c} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (a)$$

l', the length of the portion of the stroke performed when the steam is cut off.

s, the ratio of expansion of the steam, so that

$$\left.\begin{aligned} \frac{1}{s} &= (1-c)\frac{l'}{l}+c \\ \frac{l'}{l} &= \frac{\frac{1}{s}-c}{1-c} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (b)$$

Let W be the weight of steam expended in unity of time.

P_1, the pressure at which it enters the cylinder.

V_1, the volume of unity of weight of steam at saturation at the pressure P_1; which may be found from Table I. of the Appendix to the original paper.

F, the sum of all the resistances not depending on the useful load, reduced to a pressure per unit of area of piston; whether arising from imperfect vacuum in the condenser, resistance of the air-pump, feed-pump, and cold-water pump, friction, or any other cause.

R, the resistance arising from the *useful load*, reduced to a pressure per unit of area of piston.

Z, the ratio of the total action of steam working at the expansion s, to its action without expansion. Values of this ratio are given in the second Table of the Appendix to the original paper.

Then the following are the two fundamental equations of the motion of the steam-engine, as comprehended in equation (50) of the original paper.

First, Equality of power and effect,—

$$\mathrm{R\,A}\,l\,n = \mathrm{W\,V_1}\,\{\mathrm{P_1}\,(\mathrm{Z}-c\,s)-\mathrm{F}\,(1-c)\,s\} \quad . \quad . \quad (c)$$

Secondly, Equality of two expressions for the weight of steam expended in unity of time,—

$$\mathrm{W} = \frac{\mathrm{A}\,l\,n}{\mathrm{V_1}\,(1-c)\,s} \quad . \quad . \quad . \quad . \quad (d)$$

From these two equations is deduced the following, expressing the ratio of the mean load on the piston to the initial pressure of the steam:—

$$\frac{R+F}{P_1} = \frac{Z-cs}{(1-c)s} \quad . \quad . \quad . \quad . \quad (e)$$

being equivalent to equation (51).

In computing the effect of Cornish engines these formulæ require to be modified, owing to the following circumstances.

The terms depending on the clearance c have been introduced into equations (c), (d), on the supposition that the steam employed in filling the space above the piston at the top of its stroke is lost, being allowed to escape into the condenser, without having effected any work; so that a weight of steam $W c s$ is wasted, and an amount of power $WV_1 (P_1 - F) c s$ lost, in unity of time. But in Cornish engines this is not the case; for by closing the equilibrium-valve at the proper point of the up or out-door stroke, nearly the whole quantity of steam necessary to fill the clearance and valve-boxes may be kept imprisoned above the piston so as to make the loss of power depending on it insensible in practice. This portion of steam is called a cushion, from its preventing a shock at the end of the up-stroke; and as Mr Pole in his valuable work on the Cornish engine has observed, its alternate compression and expansion compensate each other, and have no effect on the duty of the engine. The proper moment of closing the equilibrium-valve is fixed by trial, which is, perhaps, the best way; but if it is to be fixed by theory, the following is the proper formula. Let l'' be the length of the portion of the up-stroke remaining to be performed after the equilibrium-valve has been closed: then—

$$\frac{l''}{l} = \frac{c(s-1)}{1-c} \quad . \quad . \quad . \quad . \quad . \quad (f)$$

A slight deviation from this adjustment will produce little effect in practice, if the fraction c is small.

In forming the equations of motion, therefore, of the Cornish engine, we may, without material error in practice, omit the terms denoting a waste of steam and loss of power due to clearance and filling of steam-passages; and the results are the following:—

Equation of effect and power in unity of time:—

$$\text{Useful effect } E = R A l n = W V_1 \{P_1 Z - F\} \quad . \quad (57.)$$

Weight of steam expended in unity of time:—

$$W = \frac{A l n}{V_1 s} \quad . \quad . \quad . \quad . \quad . \quad (58.)$$

From those two fundamental equations the following are deduced:—

Ratio of mean load on piston to maximum pressure,—

$$\frac{R+F}{P_1} = \frac{Z}{s} \quad . \quad . \quad . \quad . \quad . \quad (59.)$$

Duty of unity of weight of steam,—

$$\frac{E}{W} = V_1 (P_1 Z - F) \quad . \quad . \quad . \quad . \quad (60.)$$

which, being multiplied by the number of units of weight of steam produced by a given weight of fuel, gives the duty of that weight of fuel.

Weight of steam expended per stroke,—

$$\frac{W}{n} = \frac{A\,l}{V_1\,s} \quad . \quad . \quad . \quad . \quad (61.)$$

In fact, it is clear that if any five quantities out of the following seven be given, the other two may be determined by means of the equations:

$R+F$, the mean load on unit of area of piston.

P_1, the maximum pressure of steam in the cylinder.

s, the ratio of expansion.

W, the weight of steam produced in unity of time.

A, the area of the piston.

l, the length of stroke.

n, the number of strokes in unity of time.

The other quantities, E, V_1, Z, are functions of those seven.

Comparison of the Theory with Mr Wicksteed's *Experiments.*

(36.) In order to test the practical value of this theory, I shall compare its results with those of the experiments which were made by Mr Wicksteed on the large Cornish pumping engine built under the direction of that eminent engineer by Messrs Harvey and West, for the East London Water-Works at Old Ford, and which were published in 1841. The dimensions and structure of the engine, and the details of the experiments, are stated with such minuteness and precision, that there is none of that uncertainty respecting the circumstances of particular cases, which is the most frequent cause of failure in the attempt to apply theoretical principles to practice.

The engine was worked under a uniform load at five different rates of expansion successively. The number of strokes, and the consumption of steam during each trial, having been accurately registered, Mr Wicksteed gives a table shewing the weight of steam consumed per stroke for each of the five rates of expansion. I shall now compute the weight of steam per stroke theoretically, and compare the results.

Throughout these calculations I shall uniformly use the foot as the unit of length, the avoirdupois pound as that of weight, and the hour as that of time. Pressures are consequently expressed in pounds per square foot for the purpose of calculation; although in the table of experiments I have reduced them to pounds per square inch, as being the more familiar denomination.

The data respecting the dimensions and load of the engine, which are constant throughout the experiments, are the following :—

Area of piston, . . .	$A=34{\cdot}854$ square feet.
Stroke, ...	$l=10$ feet.
Cubic space traversed by piston during one down stroke,	$=A\,l=348{\cdot}54$ cubic feet.
Clearance and valve-boxes, ...	18·00
Sum, .	366·54

Therefore, $c=0{\cdot}05$

R = useful load of piston, . . .	$=1597{\cdot}$ lb. per square foot.
F = additional resistance, . . .	$=266{\cdot}6$ ... ...
R + F = total mean pressure on piston,	$=1863{\cdot}6$ ... ...

The mode of calculation is the following :—

Mr Wicksteed states the fraction $\frac{l'}{l}$ of the stroke performed at full pressure in each experiment. From this the ratio of expansion s is computed by equation (b), giving in this case

$$\frac{1}{s}=0{\cdot}95\frac{l'}{l}+0{\cdot}05$$

The value of Z corresponding to s is then found by means of the third column of Table second ; that column being selected because the initial pressures were all below four atmospheres. This affords the means of determining the initial pressure of the steam by equation (59), viz. :—

$$P_1=\frac{s}{Z}(R+F)=1863{\cdot}6\frac{s}{Z}$$

By using Table first according to the directions prefixed to it, the volume of one pound of steam at the pressure P_1, in cubic feet, is calculated, and thence, by equation (60), the weight of steam per stroke, according to theory, which is compared with the weight as ascertained by experiment.

Further to illustrate the subject, the useful effect, or duty of a pound of steam, is computed according to the theory and the experiments respectively, and the results compared.

The following Table exhibits the results.

Comparison of the Theory with Mr WICKSTEED'S *Experiments on the Cornish Pumping Engine at Old Ford.*

Number of Experiments.	Pressure in the Boiler, lb. per square inch.	Steam cut off at $\frac{v'}{l}$ of the stroke.	Ratio of Expansion s.	Maximum Pressure in the Cylinder, lb. per square inch.	Lb. of Steam expended per stroke,		Difference.	Duty of one lb. of Steam,		Difference.
					By Theory.	By Experiment.		By Theory.	By Experiment.	
								foot-lbs.	foot-lbs.	
B.	30·45	0·603	1·605	14·27	7·781	7·536	−0·245	71530	73860	+2330
C.	33·20	0·477	1·938	15·59	6·963	6·463	−0·500	79936	86123	+6187
D.	39·2	0·397	2·342	16·9	6·236	6·200	−0·036	89275	89776	+ 501
E.	41·2	0·352	2·605	17·89	5·905	5·985	+0·085	94258	93002	−1256
F.	45·7	0·313	2·882	18·93	5·626	5·470	−0·156	98940	101756	+2816
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)

This comparison sufficiently proves that the results of the theory are practically correct.

It is remarkable, that in every instance except one (experiment E) the experimental results shew a somewhat less expenditure of steam per stroke, and a greater duty per pound of steam, than theory indicates. This is to be ascribed to the fact, that although the action of the steam is computed theoretically, on the assumption that during the expansion it is cut off from external sources of heat, yet it is not exactly so in practice; for the cylinder is surrounded with a jacket or casing communicating with the boiler, in which the temperature is much higher than the highest temperature in the cylinder, the pressure in the boiler being more than double the maximum pressure of the steam when working, as columns (2) and (5) shew. There is, therefore, a portion of steam, of whose amount no computation can be made, which circulates between the boiler and the jacket, serving to convey heat to the cylinder, and thus augment by a small quantity the action of the steam expended; and hence the formulæ almost always err on the safe side.

Supposing one pound of the best Welsh coals to be capable (as found by Mr WICKSTEED) of evaporating 9·493 lb. of water at the pressure in the boiler during the experiment F, then the duty of a Cornish bushel, or 94 lb. of such coals, in the circumstances of that experiment would be—

By theory, . . .	88,288,000 ft. lb.
By experiment, . .	90,801,000 ...
Difference, .	+ 2,513,000 ...

Economy of Single-Acting Expansive Engines.

(37.) By increasing the ratio of expansion in a Cornish engine, the quantity of steam required to perform a given duty is diminished; and the cost of fuel, and of the boilers, is lowered. But at the same time, as the cylinders and every part of the engine must be made larger, to admit of a greater expansion, the cost of the engine is increased. It thus becomes a problem of maxima and minima to determine what ratio of expansion ought to be adopted under given circumstances, in order that the sum of the annual cost of fuel, and the interest of the capital employed in construction, may be the least possible, as compared with the work done.

That this problem may admit of a definite solution, the following five quantities must be given:—

P_1, the initial pressure in the cylinder.

F, the resistance not depending on the useful load.

$l\,n$, the amount of the length of the effective strokes made in unity of time.

h, the annual cost of producing unity of weight of steam in unity of time, which consists of two parts; the price of fuel, and the interest of the cost of the boilers.

k, the interest of the cost of the engine, per unit of area of piston.

Hence the annual expenditure to be taken into consideration, reduced to unity of weight of steam, is

$$h+k\,\frac{A}{W}=h+k\,\frac{V_1\,s}{l\,n}$$

And the useful effect of unity of weight of steam being

$$V_1\,(P_1\,Z-F\,s)$$

The problem is to determine the ratio of expansion s, so that

$$\frac{V_1\,(P_1\,Z-F\,s)}{h+k\,\frac{V_1\,s}{l\,n}}$$

shall be a maximum.

Dividing the numerator of this fraction by $V_1\,P_1$, and the denominator by $\frac{k\,V_1}{l\,n}$, both of which are constants in this problem, we find that it will be solved by making the ratio

$$\frac{Z-\frac{F}{P_1}\,s}{\frac{h\,l\,n}{k\,V_1}+s} \qquad . \quad . \quad . \quad . \quad (62.)$$

a maximum.

The algebraical solution would be extremely complicated and tedious. The

graphic solution, on the other hand, is very simple and rapid, and sufficiently accurate for all practical purposes; and I have therefore adopted it.

In the annexed diagram, Plate VIII., fig. 1, the axis of abscissæ, $-XO+X$, is graduated from O towards $+X$ into divisions representing ratios of expansion, or values of s. The divisions of the axis of ordinates, O Y, represent values of Z. The curve marked "locus of Z," is laid down from the third column of Table II. of the Appendix to the original paper, being applicable to initial pressures not exceeding four atmospheres.

Through the origin O draw a straight line BOA, at such an inclination to $-XO+X$ that its ordinates are represented by $\frac{F}{P_1}s$. Then the ordinates measured from this inclined line to the locus of Z represent the value of the numerator $Z-\frac{F}{P_1}s$, of the ratio (62), corresponding to the various values of s.

Take a point at C on the line BOA, whose abscissa, measured along $O-X$, represents $-\frac{h\,n\,l}{k\,V_1}$. Then the ordinates, measured from BOA, of any straight line drawn through C, vary proportionally to the denominator $\frac{h\,n\,l}{k\,V_1}+s$ of the ratio (62).

Through the point C, therefore, draw a straight line CT, touching the locus of Z: Then the ratio (62) is a maximum at the point of contact T, and the abscissa at that point represents the ratio of expansion required.

Example.

(38.) To exemplify this method, let us take the following data.

Greatest pressure in the cylinder $P_1=20$ lb. per square inch, $=2880$ lb. per square foot.

The corresponding value of V_1 is 20·248 cubic feet per pound of steam.

To obtain this initial pressure in the cylinder, it will be necessary to have a pressure of about 50 lbs. per square inch in the boiler.

F, resistance not depending on the useful load $=2$ lb. per square inch, $=288$ lb. per square foot, $=\frac{1}{10}P_1$.

ln, amount of down strokes, $=4800$ feet per hour; being the average speed found to answer best in practice.

To estimate h, the annual cost of producing one pound of steam per hour, I shall suppose that the engine works 6000 hours per annum; that the cost of fuel is one penny per 100 lb. of steam;* that the cost of boiler for each pound of steam per hour is 0·016 ton, at £27, $=$£0·432; and that the interest of capital is five per cent. per annum. Hence h is thus made up—

* This estimate is made on the supposition that coals capable of producing nine times their weight of steam are worth about 16s. 9d. per ton.

Fuel for 6000 lb. of steam at 0·01d., . . .	£0·2500
Interest on £0·432, at 5 per cent., ...	0·0216
h =	£0·2716

Estimating the cost of the engine at £250 per square foot of piston, we find k= 5 per cent. per annum on £250 = £12·5,

$$\text{and} \quad \frac{h}{k} = 0{\cdot}0217\,; \quad \frac{h\,l\,n}{k\,V_1} = 5{\cdot}144$$

The line BOA, then, is to be drawn so that its ordinates are $\frac{F}{P_1}s = \frac{1}{10}s$.

The point C is taken on this line, at $\frac{h\,l\,n}{k\,V_1} = 5{\cdot}144$ divisions of the axis of abscissæ to the left of O Y.

The tangent C T being drawn, is found to touch the locus of Z at 2·800 divisions to the right of O Y.

Then s=2·800 is the ratio of expansion sought, corresponding to the greatest economy.

If we make c=0·05 as in Mr Wicksteed's engine, then the fraction of the stroke to be performed at full pressure is

$$\frac{l'}{l} = 0{\cdot}323$$

being nearly the same as in experiment F.

The mean resistance of the useful load per square foot of piston is

$$R = \frac{Z}{s}P_1 - F = 1713{\cdot}6 \text{ lb.}$$

The duty of one square foot of piston per hour,—

$$R\,l\,n = 8{,}225{,}300 \text{ foot-lb.}$$

And one horse-power being 1,980,000 foot-lb. per hour, the real horse-power of the engine is

4·154 per square foot of piston.

The duty of one pound of steam is

$$R\,V_1\,s = 97{,}154 \text{ foot-lb.}$$

To give an example of a special case, let the duty to be performed be 198,000,000 foot-pounds per hour, being equal to 100 real horse-power, for 6000 hours per annum. This being called E, we find from the above data that the area of piston required is

$$A = \frac{E}{R\,l\,n} = 24{\cdot}072 \text{ square feet.}$$

The consumption of steam per hour is

$$W = \frac{E}{R\,V_1\,s} = 2038 \text{ lb.}$$

which requires 2038 × 0·016 = 32·608 tons of boilers.

The expenditure of steam per annum is

$$2038 \times 6000 = 12{,}228{,}000 \text{ lb.}$$

Hence we have the following estimate:—

Cost of engine, 24·072 square feet of piston at £250, .	£6018·000
Cost of boilers, 32·608 tons at £27, . . .	880·416
Total capital expended,	£6898·416
Interest at five per cent. per annum, . . .	£344·921
Cost of fuel per annum, 12,228,000 lb. of steam at 0·01d.,	509·500
Annual cost for interest and fuel,	£854·421

I wish it to be understood that the rates I have adopted in the foregoing calculations, for interest, cost of fuel, and cost of construction, are not intended as estimates of their average amount, nor of their amount in any particular case, but are merely assumed in order to illustrate, by a numerical example, the rules laid down in the preceding article. It is of course the business of the engineer to ascertain those data with reference to the special situation and circumstances of the proposed work; and having done so, the method explained in this paper will enable him to determine the dimensions and ratio of expansion which ought to be adopted for the engine, in order that it may effect its duty with the greatest possible economy.

X.—*On the Economy of Heat in Expansive Machines*, forming the Fifth Section of a Paper *On the Mechanical Action of Heat*. By William John Macquorn Rankine, Civil Engineer, F.R.S.E., F.R.S.S.A., &c.

(Read 21st April 1851.)

(39.) A machine working by expansive power consists essentially of a portion of some substance to which heat is communicated, so as to expand it, at a higher temperature, being abstracted from it, so as to condense it to its original volume, at a lower temperature. The quantity of heat given out by the substance is less than the quantity received; the difference disappearing as heat to appear in the form of expansive power.

The heat originally received by the working body may act in two ways: to raise its temperature, and to expand it. The heat given out may also act in two ways: to lower the temperature, and to contract the body. Now, as the conversion of heat into expansive power arises from changes of volume only, and not from changes of temperature, it is obvious, that the proportion of the heat received which is converted into expansive power will be the greatest possible, when the reception of heat, and its emission, each take place at a constant temperature.

(40.) Carnôt was the first to assert the law, *that the ratio of the maximum mechanical effect, to the whole heat expended in an expansive machine, is a function solely of the two temperatures at which the heat is respectively received and emitted, and is independent of the nature of the working substance.* But his investigations not being based on the principle of the dynamical convertibility of heat, involve the fallacy that power can be produced out of nothing.

(41.) The merit of combining Carnôt's *Law*, as it is termed, with that of the convertibility of heat and power, belongs to Mr Clausius and Professor William Thomson; and in the shape into which they have brought it, it may be stated thus:—

The maximum proportion of heat converted into expansive power by any machine, is a function solely of the temperatures at which heat is received and emitted by the working substance; which function, for each pair of temperatures, is the same for all substances in nature.

This law is laid down by Mr Clausius, as it originally had been by Carnôt, as an independent axiom; and I had at first doubts as to the soundness of the reasoning by which he maintained it. Having stated those doubts to Professor Thomson, I am indebted to him for having induced me to investigate the subject thoroughly; for although I have not yet seen his paper, nor become acquainted with

the method by which he proves CARNÔT's law, I have received from him a statement of some of his more important results.

(42.) I have now come to the conclusions,—First: *That* CARNÔT's *Law is not an independent principle in the theory of heat; but is deducible, as a consequence, from the equations of the mutual conversion of heat and expansive power, as given in the First Section of this paper.*

Secondly: *That the function of the temperatures of reception and emission, which expresses the maximum ratio of the heat converted into power to the total heat received by the working body, is the ratio of the difference of those temperatures, to the absolute temperature of reception diminished by the constant, which I have called* $\kappa = C n \mu b$, and which must, as I have shewn in the Introduction, be the same for all substances, in order that molecular equilibrium may be possible.

(43.) Let abscissæ, parallel to OX in the diagram, Plate VIII. fig. 2, denote the volumes successively assumed by the working body, and ordinates, parallel to OY, the corresponding pressures. Let τ_1 be the constant absolute temperature at which the reception of heat by the body takes place: τ_0, the constant absolute temperature at which the emission of heat takes place. Let AB be a curve such that its ordinates denote the pressures, at the temperature of reception τ_1, corresponding to the volumes denoted by abscissæ. Let DC be a similar curve for the temperature of emission τ_0. Let AD and BC be two curves, expressing by their co-ordinates how the pressure and volume must vary, in order that the body may change its temperature, without receiving or emitting heat; the former corresponding to the most condensed and the latter to the most expanded state of the body, during the working of the machine.

The quantity of heat received or emitted during an operation on the body involving indefinitely small variations of volume and temperature, is expressed by adding to Equation (6.) of Section Fourth the heat due to change of temperature only, in virtue of the real specific heat. We thus obtain the differential equation

$$\delta Q' - \delta Q = -\frac{\tau - \kappa}{C n M}\left\{\delta V\left(\frac{1}{V} - \frac{dU}{dV}\right) - \delta\tau \cdot \frac{dU}{d\tau}\right\} - k\,\delta\tau$$

In which the negative sign denotes absorption, and the positive emission.

If we now put for $\frac{dU}{dV}$, $\frac{dU}{d\tau}$, their values according to Equation (11.), we find

$$\delta Q' - \delta Q = -(\tau - \kappa)\frac{dP}{d\tau} \cdot \delta V - \left\{ k + \frac{1}{C n M}\left(\frac{\kappa}{\tau} - \frac{\kappa^2}{\tau^2}\right) + (\tau - \kappa)\frac{d}{d\tau}\int \frac{dP}{d\tau} dV \right\} \delta\tau \quad . \quad (63.)$$

The first term represents the variation of heat due to variation of volume only; the second, that due to variation of temperature. Let us now apply this

equation to the cycle of operations undergone by the working body in an expansive machine, as denoted by the diagram.

First operation. The body, being at first at the volume V_A and pressure P_A, is made to expand, by the communication of heat at the constant temperature τ_1, until it reaches the volume V_B and pressure P_B, AB being the locus of the pressures.

Here $\delta\tau=0$; therefore the total heat received is

$$\left.\begin{aligned} H_1 = -Q'_1 &= (\tau_1-\kappa)\int_{V_A}^{V_B} \frac{d\,P}{d\,\tau}\,dV \\ &= (\tau_1-\kappa)\{\phi(V_B,\tau_1)-\phi(V_A,\tau_1)\} \end{aligned}\right\} \quad . \quad . \quad . \quad (a)$$

Second operation. The body, being prevented from receiving or emitting heat, expands until it falls to the temperature τ_0, the locus of the pressures being the curve BC. During this operation the following condition must be fulfilled,—

$$0 = \delta Q' - \delta Q$$

Which, attending to the fact that V is now a function of τ, and transforming the integrals as before, gives the equation

$$0=\mathfrak{k}+\frac{1}{C\,n\,M}\left(\frac{\kappa}{\tau}-\frac{\kappa^2}{\tau^2}\right)+(\tau-\kappa)\left(\frac{d}{d\,\tau}+\frac{d\,V}{d\,\tau}\cdot\frac{d}{d\,V}\right)\phi(V,\tau)$$

This equation shews that

$$\phi(V_B,\tau_1)-\phi(V_C,\tau_0)=\psi(\tau_1,\tau_0) \quad . \quad . \quad . \quad (b)$$

Third operation. The body, by the abstraction of heat, is made to contract, at the constant temperature τ_0, to the volume V_D and pressure P_D, which are such as to satisfy conditions depending on the fourth operation. CD is the locus of the pressures. The heat emitted is evidently

$$H_0=Q'_0=(\tau_0-\kappa)\{\phi(V_C,\tau_0)-\phi(V_D,\tau_0)\} \quad . \quad . \quad (c)$$

Fourth operation. The body, being prevented from receiving or emitting heat, is compressed until it recovers its original temperature τ_1, volume V , and pressure P_A; the locus of the pressures being DA. During this operation, the same conditions must be fulfilled as in the second operation; therefore

$$\phi(V_A,\tau_1)-\phi(V_D,\tau_0)=\psi(\tau_1,\tau_0) \quad . \quad . \quad . \quad . \quad (d)$$

ψ being the same function as in Equation (*b*).

By comparing Equations (*b*) and (*d*), we obtain the relation which must subsist between the four volumes to which the body is successively brought, in order that the maximum effect may be obtained from the heat. It is expressed by the equation

$$\phi(V_B,\tau_1)-\phi(V_A,\tau_1)=\phi(V_C,\tau_0)-\phi(V_D,\tau_0) \quad . \quad . \quad (64.)$$

From this, and Equations (*a*) and (*c*), it appears that

$$\frac{H_0}{H_1}=\frac{\tau_0-\kappa}{\tau_1-\kappa} \quad . \quad . \quad . \quad . \quad . \quad . \quad (65.)$$

That is to say: *when no heat is employed in producing variations of temperature, the ratio of the heat received to the heat emitted by the working body of an expansive machine, is equal to that of the absolute temperatures of reception and emission, each diminished by the constant* κ, *which is the same for all substances.*

Hence let

$$\Pi = -Q_1 - Q_0 = H_1 - H_0$$

denote the maximum amount of power which can be obtained out of the total heat H_1, in an expansive machine working between the temperatures τ_1 and τ_0. Then

$$\frac{\Pi}{H_1} = \frac{\tau_1 - \tau_0}{\tau_1 - \kappa} \quad . \quad . \quad . \quad . \quad . \quad . \quad (66.)$$

being the law which has been enunciated in Article 42, and which is deduced entirely from the principles already laid down in the Introduction and First Section of this paper.

The value of the constant κ is unknown; and the nearest approximation to accuracy which we can at present make, is to neglect it in calculation, as being very small as compared with τ.

(44.) This approximation having been adopted, I believe it will be found that the formula (66.), although very different in appearance from that arrived at by Professor Thomson, gives nearly the same numerical results. For example: let the machine work between the temperatures 140° and 30° centigrade: then $\tau_1 = 414°{\cdot}6$, $\tau_0 = 304°{\cdot}6$, and

$$\frac{\Pi}{H_1} = 0{\cdot}2653$$

Professor Thomson has informed me, that for the same temperatures he finds this ratio to be 0·2713.*

(45.) To make a steam-engine work according to the conditions of maximum effect here laid down, the steam must enter the cylinder from the boiler without diminishing in pressure, and must be worked expansively down to the pressure and temperature of condensation. It must then be so far liquefied by conduction alone, that on the liquefaction being completed by compression, it may be restored to the temperature of the boiler by means of that compression alone. These conditions are unattainable in steam-engines as at present constructed, and different from those which form the basis of the formulæ and tables in the Fourth Section of this paper; hence it is found, both by experiment and by calculation

* From information which I have received from Professor Thomson subsequently to the completion of this paper, it appears that his formula becomes identical with the approximate formula here proposed, on making the function called by him $\mu = \frac{J}{\tau}$, J being Joule's equivalent.

Mr Joule also, some time since, arrived at this approximate formula in the particular case of a perfect gas.

from those formulæ, that the proportion of the total heat converted into power in any possible steam-engine is less than that indicated by Equation (66.)

The annexed Table illustrates this:—

CASE.	Absolute temperature in the boiler $=\tau_1$ centigrade.	Absolute temperature in the condenser $=\tau_0$ centigrade.	Total heat expended in centigrade degrees applied to liquid water.	Heat transformed into expansive power, in centigrade degrees applied to liquid water.	Proportion of heat rendered effective.	Maximum proportion according to Carnôt's Law.
First Ideal Example in Section 4, Art. 32,	144°·1 + 274°·6 = 418°·7	81°·7 + 274°·6 = 356°·3	568°·7	83°·2	0·1463	0·1490
Second Ideal Example,	170°·9 + 274°·6 = 445°·5	100° + 274°·6 = 374°·6	558°·6	86°·3	0·1545	0·1592
Mr Wicksteed's Engine, Experiment F, by calculation, . .	135°·2 + 274°·6 = 409°·8	30° + 274°·6 = 304°·6	617°·7	71°·2	0·1153	0·2567
Do., by observation, . .	Ditto.	Ditto.	Ditto.	73°·23	0·1185	Ditto.
(1)	(2)	(3)	(4)	(5)	(6)	(7)

The heat transformed into power, as given in the fifth column, has been reduced to centigrade degrees in liquid water, by dividing the duty of a pound of steam by Mr Joule's equivalent, 1389·6 feet per centigrade degree. Hence the first two numbers in that column are less than those given in Art. 32, which were computed from too small an equivalent.

The first two cases fulfil the conditions required by Carnôt's law in every respect except one, viz.:—that the steam remaining at the end of the stroke, instead of being partially liquefied by refrigeration, and then reduced to water at the temperature of the boiler by compression, is supposed to be entirely liquefied by refrigeration. This occasions the loss of the heat necessary to raise the water from the temperature of the condenser to that of the boiler; but at the same time, there is a gain of the power which would be required to liquefy part of the steam by compression, and those two quantities partially compensate for each other's effects on the ratio of the power to the heat expended, so that although it is below the maximum, the difference is small.

In the third and fourth examples, founded on the calculated and observed duty of Mr Wicksteed's engine during experiment F, the actual ratio is less than half the maximum. This waste of heat is to be ascribed to the following causes.

First, The mode of liquefaction, which has already been referred to.

Secondly, The initial pressure in the cylinder is but 18·93 lb. on the square inch, while that in the boiler is 45·7; so that although the steam is produced at 135°·2 centigrade, it only begins to work at 107°·26. This great fall of pressure is

accounted for by the fact, that the steam for each stroke, which is produced in the boiler in about seven or eight seconds, escapes suddenly into the cylinder in a fraction of a second.

Thirdly, The expansive working of the steam, instead of being continued down to 30° centigrade, the temperature of the condenser, stops at a much higher temperature, 74°·66. This is the most important cause of loss of power.

If we now take for τ_1 and τ_0 the absolute temperatures at the beginning and end of the expansive working, and calculate the maximum duty of one pound of steam by CARNÔT's Law between those temperatures, we find,—

$$\tau_1 = 107°{\cdot}26 + 274°{\cdot}6 = 381°{\cdot}86$$
$$\tau_0 = 74°{\cdot}66 + 274°{\cdot}6 = 349°{\cdot}26$$

$$\frac{\Pi}{H_1} = 0{\cdot}08542$$

$H_1 = 564°{\cdot}5$; $\therefore \Pi =$ . . . . . 48°·22

To this has to be added the duty, at full pressure, of steam at τ_0, diminished by one-third for back-pressure and friction, and by one-fifteenth for liquefaction in the cylinder, = ... 23°·14

The whole amounting to 71°·36

Which agrees very nearly with 73°·23, the observed duty, and almost exactly with 71°·2, the duty as calculated by the formulæ and tables of Section Fourth.

These examples shew clearly the nature and causes of the waste of heat in the steam-engine.

BRITISH MUSEUM
NATURAL HISTORY

Fig. 1

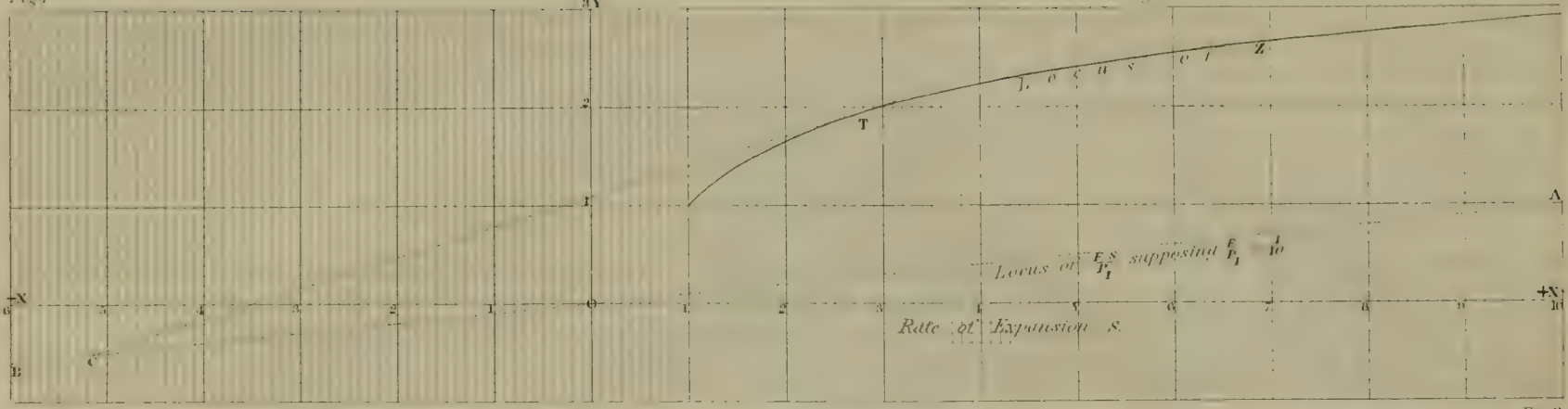
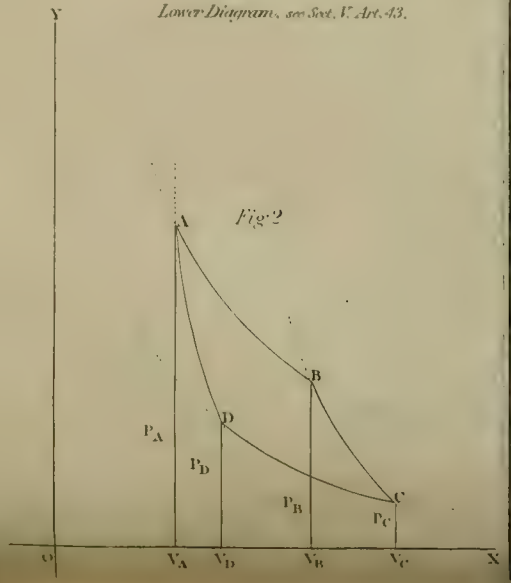


Fig. 3

RANKINE
on the
MECHANICAL ACTION OF HEAT.

Upper Diagram...to determine the most economical rate of Expansion in Single Acting Steam Engines see Sect. IV. Art. 37, 38.

Lower Diagram, see Sect. V. Art. 43.



XI.—*Notes on the Geology of the Eildon Hills, in Roxburghshire.* By James D. Forbes, Esq., F.R.S., Sec. R.S. Ed., Professor of Natural Philosophy in the University of Edinburgh.

(Read 7th April 1851.)

The following remarks, being the result of a careful examination of a small district of country characteristic of the relations of the trap formations, are perhaps worthy of being recorded; although the general features of the county of Roxburgh have been very clearly stated in a paper by Mr Milne, published in the 15th volume of the Edinburgh Transactions.

The outburst of porphyritic trap forming the conspicuous small group of the Eildon Hills, may be stated to be surrounded by the characteristic greywacke of the south of Scotland. It forms an elongated patch on the map, extending from the west end of Bowden Muir in the direction of the town of Selkirk, and running from west-south-west to east-north-east (true) towards Bemerside Hill, on the north bank of the Tweed. The breadth is variable, probably less than is generally supposed; but it cannot be accurately ascertained, owing to the accumulated diluvium which covers the whole south-eastern slope of this elevated ridge. On this account, my observations on the contact of rocks have been almost entirely confined to the northern and western boundaries of the trap, although the other side was examined with equal care.

The character of the greywacke strata near Melrose is in general that of the surrounding country. The strike is nearly due east and west, the position nearly vertical, rather declining to the north; and these features are remarkably uniform and uninterrupted. In the excellent sections exhibited by the course of the railway, immediately to the east of Melrose, where the greywacke is not far distant from the trap of the Eildon Hills, the strike of the strata inclines more to the south-west, the strata are thinner and more undulating, mixed with more numerous clayey strata, and including many veins of calcareous spar. If we follow the greywacke strata to the eastward, we find them exposed near the village of Newstead, and along the south bank of the Tweed towards Drygrange Bridge. Between these two points they are so much altered as to be scarcely recognisable, yet having the usual stratification from east to west. There is every appearance of a real barrier having crossed the present course of the river, which still runs in a very uneven channel; and behind this barrier is an enormous accumulation of *debris* of all sorts, forming the eminences through which the railway passes, beyond the village of Newstead, which have no nuclei of solid rock, as far as can be seen. Among these *debris*, boulders of the trap tufa of Melrose are conspicu-

ous, which appear to be derived more immediately from boulders of that rock imbedded in the drift formation. It is also evident that the partial or complete removal of the barrier of altered rock just mentioned has changed the course of the Tweed, which appears once to have swept over the site of the present village and abbey of Melrose, forming the well-marked cliffs at Newstead, which may also have been the boundary of a fresh-water lake, whose depth depended on the height of the rocky barrier. The remarkable promontory of Old Melrose, nearly three miles below the present village, and the picturesque site of the *original* abbey of that name, founded, as is stated, in the end of the sixth century, is unquestionably owing to the prolongation of the trap-formation of the Eildons, which here becomes very narrow, crossing the Tweed just below Gladswood, and probably uniting itself to the trap of Bemerside Hill. The greywacke strata may easily be traced on each side of the narrow belt of trap on which the mansion-house of Old Melrose stands.

If we now return to the little basin of the village of Melrose, close under the north foot of the Eildon Hills, we find the following arrangement of the rocks, the understanding of which will be facilitated by the inspection of the map, Plate VIII., fig. 3, where the outlines of the formations are marked, and reference is made by numbers to the principal specimens, and by lines to the *strike* of the strata, where it has been observed.

In the course of a little stream passing through the town of Melrose, called Matty's or Dingleton Burn, the greywacke strata may with care be observed almost continuously; and it is remarkable that they exhibit the east and west strike* and vertical dip with scarcely any alteration until we approach the farm-house of Dingleton Mains, when they become suddenly much confused at the point marked 3. In the field above Dingleton farm occurs a quarry of felspar porphyry, including much quartz (specimen M. 4 a.†). This seems to be an offset from the trap of the north-east Eildon Hill, the greywacke appearing higher up (at 5 and 6) nearly unaltered, and may be traced almost to the head of the small streams which rise between the Eildons, and afterwards join the Dingleton Burn. It has probably not been suspected that so large a portion of this face of the Eildons is formed of the rock of the surrounding country. The greywacke skirts the base of the principal Eildon Hill, the portion with the porphyry passing a little to the south of a water-tank on the moor, near the point marked 17 on the map, where the position of the strata is east by north, and vertical. From this point the junction trends round the north slope of Bowden Muir, until we reach Cauldshiels Loch, on the Abbotsford property, where the junction is well marked on the eastern bank.

* The deviation from true east and west is less than 5°.

† The collection of specimens referred to in this paper has been placed in the Museum of the Royal Society of Edinburgh.

In the little basin to the south of Melrose, which has been so far described, we farther find a local and nearly concealed deposit of the red or Dryburgh sandstone, which possesses considerable interest. It lies between the back of the "Quarry Hill," which is a remarkable eminence of trap tufa close to the railway station at Melrose, and the strata of greywacke which we have seen to skirt continuously the north-west slopes of the Eildons. This curious deposit may be easily detected in the wood inclosing a very small ravine with the local name of "the Duke's Glen," and whose position will be best indicated by the numbers 28 and 29 on the map. It is here very nearly in contact with the trap tufa just mentioned. The strata absolutely resemble those at Dryburgh, four miles lower down the banks of the Tweed. They are purplish-red and white alternating, consisting of sandstone mixed with much slate-clay, and are here occasionally very much altered in texture; the soft sandstone becoming very white and crystalline, and the slate-clay becoming extremely hardened, without losing its power of being diffused in water by steeping. The strata are horizontal; and they are intermixed in some places with trap rock, intermediate between trap tufa and felspar rock. The altered sandstones and shales extend up both branches of the little stream until they touch the greywacke between the numbers 18 and 19, the former being iron-shot strata of greywacke, vertical and running north-east by east, the latter is the altered slate-clay of a pearl-grey colour, which can here only form a narrow strip dividing the red sandstone from the Eildon trap. I have not succeeded in tracing this patch of red sandstone farther west, at least with any certainty.

I now come to speak of the trap tufa of Melrose, a rock always interesting in its geognostic relations, and on which my repeated examinations throw some light. It is a very perfect rock of its kind; including numberless fragments of felspar porphyry, usually rather small, and united by an earthy basis, which is either of a yellowish-brown or of a leaden-grey colour. It also contains many small fragments of a pearl-grey hue and uniform texture. These I believe to be portions of the altered slate-clay already spoken of. It is rather extensively quarried as a building material, for which it is exceedingly well adapted, as it is soft at first, and hardens on exposure.

In the Dingleton Burn, already mentioned, it may be seen that the vertical strata of greywacke run towards the "Quarry Hill," without the slightest discontinuity or swerving; and though we cannot trace the junction, it is all but certain that the mass of trap tufa must cut off the greywacke strata *abruptly*. I was able to detect traces of the greywacke in a very imperfect section immediately behind the Melrose Station, which is within a short distance of the lofty escarpment of trap tufa, so that the transition is probably extremely abrupt. The trap tufa is separated *throughout* from the Eildon trap by greywacke strata. I imagine that it is more recent than the Eildon trap. It has unquestionably succeeded the deposit of the Dryburgh sandstone, as is also manifested by the alterations which

we observe on the same sandstone by another patch of trap tufa on the south bank of the Tweed, opposite Dryburgh Abbey, to which I was directed by Mr Milne's map, and which perfectly resembles the Melrose tufa; but it is evidently separated from it by the entire mass of the Eildon Hills and the adjoining greywacke rocks.*

The Melrose tufa is completely lost to the north, in consequence of the ancient excavations occasioned by the river Tweed as already mentioned; it sinks under flats and mounds of *débris*. But it may be traced to the eastward in the bed of the Huntly Burn, close to the house of that name, and to the villa of Chiefswood. It also extends to the Rhymer's Glen, forming evidently a tongue, which runs up between the narrow belt of greywacke which continues to fringe the trap of Bowden Muir and the well-marked greywacke ridge parallel to it on the north, which stretches to Faldonside. The section in the Rhymer's Glen is not without interest. Characteristic trap tufa (No. 26) first appears from under the detritus of the valley in the bed of the small stream. This unquestionably belongs to the same mass as the Melrose tufa. It may be traced up the stream of the Rhymer's Glen, until it passes into a yellowish felspar rock in a gradual manner, which is probably in contact with the greywacke strata which succeed in almost vertical strata. Some of these strata are exceedingly hard, and form the barrier at the first waterfall. It is here in contact with a singular bed of a coaly appearance, which I believe has been mistaken for an indication of the coal formation, which, however, it cannot be, as it is interstratified with the hardened strata of greywacke just mentioned, which, it may be added, include traces of common galena (No. 24), and are traversed by calcspar veins (No. 25). The dark bed is a shale (No. 23) resembling alum shale, mechanically diffusible in water, and including soft whitish fragments resembling steatite. At the highest waterfall in the Rhymer's Glen, the greywacke strata (which here run in a direction of east by north) are interrupted by a dyke of felspathic trap, sometimes of a purplish, sometimes of a yellow colour, and which I have no doubt is the same vein as may be discovered in the greywacke on the east side of Cauldshiels Loch (No. 20), not far from its contact with the main mass of the porphyry of Bowden Muir, of which this vein may be an offset. The waterfall above mentioned is unquestion-

* The deposit in question occurs at the house of Holmes, exactly opposite to Dryburgh Abbey. The course of the Tweed is here north-north-west to south-south-east. The strata on both sides of the tufa mass are red and white sandstone, stratified nearly horizontally with some slate-clay. At the north junction the strata cannot be *distinctly* traced to within 50 or 60 yards of the trap; but, when the river is low, a better view might be had. The tufa rock, however, is modified and compacted, including large and small nodules of rounded quartz, and, in one place, includes soft angular fragments (perhaps of slate-clay), which give it almost a porphyritic appearance (specimen No. 30). The characteristic tufa rock (No. 31) may be traced 100 yards or so up a little side ravine, but is then completely lost under diluvium. The southern part of the tufaceous mass becomes very compact, and assumes the character of a very tough felspar porphyry (No. 32). The sandstone strata in contact with it are hardened and bleached in a remarkable manner.

ably very near the mass of Bowden trap. The purple and yellow trap-dyke may probably be identified also with one (No. 29) cutting the new red sandstone in the "Duke's Glen," behind the Quarry Hill at Melrose, already referred to.

I shall conclude with some observations on the structure of the Eildon Hills themselves. We have seen that the greywacke formation rises to within 200 feet or thereabouts of the level of the *col* or neck which unites the two principal eminences. At this very level occurs a tolerably marked shelf of diluvium, which has strongly the appearance of having been caused by a temporary sojourn of stagnant water at that height. Mr Milne has very correctly remarked, that *the drift* on the Eildon Hills includes fragments of bright red sandstone. This phenomenon is better marked, however, on the south side of the col or neck above referred to. It is an inquiry of some interest whence these fragments could possibly have been derived so as to have been transported by water or otherwise to so high a level. The last visible greywacke strata (at 5 or 6) are not much altered (whilst nearer the farm of Dingleton the alteration is very marked, the strata being iron-shot and hardened, and the direction of strike in some places changed). Here the rock is sandy and of natural hardness, the strata nearly vertical, and running almost due east and west; in short, in almost exact parallelism to the general stratification of the country. Yet this must be very close to the contact with the great mass of porphyry of the Eildons, though the junction can no where be perceived. In ascending slopingly to the top of the highest Eildon by its north-west acclivity, I found many blocks, apparently of altered greywacke, having a singular character, some quite injected (as it appeared to me) with felspar, yet distinguishable almost by the *touch* from felspar rock, having a peculiar *gritty* feel. These blocks appeared to have fallen from small cliffs above, which, having ascended, I found to display a progressive alteration or metamorphosis from the trap rock of the hill into a rock having in one place almost the character of gneiss, and which I take to be a portion of indurated greywacke caught up by the trap, and forming the greater part of the summit of the Eildon, whose bold form arises in part from the excessive resistance of such metamorphic rocks to the action of the weather. The real trap which has effected this metamorphosis is a porphyritic claystone, and the whole somewhat resembles the well-known features of the geology of the Pentland Hills at Habbie's How.*

Repeated visits and a careful selection of specimens confirmed this view. Specimens of the brick-red felspar passing into claystone porphyry are found in Nos. 7, 11, 15. As we approach the top it becomes slaty, and the direction of cleavage *shifts round*, dipping towards the centre of the cone, the summit being what appeared to me the altered rock. The slaty felspar acquires green dots (Nos. 8 and 9.) Then we have the slaty rock *shot* with red felspar (12), before

* Mr Milne describes the top of Eildon as composed of a very hard clinkstone with a grey basis, which strikes fire with steel. But true clinkstone could not do so, being a pure felspar.

referred to. The most perfect green rock is found at (10) to the north-west of the summit, where it occurs chiefly in large detached blocks and nearly in contact with the porphyry (11) without much apparent intermixture. Here, then, the contrast is sharpest. On the opposite or south-east side of the hill-top, which is steep, the transition is imperceptible. The top is a yellowish-grey slaty rock passing into red slaty felspar.

Should these views be confirmed by other observers, we have here an interesting and accessible example of that mysterious operation of metamorphic action which is now so much insisted on by geologists, but of whose exact physical and chemical nature we know so little.

The *col* uniting the two higher Eildons is composed of a hard red porphyry (15) having rude cleavage planes running nearly east and west, and dipping to north. The north-eastern summit of the Eildons I examined carefully without tracing in any point its junction with other rocks ; it is composed of a remarkably uniform claystone porphyry. I did not perceive any imbedded fragments of greywacke in this part of the hill such as seem to be referred to by Mr Milne.

The south-western hill of the three-headed Eildon group includes, as is well known, a felspar porphyry in rather perfect columns. I observed no other peculiarity. As already observed, the whole south-eastern base of the group is so hopelessly covered by soil and drift that the examination of the subjacent rocks is impossible.

LIST OF SPECIMENS,

(now in the Museum of the Royal Society.)

M. 1. a. Hardened trap tufa, back of Quarry Hill.
M. 1. b. Trap tufa, Quarry Hill.
M. 1. c. Do. do.,
M. 2. Altered greywacke, Matty's Burn, below Dingleton Mains.
M. 3.
M. 4.
M. 4. a. Porphyry, including quartz, above Dingleton Mains.
M. 5. Greywacke sandstone on Eildon, below *col*, west side.
M. 6. Hardened greywacke on Eildon, near *col*, west side.
M. 7. Felspar porphyry, foot of cone of centre Eildon.
M. 8. Felspar rock with green patches, centre Eildon.
M. 9. Do. do.
M. 10. Altered greywacke, north-west of centre Eildon top.
M. 11. Porphyry, near greywacke slate, No. 10.
M. 12. Altered greywacke slate, shot with felspar, centre Eildon.
M. 13.

M. 14. Much altered slate, north-east of Eildon top.
M. 15. Porphyry; *col* between Eildons.
M. 16. Porphyry, Eastern Eildon.
M. 17.
M. 18. Altered greywacke, Duke's Glen.
M. 19. Much altered specimen of Dryburgh sandstone, Duke's Glen, near No. 18.
M. 19. a. Slaty porphyry, west part of Eildon, near Bowden Muir.
M. 20. Porphyry, Cauldshiels Loch.
M. 21. Variegated porphyry, Rhymer's Glen.
M. 22. Porphyry near greywacke, Rhymer's Glen.
M. 23. Black shale associated with greywacke, Rhymer's Glen.
M. 24. Do. do., containing galena.
M. 25. Greywacke, near the above.
M. 26. Trap tufa, Rhymer's Glen.
M. 27. Specimen of Dryburgh sandstone from Duke's Glen, horizontal.
M. 28. Do. do., not altered.
M. 29. Felspar porphyry, apparently traversing sandstone in Duke's Glen.
M. 30. Porphyritic rock near boundary of trap tufa, The Holmes, opposite Dryburgh.
M. 31. Trap tufa, do. do.
M. 32. Felspar porphyry, do. do.
M. 33. Altered sandstone, close to No. 32.
M. 34. Modified felspar rock, do.

XII.—*On a New Source for obtaining Capric Acid, and Remarks on some of its Salts.* By Mr Thomas Henry Rowney, F.C.S.

(Read March 17, 1851.)

The following examination of capric acid and some of its salts, was made in the laboratory of Dr T. Anderson, to whom I am much indebted for his kindness in procuring for me the materials to work upon, and also for advice during the progress of this investigation. Capric acid has been found by Chevreul and Lerch in the butter of the cow and goat; by Redtenbacher,* amongst the volatile oily acids he obtained by acting on oleic acid with nitric acid; by Gerhardt and Cahours,† by the action of nitric acid on the oil of rue; and by Görgey,‡ in cocoa-nut oil;—from all these sources it has been obtained only in small quantities, and always along with other acids of the series to which capric acid belongs.

In the present paper I have to point out a new source for obtaining it, namely, the fousel oil or grain oil of the Scotch distilleries. The principal constituents of grain oil are water, alcohol, and the hydrated oxide of amyl. The proportions of these constituents vary in the oils obtained from different distilleries; in some cases it is soluble in water, and then consists chiefly of water and alcohol, with a small quantity of the hydrated oxide of amyl; generally, it is an oily liquid lighter than, and insoluble in, water. Besides the three above-mentioned constituents, other compounds have been found in it in small quantities. Mulder§ found œnanthic acid, and Kolbe‖ found margaric acid. In the oil examined by myself, an acid was found, which analysis proved to be capric acid. In what state it exists in the oil, I am not able to say, but I think it most probable that it is in combination with the hydrated oxide of amyl.

To obtain the capric acid, the grain oil was distilled with a thermometer placed in the tubulure of the retort, and the distillate collected in separate receivers. The first portion consisted of water, alcohol, and the hydrated oxide of amyl; the second portion was the hydrated oxide of amyl, and a dark-coloured residue was left. This residue was oily, it had a very disagreeable smell, and was insoluble in water and KO, CO_2, even when boiled with a solution of the latter; when boiled with a strong solution of caustic potassa it is rendered soluble in water. Whilst boiling, a strong smell of the hydrated oxide of amyl is

* Journal of the Chemical Society, Part 19.
† Annales de Chemie et de Physique, 3d Series, Tome 24.
‡ Annalen de Chemie und Pharmacie, Band 56.
§ Liebeg's Annalen, Bd. 24, p. 248.
‖ *Idem*, Bd. 41, p. 35.

given off; and if the operation be performed in a retort with a receiver attached, this compound is found floating on the water that passes over during the operation. It is also rendered soluble by being digested with a strong solution of caustic potassa for two or three days on the sand-bath. On adding HCl or HO, SO_3 to the cold alkaline solution, a dark oily mass rises to the surface; this was filtered and washed with cold water. I found that the best way to obtain the acid pure, was to dissolve it in a dilute solution of NH_4O, and then to add Ba. Cl until it ceased to give a precipitate; this precipitate was filtered and washed with cold water, then dissolved by boiling with water, filtering whilst hot, and allowing the filtrate to crystallize. It is sometimes dissolved with difficulty by boiling water, owing to its forming hard soapy masses during the boiling; by crystallizing the baryta salt two or three times, it becomes nearly colourless. It was then decomposed by boiling with NaO, CO_2, and filtered from the precipitated BaO, CO_2, dilute HO, SO_3 was added to the filtrate to separate the capric acid, and by these means it was obtained nearly colourless, and in a solid state. To obtain it perfectly pure, it was dissolved in alcohol, and a large quantity of water was added to the alcoholic solution, the mixture becomes turbid, and after standing for some hours capric acid crystallizes out; and by repeating this process it may be obtained perfectly pure and colourless. The mother liquors from the baryta salt were concentrated by evaporation and then boiled with NaO, CO_2, filtered from the BaO, CO_2, and the filtrate decomposed by HO, SO_3; the capric acid obtained from this portion was mixed with a small quantity of an oily acid, the quantity was so small that I was not able to ascertain its constitution.

Capric Acid.

Capric acid, as obtained in the manner I have described, is a solid, white, and crystalline compound, having a faint odour, and fuses readily when taken between the fingers. It is very soluble in cold ether and alcohol, and does not crystallize from these solutions. It is insoluble in cold water, but dissolves sparingly in boiling water, and crystallizes from this solution on cooling in the form of scales. It is also soluble without decomposition when boiled with concentrated nitric acid, and is precipitated from this solution by the addition of water. It is obtained in a mass of needle-shaped crystals by the addition of water to the alcoholic solution. Its specific gravity is less than that of water. The crystallized acid commences to fuse at 81° Fahr., and the mercury of the thermometer continues to rise to about 116 Fahr. before the whole is completely fused. When allowed to cool, it becomes solid at 81° Fahr. The fused acid is slightly coloured, and has a faint smell; it becomes crystalline on cooling. This fusing point differs from that generally given. Görgey gives it at 86° Fahr., and others have stated it to be from 60 to 66 Fahr. These differences probably arise from impure acids having been used. For analysis the crystallized acid dried *in vacuo* over sulphuric acid was employed.

I. ·3759 grammes of substance gave
·9558 ... of carbonic acid, and
·3930 ... of water.

II. ·3176 grammes of substance gave
·8080 ... of carbonic acid, and
·3330 ... of water.

	Theory.		Experiment.		
			I.	II.	Mean.
C_{20} . .	120	69·76	69·35	69·38	69·36
H_{20} . .	20	11·62	11·61	11·65	11·63
O_4 . .	32	18·62	...	...	...
	172	100·00			

Caprate of Silver.

This salt is formed when AgO, NO_5 is added to a slightly ammoniacal solution of capric acid. It is insoluble in cold water, sparingly soluble in boiling water, and is deposited again on cooling in needle-shaped crystals. It is more soluble in boiling alcohol, but the solution becomes dark-coloured, and the crystals deposited from it are also dark-coloured. GÖRGEY also observed this change. It is very soluble in ammonia, and if the ammoniacal solution be kept in a warm place, so as to drive off the ammonia, a crystalline salt is obtained; but not having a sufficient quantity, no examination of this compound was made. The silver salt whilst moist is rapidly blackened, if exposed to bright daylight; but after drying it may be exposed to the light without undergoing any change. The silver salt for analysis was precipitated and washed during the evening, dried *in vacuo* over sulphuric acid, the receiver being covered with a cloth, to prevent the access of light to it; it was then dried in a water-bath at 212° Fahr.

I. ·2485 grammes of silver salt gave
·0951 ... of silver.

II. ·3050 grammes of silver salt gave
·1175 ... of silver.

III. ·2715 grammes of silver salt gave
·1050 ... of silver.

IV. ·4265 grammes of silver salt gave
·6650 ... of carbonic acid, and
·2617 ... of water.

V. ·3402 grammes of silver salt gave
·5282 ... of carbonic acid, and
·2062 ... of water.

	Theory.		Found.					
			I.	II.	III.	IV.	V.	Mean.
C_{20} .	120	43·01	...	...	...	42·52	42·34	42·43
H_{19} .	19	6·81	...	...	...	6·81	6·73	6·77
O_4 .	32	11·47	...	...	...	...	...	...
Ag .	108	38·71	38·27	38·52	38·67	...	...	38·49
	279	100·00						

Caprate of Baryta.

The baryta salt was obtained by adding Ba Cl to an ammoniacal solution of capric acid, the precipitate was filtered and washed with cold water. It is soluble both in water and alcohol when boiled with these liquids, and crystallizes out from these solutions, on cooling, either in needle-shaped or prismatic crystals; the crystals obtained from the alcoholic solution are sometimes of considerable size. This, as also the other salts of the alkaline earths, and the silver salt, are insoluble in water after having been dried, as they float on the surface of the water and repel it; but by first moistening the salt with alcohol, they may be again rendered soluble in boiling water. They are also very difficult to powder and mix for analysis.

The salt analyzed was crystallized from water, and dried in the water-bath at 212° Fahr. The baryta was determined as BaO, SO_3, and the combustion was made with chromate of lead.

I. { ·2935 grammes of baryta salt gave
·1415 ... of BaO, SO_3.

II. { ·4375 grammes of baryta salt gave
·2125 ... of BaO, SO_3.

III. { ·2411 grammes of baryta salt gave
·4410 ... of carbonic acid, and
·1750 ... of water.

IV. { ·2335 grammes of baryta salt gave
·4245 ... of carbonic acid, and
·1732 ... of water.

	Theory.		Found.				
			I.	II.	III.	IV.	Mean.
C_{20} .	120	50·08	...	...	49·88	49·58	49·73
H_{19} .	19	7·93	...	...	8·06	8·24	8·15
O_3 .	24	10·02	...	...	...	...	...
BaO .	76·6	31·97	31·65	31·88	...	...	31·78
	239·6	100·00					

Caprates of Lime and Magnesia.

These salts crystallize and have similar properties to the baryta salt, but they are more soluble both in alcohol and boiling water. No analysis was made of the lime salt, but the base of the magnesia salt was determined. The salt employed for this purpose was crystallized from water, and dried at 212° Fahr. The magnesia was determined as $2\,MgO, PO_5$.

{ ·3687 grammes of caprate of magnesia gave
·1145 ... of $2\,MgO, PO_5$.

The formula $C_{20}\,H_{19}\,O_3, MgO$ requires 11·25 per cent. of MgO, and the per-centage obtained by analysis was 11·37 MgO.

I endeavoured to obtain some other salts of capric acid, but as only the salts of the alkaline earths are readily crystallizable, I did not succeed in doing so. The salts I tried were the soda, copper, and lead salts. The copper salt is insoluble in water and alcohol, but soluble in ammonia. The analyses of these salts always gave an excess of base; this was caused by my not being able to obtain a neutral ammoniacal salt of capric acid. The lead salt is insoluble in water, and very sparingly soluble in boiling alcohol; the solution, on cooling, deposits the lead salt in rounded grains.

The soda salt is exceedingly soluble both in cold water and alcohol, and does not crystallize from these solutions. When evaporated to dryness, it dries up to a horny mass, partially crystalline on the surface. It is soluble in absolute alcohol when warmed, and the solution when allowed to cool becomes an opalescent mass. I could not obtain it free from NaO, CO_2, even by means of absolute alcohol, consequently the analysis gave an excess of base.

From the analyses made of the soda and copper salts, they appear to be neutral salts; the formula of the soda salt being $NaO, C_{20} H_{19} O_3$, that of the copper salt being $CuO, C_{20} H_{19} O_3$.

Capric Ether.

This ether I obtained by dissolving capric acid in absolute alcohol, and passing dry hydrochloric acid gas into the solution to saturation. The addition of water to the solution caused the capric ether to rise to the surface as an oily liquid. It was separated from the acid liquid and washed with cold water, and then dried by digesting it with fused Ca, Cl: its specific gravity is ·862. It is insoluble in cold water, but readily soluble in alcohol and ether. As the quantity of the ether was too small to allow of an analysis of it being made, I converted it into the following compound:—

Capramide.

The capric ether was dissolved in alcohol, and a strong solution of ammonia was added to it in a stoppered bottle; after a few days the solution became turbid; this turbidity increased after allowing it to stand for a longer period, and crystals began to make their appearance. The digestion was continued until the whole of the ether had disappeared. The crystals were then filtered off, and the filtrate evaporated to dryness on a water-bath; the residue was dissolved in alcohol, and the addition of water caused the capramide to crystallize from the solution; the whole was dissolved in warm dilute alcohol, and allowed to crystallize. As obtained in this manner it is quite colourless, and crystallizes in brilliant scales, which, when dry, have a bright silvery lustre. It fuses below 212 Fahr., and is insoluble in water and ammonia. It is very soluble in cold alcohol, and in dilute alcohol when warmed in it. Its other properties I could not examine, as I had only suf-

ficient substance for one combustion. For analysis it was dried *in vacuo* over sulphuric acid, and the combustion was made with oxide of copper.

·2088 grammes of substance gave
·5407 ... of carbonic acid, and
·2287 ... of water.

The numbers correspond to the formula $C_{20} H_{21} O_2 N$, which requires C 70·17, H 12·28. The numbers obtained are C 70·62, H 12·17.

XIII.—*On certain Salts and Products of Decomposition of Comenic Acid.* By Mr Henry How. Communicated by Dr T. Anderson.

(Read 7th April 1851.)

The study of the organic acids appears scarcely to have advanced of late years *pari passu* with the other branches of organic chemistry. It seems, indeed, as if the development of each of the different departments of the science had been, to a certain extent, periodical; each engrossing the labours of investigators to the temporary exclusion of the others, themselves to be renewed when some new experiments should reawaken an interest in them.

However this may be, the subject of the natural and artificial bases has proved so productive of interesting results as to have recently become the chosen and almost exclusive field of inquiry, notwithstanding several investigations which have thrown much light on one class of organic acids, namely, that represented by the general formula $C_n H_n O_4$. With the exception of this section, the history of the organic acids remains very imperfect, and in many cases we have but a meagre account of a few of their salts.

These remarks apply with peculiar force to the polybasic acids; and it was with a view to add something to the existing information respecting this important class of bodies that I undertook an examination of the acid which forms the subject of the present paper. Although this is not among those which have been least investigated, many gaps existed in its history which seemed to me worthy of being filled up. I first gave my attention to those of its salts, which had hitherto remained undescribed or been but imperfectly examined, not from their possessing any very marked interest in themselves, but with the idea of obtaining points of comparison between acids likely to occur in the course of the proposed investigation.

My experiments were performed in the laboratory of Dr T. Anderson.

Comenic acid was discovered by Robiquet,* who observed that meconic acid undergoes a change of properties when boiled with water, carbonic acid being evolved, and a product obtained to which he gave the name Parameconic Acid, indicative of isomerism with the original substance. Liebig,† however, pointed out that there was also difference in composition, and proposed the provisional name Metameconic Acid for the new substance, whose composition he represented by the formula $C_{12} H_4 O_{10}$, derived from the analysis of the acid itself and of a silver salt. In a subsequent paper‡ he shewed its bibasic nature, and entered

* Annales de Chimie et de Phys., Tome 51, p. 244. † *Ibid.*, 54, p. 26.
‡ Annalen der Chemie und Pharmacie, Band 26.

fully into the relations between it, which was now named Comenic Acid, Meconic Acid, and Pyromeconic Acid, the product of dry distillation common to both the former bodies. The subject was further discussed by Dr STENHOUSE,* in a paper, to some of the details of which I shall have occasion to refer.

I employed for the preparation of comenic acid the process of ROBIQUET as modified by GREGORY, which consists in boiling crude meconate of lime (or, still better, the acid salt obtained by once treating this substance with boiling water and hydrochloric acid) with a quantity of pretty concentrated hydrochloric acid sufficient to dissolve it. For the purification of the acid which is deposited in the form of very dark-coloured hard crystalline grains, STENHOUSE recommends solution in a slight excess of caustic potass or soda, and recrystallization of the salt deposited from the boiling fluid. I preferred, however, to use ammonia, since, if certain precautions are adopted, a salt is obtained as readily deprived of colour as the potass salt, and much more insoluble in cold water than the corresponding salt of soda; while the mother liquors afforded a convenient means of trying the action of various chemical agents upon the acid. The process I employed consists in boiling the dark-coloured grains in water, with gradual addition of caustic ammonia, till the whole is in solution. The fluid is then immediately filtered. The addition of an excess of ammonia, and the continuance of a boiling heat are to be avoided, as there ensues, if this be not attended to, a curious decomposition, attended with the production of much colouring matter, the explanation of which will be entered into subsequently.

The ammonia salt obtained as above, deposits from the black fluid in yellow hard crystals if the solution is left at rest, but in soft silky prisms when it is agitated; in the latter state the salt is not so readily washed free of the coloured mother liquor. By two or three crystallizations from boiling water, a salt of dazzling whiteness, in fine radiated four-sided prisms, is obtained.

From solutions of this salt, which, when even quite pure, have a faint shade of straw-colour, the addition of concentrated hydrochloric acid throws down comenic acid in the form of a white heavy crystalline powder adhering to the sides of the vessel, which, when dissolved in boiling water, in which it is not very soluble, is deposited from a saturated solution in grains and crusts, almost colourless; but as the solution cools, groups of short prismatic, or sometimes leaf-like, crystals appear, always possessing a characteristic yellowish-red tinge of colour.

The general chemical and physical properties of comenic acid have been already too well described to require any special remarks on my part; I shall therefore proceed at once to the details of the salts I have examined.

Bicomenate of Ammonia.

This salt was obtained and analysed by STENHOUSE, who formed it by solution

* Mem. and Proc. Chem. Soc., vol. i.

of the acid in a slight excess of ammonia, and subsequent concentration *in vacuo* over sulphuric acid. He describes it as "partly amorphous, partly crystalline;" he found that it lost two equivalents of water on drying at 212°. As obtained by the process above given, it is in the form of square prismatic crystals, white and of great brilliancy, presenting when in mass a beautiful appearance. It is very soluble in boiling water, very little soluble in alcohol. It has a strong acid reaction, and is deposited even from a solution of the acid in an excess of hot caustic ammonia, if the boiling has not been continued. It is represented by the formula

$$NH_4O, HO, C_{12} H_2O_8 + 2\,aq.$$

My own analyses agree with this, which is the result of the analyses of Dr STENHOUSE; but a salt containing an additional single atom of water is obtained when strong alcohol is added to cold saturated alkaline solution of comenic acid in ammonia. It falls in groups of radiated prisms.

{ 5·193 grains of the air-dried salt lost at 212° Fahr.
{ 0·713 ... water.

The per-centage calculated from the number is 13·73, while 13·50 is that corresponding to the formula

$$NH_4O, HO, C_{12} H_2O_8 + 3\,aq.$$

Bicomenate of ammonia, in the dry state, sustains a temperature of 350° Fahr. without decomposition or loss of weight. When heated to 390° Fahr. in a closed tube, it blackens and fuses, and on examination it is found to have undergone a change, an acid substance being produced, which I shall describe fully hereafter.

Bicomenate of Potass.

When comenic acid is boiled with a slight excess of caustic potass, it dissolves readily, and the fluid on cooling deposits a salt, which, when washed with cold water, and subsequently recrystallized from the same menstruum boiling, presents itself in groups of short, square, prismatic needles. They are not very readily deprived entirely of colour. They have a strongly acid reaction, and are the anhydrous bicomenate of potass. They gave the following results on analysis; the potass was determined by ignition with a few drops of strong sulphuric acid.

{ 6·250 grains air-dry salt gave
{ 8·497 ... carbonic acid, and
{ 0·987 ... water.

{ 5·291 grains gave
{ 2·335 ... sulphate of potass.

	Experiment.	Calculation.		
Carbon,	37·07	37·07	C_{12}	72
Hydrogen,	1·75	1·54	H_3	3
Oxygen,	...	37·09	O_9	72
Potass,	23·88	24·30	Ko	47·2
	100·00	100·00		194·2

The formula of the above salt is therefore

$$KO, HO, C_{12} H_2 O_8.$$

Bicomenate of Soda.

Comenic acid was dissolved in a tolerably strong solution of caustic soda by boiling; the fluid on cooling deposited two forms of crystals, one in mammillated masses, the other in transparent prisms half an inch in length. On washing the mixture with a little cold, and resolution in boiling, water, no deposit was obtained on cooling, even after the lapse of some hours; but on evaporation of the fluid to about two-thirds of its bulk, groups of mammillary crystals appeared, which when magnified were found to consist of four-sided elongated prisms. From this it appears that the salt is much more soluble than either the potass and ammonia salt, and cannot be employed with advantage in the preparation of comenic acid. It has an acid reaction, and is anhydrous; its analysis is subjoined. The soda was determined by simple ignition, and subsequent weighing of the carbonate of soda produced

6·020 grains dried at 212° gave
1·753 ... carbonate of soda.

The per-centage of soda calculated from this experiment is 17·09 : 17·41, being that corresponding with the formula

$$NaO, HO, C_{12} H_2 O_8.$$

It is obvious, from the foregoing experiments, that neutral salts of comenic acid with the fixed alkalies or with ammonia do not exist in the dry state. That this is not the case with reference to the alkaline earths, I shall now proceed to shew.

Salts of Lime with Comenic Acid.

Finely-powdered comenic acid, mixed with water and an excess of carbonate of lime, decomposes the earthy salt with effervescence, in the cold. When the liquid is boiled for some time, then filtered and allowed to stand, a few rhombic crystals appear; but by far the larger proportion of the acid remains on the filter in combination with the lime, mixed with the excess of carbonate employed. The crystals were in very small quantity; they consisted doubtless of the acid salt, which I obtained more conveniently in another way.

Bicomenate of Lime.—When a cold, saturated, aqueous solution of bicomenate of ammonia is added to a solution of chloride of calcium, brilliant crystals soon begin to appear which gradually increase in quantity. They are, though small, perfectly defined transparent rhombs; they dissolve readily in boiling water, and are deposited on cooling of a larger size than when first obtained. In the following analysis the substance was dried at 250° Fahr., as it was found that the whole water of crystallization was not expelled at 212°, or only after the lapse of a very long time. The lime was determined as sulphate, by ignition with a few drops of sulphuric acid, as the salt swelled up inconveniently when heated by itself.

4·512 grains dried at 250° Fahr. gave
6·755 ... carbonic acid, and
0·788 ... water.

{ 6·475 grains dried at 250° gave
{ 2·520 ... sulphate of lime.

	Experiment.	Calculation.		
Carbon, . .	40·83	41·14	C_{12}	72
Hydrogen, .	1·94	1·71	H_3	3
Oxygen, . .	...	41·15	O_9	72
Lime, . .	16·02	16·00	CaO	28
	100·00	100·00		175

Hence the composition of the salt dried at 250° is represented by the formula

$$CaO\ HO,\ C_{12}\ H_2\ O_8.$$

The crystals contain seven equivalents water of crystallization.

{ 7·177 grains air-dry salt lost at 250° Fahr.
{ 1·893 ... water.

{ 8·757 grains air-dry salt lost at 250° Fahr.
{ 2·270 ... water.

these numbers, when calculated for per-centage, give

I.	II.	Mean.
26·37	25·92	26·15

and 26·47 is that corresponding to the formula

$$CaO,\ HO,\ C_{12}\ H_2\ O_8 + 7\,aq.$$

Neutral Comenate of Lime.—This salt is obtained in the form of crystalline grains, when a solution of the acid ammonia salt, to which an excess of ammonia has been added, is poured into a solution of chloride of calcium. According to the state of dilution of the fluids employed, salts containing different amounts of water of crystallization are obtained, and the appearance of the product varies accordingly. They are all insoluble in water. The well-washed substance gave the following results; the lime being estimated as sulphate, because the salt when dried blows up in a cloud on ignition.

{ 9·670 grains dried at 250° gave
{ 6·270 ... sulphate of lime.

{ 6·015 grains dried at 250° gave
{ 7·545 ... carbonic acid, and
{ 1·280 ... water.

	Experiment.	Calculation.		
Carbon, . .	34·20	33·96	C_{12}	72
Hydrogen, .	2·36	1·88	H_4	4
Oxygen, . .	...	27·75	O_{10}	80
Lime, . .	26·59	26·41	2 CaO	56
	100·00	100·00		212

The formula of the salt, dried at 250°, is therefore

$$2\,CaO,\ C_{12}\,H_2\,O_8 + 2\,H_0.$$

two equivalents of water being retained at this temperature.

As before mentioned, salts containing various amounts of water of crystallization are formed in more or less concentrated solutions. The crystals of that one whose analysis in the dry state is given, were in the form of groups of minute prisms. They were deposited in a tolerably dilute fluid, and lost five atoms of water in drying.

{ 11·785 grains lost at 250°
{ 2·145 water.

which number gives a per-centage of 18·20 : 17·50, being that corresponding to the formula

$$2\,CaO,\ C_{12}\,H_2\,O_8,\ 2\,Ho + 5\,aq.$$

By employing very dilute solutions, a salt was got in very well-defined, small, brilliant crystals, which lost, at 250° Fahr., 31·27 per cent. of water: now the number 31·82 is that required by the formula

$$2\,CaO,\ C_{12}\,H_2\,O_8,\ 2\,Ho + 11\,aq.$$

This dried salt gave 26·35 per cent. of lime, which agrees perfectly well with the results obtained in the former case.

All these neutral salts are converted into basic compounds by simple ebullition in water.

Salts of Baryta with Comenic Acid.

Carbonate of baryta is partially decomposed by comenic acid in the cold, and completely so when heated with an excess in water, the acid comenate of baryta being produced. On the other hand, when a mixture of the acid and an excess of carbonate of baryta is boiled with water, effervescence ensues, but the comenic acid remains undissolved, being in combination with the earth in form of a basic salt. I readily obtained both an acid and a neutral salt by double decomposition.

Bicomenate of Baryta.—A cold saturated aqueous solution of bicomenate of ammonia gives, with a solution of chloride of barium, an immediate precipitate of a crystalline nature. With more dilute solutions, the salt appears more slowly in well-defined transparent rhombs. It is readily soluble in boiling water, and has a strong acid reaction. It loses its water of crystallization at 212°, but very slowly; the dried salt fuses on ignition.

{ 5·878 grains dried at 212° gave
{ 6·874 ... carbonic acid, and
{ 0·905 ... water.

{ 5·797 grains dried at 212° gave, on ignition,
{ 2·525 ... carbonate of baryta.

	Experiment.	Calculation.		
Carbon, . .	31·89	32·19	C_{12}	72
Hydrogen, .	1·71	1·34	H_3	3
Oxygen, . .	...	32·21	O_9	72
Baryta, . .	33·81	34·26	BaO	76·64
	100·00	100·00		223·64

Hence the composition of the salt dried at 212° is expressed by the formula

$$BaO\ HO\ C_{12}\ H_2\ O_8.$$

In the crystals, as appears from the following experiments, two equivalents of this substance are combined with thirteen of water.

{ 14·815 grains air-dry substance lost at 212°
{ 3·105 ... water.

{ 10·721 grains air-dry substance lost at 212°
{ 2·228 ... water.

The per-centage calculation from which,

I.	II.	Mean.
20·95	20·78	20·86

agrees well with the number, 20·73, required by the formula

$$2\,(BaO,\ HO,\ C_{12}\ H_2\ O_8) + 13\ aq.$$

Neutral Comenate of Baryta.—An alkaline ammoniacal solution of comenic acid causes an immediate precipitate in chloride of barium, of minute radiated crystals. In dilute solutions these do not appear immediately, but in a very short time they commence forming, and their quantity increases till the whole fluid is filled. Under these circumstances they present a very beautiful appearance, being in individual groups, whose silky needles radiate regularly from a centre. Under the microscope these needles are seen to be square prismatic crystals.

This salt is insoluble in boiling water, and does not lose its water of crystallization at a temperature of 212°. When dried at 250°, it is almost pyrophoric on ignition, blowing up in a light fiery cloud; for this reason the base was determined as sulphate in the analysis which follows, by ignition with a little sulphuric acid.

{ 5·418 grains dried at 250° Fahr. gave
{ 4·584 ... carbonic acid, and
{ 0·721 ... water.

{ 5·556 grains dried at 250° Fahr. gave
{ 4·194 ... sulphate of baryta.

	Experiment.	Calculation.		
Carbon, . .	23·07	23·27	C_{12}	72
Hydrogen, .	1·47	1·29	H_4	4
Oxygen, . .	...	25·89	O_{10}	80
Baryta, . .	49·54	49·55	2 BaO	153·28
	100·00	100·00		309·28

From the above it appears that this salt, like the corresponding one of lime, retains two equivalents of water at this temperature, its formula being

$$2\ BaO,\ C_{12}\ H_2\ O_8 + 2\ HO.$$

The crystals contain in addition eight atoms of water.

{ 14·380 grains air-dry salt lost at 250°
{ 2·775 ... water, and

{ 11·665 grains air-dry substance lost at 250°
{ 2·180 ... water.

the mean of these numbers, when calculated on 100 parts,

I.	II.	Mean.
19·29	18·77	19·03

agrees perfectly with that,—18·88,—corresponding to the formula

$$2\,BaO,\ C_{12}\ H_2\ O_8,\ 2\ HO + 8\ aq.$$

The crystallized salt when boiled in water is converted into a basic compound, which loses no water at a temperature of 250°. A portion, which had been so dried, gave on analysis 54·5 per cent. of baryta; this is considerably more than would correspond with a normal neutral salt entirely free from water.

Salts of Magnesia with Comenic Acid.

Acid Comenate of Magnesia.—This salt is much more soluble than the corresponding salts of lime and baryta; it crystallizes out after some time, in perfect small rhombs, when strong cold solutions of bicomenate of ammonia and sulphate of magnesia are mixed. When obtained from dilute solutions, by spontaneous or very slow artificial evaporation, these crystals are of very large size, and when possessing the yellow colour so apt to adhere to salts of comenic acid, they very much resemble regular crystals of ferrocyanide of potassium. They are readily soluble in hot water, and react strongly acid. The following is the analysis, the magnesia being estimated as sulphate, by ignition with sulphuric acid:—

{ 7·426 grains dried at 240° Fahr. gave
{ 10·517 ... carbonic acid, and
{ 1·988 ... water.

{ 5·613 grains dried at 240° Fahr. gave
{ 1·863 ... sulphate of magnesia.

	Experiment.	Calculation.		
Carbon, . .	38·62	38·77	C_{12}	72
Hydrogen, .	2·97	2·69	H_5	5
Oxygen, . .	...	47·41	O_{11}	88
Magnesia, .	11·10	11·13	MgO	20·67
	100·00	100·00		185·67

from which it appears that two atoms of water are retained in combination at the temperature of 240 Fahr.; the composition of the so-dried salt being expressed by the formula

$$MgO,\ Ho,\ C_{12}\ H_2\ O_8 + 2\ HO.$$

The crystals contain further six atoms of water, according to one experiment,

{ 16·794 grains air-dry substance lost at 240° Fahr.
{ 3·709 ... water,

which gives for 100 parts, 22·08 ; the number 22·53 is that required by the formula

$$MgO, HO, C_{12} H_2 O_8, 2 HO + 6 aq.$$

Neutral Comenate of Magnesia.—When an alkaline ammoniacal solution of comenic acid is added to a solution of sulphate of magnesia, a salt is precipitated, especially on stirring the fluid, in the form of hard crystalline grains, adhering very much to the sides and bottom of the vessel. Under the microscope, those grains are found to be made up of groups of short prismatic needles. They are insoluble in boiling water. They lose their water of crystallization at 212°, but only after long exposure to that heat; thus dried they give the following results on analysis:—

{ 5·680 grains dried at 212° gave
{ 7·305 ... carbonic acid, and
{ 1·387 ... water.

{ 5·422 grains dried at 212° gave
{ 3·115 ... sulphate of magnesia.

	Experiment.	Calculation.		
Carbon, . .	35·07	34·89	C_{12}	72
Hydrogen, .	2·53	2·42	H_5	5
Oxygen . .	...	42·66	O_{11}	88
Magnesia, .	19·53	20·03	2 MgO	41·34
	100·00	100·00		206·34

from which it appears that the formula expressing the composition of the salt, dried at 212°, is

$$2 MgO, C_{12} H_2 O_8 + 3 HO.$$

I endeavoured, by employing a higher temperature, to obtain a salt corresponding with the neutral salts of lime and baryta, in the amount of water retained at the same heat; but in the experiment I made, the substance lost weight at 250° gradually through a space of four days, and then the loss between each weighing was very small. It yielded 21·3 per cent. of magnesia, which is more by a half per cent. than is required by a salt of the constitution sought for. The neutral comenate of magnesia, precipitated as above mentioned, has the composition expressed by the formula

$$2 MgO, C_{12} H_2 O_8, 3 HO + 8 aq.$$

the eight atoms aq. being lost at 212°.

{ 15·103 grains air-dry substance lost at 212°
{ 4·003 ... water.

This experimental number, calculated for 100 parts, gives 26·50; the number calculated from the above formula is 25·86.

The salts of strontian somewhat resemble in appearance those of baryta, but are more soluble.

It is curious that this acid does not form an acid salt with oxide of copper; the salt with two equivalents of base being obtained both by the addition of comenic acid itself and of acid comenate of ammonia to a solution of sulphate of copper. This salt was analysed by STENHOUSE, who also examined some others, the details of which will be found in the paper already referred to.

Products of Decomposition of Comenic Acid.

By Oxidation.—The conversion of comenic into carbonic, oxalic, and hydrocyanic acids, by the agency of nitric acid, was noted among the first facts connected with the subject. It takes place with very dilute acid. When tolerably strong nitric acid is employed, the action is very rapid and violent, and when once commenced by application of a gentle heat, is completed in very few minutes, though the heat be withdrawn.

Dr STENHOUSE, in the paper before mentioned, states that when comenic acid is kept for some hours at a temperature of 150° Fahr. in a solution of persulphate of iron, yellow crystals are formed, which contain protoxide of iron, and an acid which is not comenic acid. I did not succeed in obtaining a similar result on a repetition of his experiment, possibly because the circumstances were not strictly the same. I think it possible, however, that these crystals consisted of oxalate of protoxide of iron, from the ease with which comenic acid is oxidized, when boiled in a solution of persulphate of iron. I treated a quantity of comenic acid in this way, effervescence of carbonic acid ensued strongly, and the fluid was found to contain much protoxide of iron and oxalic acid. I identified the latter by a preparation and analysis of its lime salt in a pure state, after the removal of the iron and sulphuric acid by appropriate means.

I could not succeed in producing any change by the action of sulphurous acid or of sulphide of hydrogen upon comenic acid.

Action of Chlorine on Comenic Acid.

Chlorocomenic Acid.—When a current of moist chlorine is passed through water holding powdered comenic acid in suspension, a portion of the acid is dissolved, and the clear liquid deposits, after the lapse of some time, long, brilliant, and colourless prismatic needles of the new acid. The same effect is produced when a solution of the ammonia salt is employed, and as, from the more ready solubility of this substance, results were more conveniently obtained, I used it in preference in my experiments.

If an alkaline ammoniacal solution of comenic acid be exposed to the action of chlorine, the first result is a precipitation of the acid comenate of ammonia; but if a cold, saturated, coloured solution of the latter salt be employed, and the

gas be passed through it for some time, the whole of the colour disappears, or the liquid only retains the faint yellowish-green characteristic of an aqueous solution of chlorine, without the formation of an immediate precipitate. After the lapse of some hours, groups of long, colourless, prismatic needles are deposited, the quantity of which is increased by the addition of hydrochloric acid. The mother liquor, on gentle evaporation, gradually acquires a brownish shade of colour, which passes ultimately into a very dark brown, and there deposited a further quantity of the new acid in prismatic crystals, separate and in groups, of a brown, nearly black, lustrous appearance. In this second mother liquor, in addition to the colouring matter, oxalic acid is to be detected. The colourless crystals at first obtained, after washing with cold water, were recrystallized from boiling water, in which they are readily soluble: they acquired, in this process, a slight shade of yellow, and presented themselves in the form of short, thick, square prisms. They gave the following results on analysis:—

{ 5·240 grains dried at 212° Fahr. gave
{ 7·210 ... carbonic acid, and
{ 0·847 ... water.

{ 3·877 grains dried at 212° Fahr. gave, after burning with lime,
{ 2·940 ... chloride of silver.

	Experiment.	Calculation.		
Carbon, . .	37·53	37·79	C_{12}	72
Hydrogen, .	1·79	1·57	H_3	3
Oxygen, . .	...	42·01	O_{10}	80
Chlorine, . .	18·77	18·63	Cl	35·5
	100·00	100·00		190·5

The above shews this substance to be an acid, obtained by the substitution of an equivalent of hydrogen in comenic acid, by an equivalent of chlorine, according to the following equation:—

$$2\,HO,\,C_{12}H_2O_8 + 2\,Cl = 2\,HO,\,C_{12}\left\{\begin{matrix}H\\Cl\end{matrix}\right\}O_8 + HCl.$$

It crystallizes with three equivalents of water, which are readily expelled at 212° Fahr., as the following experiment proves:—

{ 10·528 grains air-dried substance lost at 212° Fahr.
{ 1·313 ... water,

giving for per-centage 12·47, the number 12·41 being that corresponding to the formula

$$2\,HO,\,C_{12}\left\{\begin{matrix}H\\Cl\end{matrix}\right\}O_8 + 3\,aq.$$

This acid, as before mentioned, is readily soluble in hot water, less so in cold, but under both circumstances its solubility is much greater than that of the parent acid; it is very soluble in alcohol when warm. It imparts to persalts of iron the same deep red colour as meconic and comenic acids. When a piece of granulated zinc is placed in its aqueous solution, hydrogen is slowly evolved, and both zinc

and hydrochloric acid are found in the liquid. Nitric acid rapidly decomposes it, with formation of hydrochloric, hydrocyanic, carbonic, and oxalic acids. Submitted to destructive distillation it fuses and blackens, hydrochloric acid is evolved in large quantity, and towards the end of the process a small quantity of a crystalline sublimate appears. This product I obtained in too small quantity to examine thoroughly. I imagine it, however, to be pyrocomenic acid, and attribute the presence of the traces of chlorine I detected to the impossibility of completely purifying the little matter I had at my disposal.

Chlorocomenic, like comenic acid, is bibasic, forming two series of salts. The salts I chose for controlling the analysis, and establishing the saturating power of the acid, were those of silver.

Bichlorocomenate of Silver.—A warm aqueous solution of the acid gives, with nitrate of silver, a white precipitate, in feathery crystals. When freed from the excess of solution of silver and nitric acid by washing with cold water, in which it is sparingly soluble, it may be recrystallized from boiling water, from which it separates on cooling in brilliant, short, prismatic needles. It is not at all, or very slightly, decomposed by boiling in water when no free nitric acid is present. The silver, in the following analysis, was determined by precipitation with hydrochloric acid; the ordinary process of burning the salt and weighing the residuary silver being inapplicable, since a portion of the chlorine of the acid remains in combination with the metal upon ignition.

{ 5·157 grains dried at 212° gave
{ 2·490 ... chloride of silver.

	Experiment.	Calculation.		
Carbon, . .	...	24·19	C_{12}	72
Hydrogen, .	...	0·67	H_2	2
Oxygen, . .	...	24·19	O_9	72
Chlorine, . .	...	11·94	Cl	35·5
Oxide of silver,	39·03	39·01	AgO	116·1
	100·00	100·00		297·6

The composition of the salt, when dried at 212°, is therefore represented by the formula

$$AgO, HO, C_{12}\left\{\begin{matrix} H \\ Cl \end{matrix}\right\} O_8.$$

The crystallized salt appears to be a combination of the above with water, in the proportion of three equivalents of the latter to two of the former; the two specimens of the salt giving this indication were of different preparations.

{ 5·674 grains air-dry salt lost at 212°
{ 0·230 ... water.

{ 5·428 grains air-dry salt lost at 212°
{ 0·253 ... water.

The per-centage calculated from these numbers are,

I.	II.	Mean.
4·22	4·66	4·44

the mean of which agrees well enough with the number 4·33, corresponding to the formula

$$2\,(AgO,\,HO,\,C_{12}\,HCl\,O_8) + 3\,aq.$$

Neutral Chlorocomenate of Silver.—This salt is obtained in the form of a yellow, flocky, amorphous substance, on the addition of a solution of the acid in a slight excess of ammonia to nitrate of silver. It is insoluble in boiling water, and acquires, in the process of drying, the consistence and adhesiveness of clay, which it also closely resembles in appearance.

Considerable difficulty was experienced in the analysis of the salt; since, for the reason mentioned with regard to the acid salt, it cannot be burned, and it is, moreover, insoluble in water. When boiled with hydrochloric acid, a part of it escapes decomposition; and if the attempt be made to dissolve it by aid of nitric acid, and precipitate the silver by hydrochloric acid, care must be taken to prevent the formation of cyanide of silver, which readily takes place when either of the silver salts is kept warm with even dilute nitric acid. The number I obtained was by carefully employing this process, and, though not accurate, comes sufficiently near to prove the composition of the salt.

7·760 grains dried at 212° gave
5·457 ... chloride of silver.

The per-centage of oxide of silver calculated from this is 56·85, and 57·37 is that corresponding to the formula

$$2\,AgO,\,C_{12}\left\{\begin{matrix}H\\Cl\end{matrix}\right\}O_8.$$

The other salts of chlorocomenic acid, as might be anticipated, present close analogies with those of comenic acid; the former are generally more soluble than the latter. I have been unable to prepare neutral salts of the alkalies.

The acid salts of potass, soda, and ammonia, crystallize readily; a solution of the latter salt gives, with chlorides of calcium and barium, radiated groups of needles, appearing more or less quickly according to the state of concentration of the fluids; with sulphate of magnesia, a few crystals after some time; with sulphate of copper, a rapidly-appearing crystalline salt. The neutral salts of these bases appear to be insoluble amorphous substances generally.

Action of Bromine on Comenic Acid.

Bromocomenic Acid.—As might be expected, the behaviour of comenic acid towards bromine is closely similar to that which it exhibits when submitted to the influence of chlorine. It dissolves readily in aqueous bromine, yielding a colourless fluid if the bromine is not in great excess. In the course of a few hours

the new acid is deposited in fine, square, prismatic crystals, often of considerable length, and presenting a very beautiful appearance, from their high refractive power.

It may also be obtained by addition of bromine water to solution of acid comenate of ammonia, but I found it more convenient to employ the acid itself. I may mention that in one instance, when operating upon a solution of the ammonia salt, a considerable excess of bromine failed to yield any new acid, even after the lapse of many hours. The solution remaining colourless, more bromine was added, and as no crystals appeared, the fluid was evaporated, but still without any signs of bromocomenic acid; and it was not until the liquid was reduced to a very small bulk, that any substance crystallized out. On pouring off the liquid, which had now become nearly black, there were found some considerable-sized transparent crystals, together with a little bromocomenic acid in groups. The crystals became perfectly colourless on washing with a few drops of water; they proved to be oxalic acid. This acid always appears in the mother liquors from which chloro and bromo comenic acids have been separated by evaporation, resulting probably from a secondary decomposition.

The crystals, as obtained by the action of bromine water upon comenic acid, after being washed, and recrystallized from boiling water, gave the following results:—

6·001 grains dried at 212° Fahr. gave
6·767 ... carbonic acid, and
0·806 ... water.

4·330 grains dried at 212° gave, when burned with lime,
3·475 ... of bromide of silver.

	Experiment.	Calculation.		
Carbon,	30·75	30·63	C_{12}	72
Hydrogen,	1·49	1·27	H_3	3
Oxygen,	...	34·06	O_{10}	80
Bromine,	34·15	34·04	Br	80
	100·00	100·00		235

which shew that they consist of an acid precisely analogous with chlorocomenic acid; an equivalent of bromine taking the place of one of hydrogen in the comenic acid. In the hydrated state it contains, like the chlorine acid, three atoms of water.

11·14 grains air-dry acid lost at 212°
1·15 ... water,

which corresponds to 10·32 per cent.; 10·30 is the number required by the formula

$$2\,HO\,C_{12}\left\{\begin{matrix}H\\Br\end{matrix}\right\}O_8+3\,aq.$$

This acid so closely resembles the chlorocomenic in its general properties and products of decomposition, that a very few words will suffice to describe it. It is

rather less soluble in hot water and in alcohol than the former acid; it is deposited from alcohol in fine rhombic crystals. It is decomposed by zinc. With nitric acid it gives hydrobromic, hydrocyanic, carbonic, and oxalic acids.

The acid ammonia salt crystallizes in fine long needles; the acid salts of potass and soda also crystallize. I could obtain no neutral alkaline salts. The acid salts of the alkaline earths are very soluble; the neutral salts are insoluble and amorphous.

The acid silver salt was obtained by adding warm aqueous solution of bromocomenic acid to an aqueous solution of nitrate of silver; the flocky precipitate which fell was well washed with cold, and subsequently dissolved in boiling water. This fluid deposited the salt, on cooling, in brilliant, short, prismatic crystals. The silver was determined, in the following analysis, by solution of the salt in boiling water, and the subsequent addition of hydrochloric acid—

6·435 grains dried at 212° gave
2·678 ··· chloride of silver.

which, calculated for per-centage, gives 33·64 oxide of silver; the number 33·93 being that corresponding with the formula

$$AgO, HO, C_{12} \left\{ \begin{matrix} Br \\ H \end{matrix} \right\} O_8.$$

A neutral silver salt was also obtained as a yellow amorphous precipitate, by adding solution of the acid in slight excess of ammonia to nitrate of silver in excess; it presented, on drying, the clayey character I remarked in the corresponding salt of chlorocomenic acid. As there could exist but little doubt of its composition, I thought it useless to occupy time with an analysis of it.

Iodine appeared, from some experiments I made, to be without the power of decomposing comenic acid.

Acid Comenic Ether.

Comenovinic Acid.—From the bibasic nature of comenic acid, and a consideration of the fact that Dr STENHOUSE* failed in a special attempt to obtain a neutral ether of this acid, I was led to seek it in its combination with ether, a compound of an acid nature analogous to sulphovinic, tartrovinic, and the other acids similarly constituted. I did not succeed in my endeavour to form such a substance by action of sulphuric acid on alcohol and comenic acid; but was more successful in a slight modification of the method usually adopted for the production of organic ethers. Comenic acid in the state of fine powder was suspended in absolute alcohol, in which it is insoluble *per se*, and a stream of dry hydrochloric acid gas was passed through the fluid. After some time the whole or the greater part of the acid was taken up, the last portions disappearing very slowly. The clear solution gave no deposit, even on standing at rest for many hours, nor was any precipitate produced by the addition of water, but when it was evaporated to

* Mem. and Proc. Chem. Soc., vol. i.

dryness, at a heat somewhat below 212 , a crystalline residue remained, which was evidently not comenic acid. This was kept at the same heat till it ceased to smell of hydrochloric acid; it was then dissolved in water under the boiling point; the fluid, on cooling, deposited well-defined, square, prismatic needles of considerable size. A portion, dried *in vacuo*, gave the following results on analysis.

I. 5·625 grains, dried *in vacuo*, gave
10·740 ... carbonic acid, and
2·280 ... water.

II. 4·110 grains, dried *in vacuo*, gave
7·865 ... carbonic acid, and
1·715 ... water.

	I.	II.	Calculation.		
Carbon,	52·07	52·18	52·17	C_{16}	96
Hydrogen,	4·50	4·63	4·34	H_8	8
Oxygen,	...	...	43·49	O_{10}	80
	100·00	100·00	100·00		184

From which it will be seen that this substance has the composition of an acid ether, or true vinic acid, and is represented by the formula

$$HO, C_4 H_5 O C_{12} H_2 O_8,$$

and I shall presently shew, that the atom of water is capable of being replaced by bases. The acid crystallizes, like the corresponding compound of tartaric acid, without water.

Comenovinic acid is readily soluble in hot water, and may be boiled a short time without undergoing decomposition; but if long kept at this temperature, comenic acid is reproduced. It is extremely soluble in alcohol. It commences to volatilize, when kept in the dry state, at 212°; it fuses at 275° Fahr. into a transparent brownish liquid, which becomes, on cooling, a crystalline striated mass. When kept at about its fusing point, it sublimes, unaltered in composition, in brilliant, long, flattened prisms, of great beauty; the second analysis above given is that of the sublimed product. It gives a strongly acid reaction with test-papers; its aqueous solution readily coagulates the white of eggs; it imparts to persalts of iron a deep red colour.

Though of so stable a nature *per se*, this substance rapidly decomposes in contact with fixed bases; so that I have been unable to obtain any of its salts in the dry state. All those I have attempted to prepare gave, upon analysis, results closely agreeing with the composition of salts of comenic acid, with which their general properties were also identical, notwithstanding that I carefully avoided application of heat.

I obtained a salt of ammonia by passing the dry gas into a solution of the acid in absolute alcohol. Under these circumstances a precipitate soon forms, in small silky tufts of a yellow colour. They preserve their silky appearance on being

dried, but soon commence to lose ammonia in a dry atmosphere. A portion of the substance which had been exposed one night was placed *in vacuo* over sulphuric acid; it was found to lose less weight by about half a per cent. than would correspond to the ammonia in a salt of the composition.

$$NH_4\,O,\,C_4\,H_5\,O,\,C_{12}\,H_2\,O_8.$$

As the residue was found to consist of pure comenovinic acid, there can be little doubt that the above is a true ammonia salt.

Its other salts, with the alkalies and alkaline earths, are very soluble. Its silver salt is gelatinous, and rapidly decomposable even in the dark.

Decomposition of Comenate of Ammonia.

Comenamic Acid.—I mentioned, in a former part of this paper, that bicomenate of ammonia is decomposed, when subjected to a temperature of 390° Fahr. in a sealed tube. The contents of the tube were a black coaly mass, which partially dissolved in boiling water. The filtered solution gave, with hydrochloric acid, a white scaly precipitate, separating on cooling. I did not endeavour to procure more of this substance in this manner, as I considered it to be the comenamic acid, which a more convenient process enabled me to obtain in sufficient quantity.

When a solution of comenate of ammonia, containing an excess of the alkali, is boiled, it soon becomes coloured, and after some little time a black-red fluid is obtained; if the boiling be continued till the whole or the greater part of the excess of ammonia is expelled, and the fluid be then allowed to cool, a grey sediment falls to the bottom of the vessel. This, when thrown upon a filter, is found to have a most peculiar, clayey, tenacious character; it is the ammonia salt of comenamic acid, very impure, from adhering colouring matter. It dissolves, though sparingly, in boiling water; and hydrochloric acid added in just sufficient quantity to decompose it, precipitates very dark bronze-coloured scales of comenamic acid, which separate completely when the liquid cools. Excess of hydrochloric acid is to be avoided, as the new acid is extremely soluble in this reagent. The dark crystals are readily deprived of their colour by two or three crystallizations from boiling water, and very easily by the aid of animal charcoal, which must, however, for this purpose be entirely free from iron, as the least quantity of this substance imparts a purple colour to solutions containing the acid.

When pure, comenamic acid presents itself in the form of brilliant colourless plates, the following is its analysis:—

I. { 5·540 grains dried at 212° gave
9·345 ... carbonic acid, and
1·685 ... water.

II. { 5·585 grains dried at 212° gave
9·487 ... carbonic acid, and
1·715 ... water.

{ 6·505 grains dried at 212° gave
9·500 ... platinum salt of ammonia.

	I.	II.	Calculation.		
Carbon, . .	46·00	46·32	46·45	C_{12}	72
Hydrogen, .	3·37	3·41	3·22	H_5	5
Oxygen, . .	...	...	41·30	O_8	64
Nitrogen, .	9·17	...	9·03	N	14
	100·00		100·00		155

It is obvious that this substance is an acid amide, analogous to oxamic acid, and that its constitution is expressed by the formula of acid comenate of ammonia minus two atoms of water = $HO, NH_2 C_{12} H_2 O_7$.*

It crystallizes with four equivalents of water.

{ 10·250 grains air-dry acid lost at 212°
{ 1·955 ... water.

{ 7·810 grains air-dry acid lost at 212°
{ 1·450 ... water.

	I.	II.	Mean.
Per-centage, .	19·07	18·56	18·81

The number 18·84 is that corresponding to the formula

$$HO, NH_2 C_{12} H_2 O_7 + 4\,HO.$$

Comenic acid, as obtained above, is in brilliant scales, very slightly soluble in cold water; the crystals effloresce, and partially lose their lustre in a dry atmosphere. It is soluble in boiling spirit, but very slightly in absolute alcohol. It has a powerful acid reaction; dissolves readily in excess of alkalies; also with extreme facility in the strong mineral acids. From a solution in any of these, ammonia, added in quantity not quite sufficient to neutralize the whole of the solvent, throws down a granular precipitate of the ammonia salt. Its aqueous solution imparts to salts of peroxide of iron a magnificent and deep pure purple colour, which is destroyed by a few drops of a mineral acid, but reappears on dilution of the fluid with water. It is decomposed by boiling with caustic potass, with evolution of ammonia and production of comenic acid.

It forms readily crystallizable salts with a certain proportion of ammonia, potass, and soda; these have an acid reaction. The acid remains completely in solution in a small quantity of water, when supersaturated with any alkali; if ammonia be employed, and the fluid be evaporated to dryness at 212°, the salt with acid reaction remains.

It dissolves the earthy carbonates with effervescence, when heated with them in water; if the acid be in excess, a crystalline salt, with an acid reaction, is obtained; if the carbonate predominate in quantity, almost the whole acid remains undissolved as some basic compound.

* I have also obtained this substance from meconate of ammonia; the details of my experiments will be given in a future paper on the subject of some derivatives of meconic acid.

A solution of the crystalline ammonia salt gives, with nitrate of silver, a white gelatinous precipitate, which partially decomposes in boiling water. The same solution made alkaline gives, with nitrate of silver, a yellow flocky precipitate, which almost instantly passes through deepening shades of colour into a heavy, black, amorphous precipitate. The same solutions give, with acetate of lead, heavy insoluble precipitates; the acid solution gives, with sulphate of copper, a grey precipitate.

I have examined more fully and analyzed two or three of the salts of comenamic acid.

Comenamate of Ammonia.—Comenamic acid dissolves readily in ammonia, when the alkali is added in excess, and such a fluid deposits no salt on standing; but if ammonia be added to a boiling aqueous solution of the acid, in such quantity that the reaction remains slightly acid, the ammonia salt crystallizes out on cooling of the fluid in small grains, which, when magnified, are found to consist of bundles of needles radiating from a centre. They are difficult of solution in boiling water, but do not always reappear quickly when the liquid is cold. Their solution shews the phenomenon of epipolic dispersion very beautifully, when rendered alkaline by ammonia. They are anhydrous.

{ 5·623 grains dried at 212° gave
{ 8·570 ... carbonic acid, and
{ 2·446 ... water.

{ 4·945 grains dried at 212° gave
{ 12·715 ... platinum salt of ammonia.

	Experiment.	Calculation.		
Carbon, . .	41·56	41·86	C_{12}	72
Hydrogen, .	4·83	4·64	H_8	8
Oxygen, . .	...	37·23	O_8	64
Nitrogen, . .	16·14	16·27	N_2	28
	100·00	100·00		100

The above analysis leads to the formula

$$NH_4\ O,\ NH_2\ C_{12}\ H_2\ O_7.$$

Comenamate of Baryta.—The salt I analyzed was obtained by adding a solution of the ammonia salt to chloride of barium; a precipitate was obtained in radiated groups, which, on crystallization from boiling water, assumed the form of separate prisms. It had an acid reaction.

{ 4·565 grains dried at 212° gave
{ 5·055 ... carbonic acid, and
{ 1·105 ... water.

{ 4·425 grains dried at 212° gave, when ignited with SO_3,
{ 2·190 ... sulphate of baryta.

	Experiment.	Calculation.		
Carbon, . .	30·20	29·92	C_{12}	72
Hydrogen, .	2·68	2·82	H_6	6
Oxygen, . .	...	30·62	O_9	72
Nitrogen, .	...	5·81	N	14
Baryta, . .	32·02	31·83	BaO	76·64
	100·00	100·00		240·64

The formula deduced from the analysis of this substance, supposing it to be the neutral salt of a monobasic acid will be

$$BaO, NH_2 C_{12} H_2 O_7 + 2\,HO.$$

Comenamate of baryta, precipitated in an alkaline solution of the ammonia salt by chloride of barium, falls as a heavy white powder, insoluble in boiling water. Its analysis is subjoined:—

6·023 grains dried at 212° gave
5·065 ... carbonic acid, and
0·989 ... water.

5·247 grains dried at 212° gave
4·020 ... sulphate of baryta.

	Experiment.	Calculation.		
Carbon, . .	22·93	23·35	C_{12}	72
Hydrogen .	1·80	1·62	H_5	5
Oxygen, . .	...	20·77	O_8	64
Nitrogen, . .	...	4·54	N	14
Baryta, . .	50·29	49·72	2BaO	153·28
	100·00	100·00		308·28

From this analysis it follows, that, to assimilate this salt to the last, it must be considered as a basic compound, in which one of the equivalents of water retained at 212°, is replaced by an atom of baryta, according to the formula

$$BaO, NH_2 C_{12} H_2 O_7 + BaO\,HO.$$

As precipitated from water, it contains an additional equivalent of water:—

11·720 grains air-dry lost at 212°
0·362 ... water.

the per-centage calculation from the experiment is 3·08; and the number 2·83 corresponds to the formula

$$BaO, NH_2 C_{12} H_2 O_7, BaO\,HO + aq.$$

The salts of lime are very similar in appearance to the above, and with every base this acid seems to form two salts, which is a curious fact, since, reasoning from analogy, a substance originating as it does, should be monobasic in its nature. I am not at present able to afford any further information on the subject of its constitution and products of decomposition; but I may mention that I have observed, in its behaviour under certain circumstances, phenomena which I believe may prove of sufficient interest to encourage investigation. I append a list of the salts, &c., mentioned in this paper.

Salts of Comenic Acid.

$NH_4O, HO, C_{12}H_2O_8 + 2$ aq.,	crystallized from water.
$NH_4O, HO, C_{12}H_2O_8 + 3$ aq.,	thrown down from alkaline solution by strong alcohol.
$NH_4O, HO, C_{12}H_2O_8$,	dried at 212° Fahr.
$KO, HO, C_{12}H_2O_8$,	crystallized from water.
$NaO, HO, C_{12}H_2O_8$,	... ...
$CaO, HO, C_{12}H_2O_8 + 7$ aq.,	... ...
$CaO, HO, C_{12}H_2O_8$,	dried at 250° Fahr.
$2\,CaO, C_{12}H_2O_8, 2\,HO + 5$ aq.,	deposited in concentrated cold fluids.
$2\,CaO, C_{12}H_2O_8, 2\,HO + 11$ aq.,	... dilute ...
$2\,CaO, C_{12}H_2O_8, 2\,HO$,	dried at 250° Fahr.
$2\,(BaO, HO, C_{12}H_2O_8) + 13$ aq.,	crystallized from water.
$BaO, HO, C_{12}H_2O_8$,	dried at 212° Fahr.
$2\,BaO, C_{12}H_2O_8, 2\,HO + 8$ aq.,	deposited in cold water.
$2\,BaO, C_{12}H_2O_8, 2\,HO$,	dried at 250° Fahr.
$MgO, HO, C_{12}H_2O_8, 2\,HO + 6$ aq.,	crystallized from water.
$MgO, HO, C_{12}H_2O_8, 2\,HO$,	dried at 240° Fahr.
$2\,MgO, C_{12}H_2O_8\,3\,HO + 8$ aq.,	deposited in concentrated cold fluids.
$2\,MgO, C_{12}H_2O_8\,3\,HO$,	dried at 212° Fahr.

Acids derived from Comenic Acid.

Chlorocomenic acid, crystallized,	$2\,HO, C_{12}\left\{\begin{matrix}H\\Cl\end{matrix}\right\}O_8 + 3$ aq.
... ... dried, at 212°,	$2\,HO, C_{12}\left\{\begin{matrix}H\\Cl\end{matrix}\right\}O_8$
... acid, silver salt, dried at 212°,	$AgO, HO, C_{12}\left\{\begin{matrix}H\\Cl\end{matrix}\right\}O_8$
... neutral	$2\,AgO, C_{12}\left\{\begin{matrix}H\\Cl\end{matrix}\right\}O_8$
Bromocomenic acid, crystallized,	$2\,HO, C_{12}\left\{\begin{matrix}H\\Br\end{matrix}\right\}O_8 + 3$ aq.
... ... dried at 212°,	$2\,HO, C_{12}\left\{\begin{matrix}H\\Br\end{matrix}\right\}O_8$
... acid, silver salt, dried at 212°,	$AgO, HO, C_{12}\left\{\begin{matrix}H\\Br\end{matrix}\right\}O_8$
Comenovinic acid, crystallized,	$HO, C_4H_5O, C_{12}H_2O_8$
... ... ammonia salt,	$NH_4O, C_4H_5O, C_{12}H_2O_8$
Comenamic acid, crystallized,	$HO, C_{12}H_4NO_7 + 4\,HO$.
... ... dried at 212°,	$HO, C_{12}H_4NO_7$
... ... ammonia salt,	$NH_4O, C_{12}H_4NO_7$
... ... baryta salt, with acid reaction, dried at 212°,	$BaO, C_{12}H_4NO_7 + 2\,HO$.
... ... neutral ditto,	$BaO, C_{12}H_4NO_7 + BaO\,HO$.

XIV.—*On the Products of the Destructive Distillation of Animal Substances.* Part II.

By Thomas Anderson, M.D.

(Read 21st April 1851.)

I propose in the following pages to communicate to the Society the progress of my investigation of the products of the destructive distillation of animal substances, the first part of which was published in the 16th volume of the Transactions. Since that period, partly owing to my numerous avocations, and partly to the inherent difficulties of the subject, less progress has been made than I had hoped or expected, but still I have accumulated some facts of considerable interest, which I think deserving of the attention of the Society.

It may be remembered that, in the paper just referred to, I announced the discovery, among those products, of picoline, which I formerly obtained from coal-tar, and of a new base, to which I gave the name of Petinine; and I entered pretty fully into the method adopted for the preparation of these substances, and of certain other bases, the existence of which I merely indicated, without at the time attempting to characterize them. On proceeding to the more minute investigation of these bases, I soon found that the quantity of material at my disposal was much too small to admit of satisfactory or complete results, although I had employed for their preparation above 300 pounds of bone-oil. I found it necessary, therefore, to begin *ab initio* with the preparation of the bases from another equally large quantity of the oil; and after going through the whole of the tedious processes described in my previous paper, with the expenditure of the labour of some months, I found my object again defeated by deficiency of material. After various experiments, which, though they led to no definite or conclusive results, served to familiarize me with the nature and relations of the products obtained, I made up my mind once more to begin again; and being resolved on this occasion not to be foiled in the same way as before, I used for my new preparation no less than 250 gallons of crude bone-oil, the weight of which was somewhat above a ton. The result of this process, though involving an immense amount of labour, has been satisfactory, not only in supplying me with a large amount of material, but has also enabled me to obtain many substances, some of them possessed of very remarkable properties, which had escaped my observation when operating on a smaller scale.

The employment of so large quantity of material has, as might be expected, led to some modification of the process described in the first part of this paper,

which, though convenient enough on the small scale, was too tedious for the large quantities on which I now operated. The preliminary process of rectifying the oil, which was quite beyond the resources of a laboratory, was effected at a manufactory. The whole oil was introduced at once into a cast-iron retort, furnished with a good condenser, kept cool by an abundant current of ice-cold water. A very gentle heat was applied, and the first twenty gallons which passed over were collected apart; they consisted of about equal bulks of a highly volatile oil, and of water charged with sulphide of ammonium, hydrocyanate and carbonate of ammonia, and a small quantity of very volatile bases. The oil which distilled over after this fraction had been separated was collected in a succession of casks, which were numbered as they were filled.

In the after treatment of the oil, a process was employed similar to that which I had formerly made use of, with this exception, that the watery fluid, which had formerly been rejected, was employed for obtaining any bases which might have been dissolved in it along with the ammonia. For this purpose it was separated from the oil, and dilute sulphuric acid gradually added, when carbonic, hydrocyanic, and hydrosulphuric acids escaped with violent effervescence. When acid enough had been added to communicate a powerfully acid reaction to the fluid, it was put into a large copper boiler and boiled for some time, water being added at intervals, so as to keep up the bulk. After the ebullition had been sufficiently prolonged, the fluid was allowed to cool, and slaked lime added in excess. A copper head was then fitted to the boiler and luted down with clay, a condenser attached, and heat applied. The distillate was collected in a large glass receiver, which, in order to prevent the escape of ammonia and any very volatile products which might be carried along with it, was connected by a doubly-bent tube with a second receiver containing water, through which the gaseous products were allowed to stream. The fluid which distilled was coloured blue by the solution of small quantities of copper from the condenser; it had a powerfully ammoniacal and putrid odour, and when treated with sticks of caustic potass, in the manner described in the first part of this paper, ammonia was rapidly evolved with effervescence, and a small quantity of very volatile and pungent bases collected on the surface of the potash. These bases were separated from the potash fluid, which was preserved along with the ammoniacal solution obtained by the absorption of the gaseous products in the second receiver.

The treatment of the oil was conducted in a manner very similar to that already described, and as I desired to have only the more volatile products, I employed the first half of the oil only. It was agitated with dilute sulphuric acid in casks about half full, and after two or three days, during which the agitation was frequently repeated, more water was added, and the solution of the bases separated from the oil. To this fluid acid was added, so as to have a distinct excess; and it was then boiled for the separation of Runge's pyrrol, to which reference

has been made in the first part of this paper. As, however, I observed that a very powerful and pungent odour was evolved when the fluid began to boil, and the vapours presented the characteristic reaction of pyrrol in a very high degree, the head of the boiler was luted on, and the condenser attached, for the purpose of endeavouring to obtain that substance, which in my previous experiments I had not done. The fluid which distilled over carried with it a small quantity of oil, which, at the moment of distillation, was perfectly colourless, but soon acquired a reddish shade, and in the course of a few days became almost black. The greater part of this oil passed over with the first portion of water; but the last traces adhered with great obstinacy to the acid fluid, and could only be separated by very protracted distillation. The substance thus obtained proved to be a mixture of an oil insoluble in acids, and which appeared to be merely a small quantity of the crude oil, mechanically mixed with the fluid, and of a series of bases of very remarkable properties, and obviously related to one another, to which I shall afterwards refer under the provisional name of *pyrrol bases*.

When these substances had entirely distilled, the fluid was allowed to cool, excess of slaked lime added, and the distillation again commenced, in order to obtain the bases which had been retained by the sulphuric acid. The separation of these was conducted in a manner in all respects similar to that employed in the former preparations, solid caustic potash being added in sufficient quantity to cause the separation of the bases held in solution in the water. The potash fluid, however, retained a certain proportion of ammonia, another gaseous base, and of the most volatile bases, which could be separated only by a very large excess of potash. The fluid was therefore distilled in glass vessels, and the product collected in a succession of three receivers, the first of which was kept cold by water, the second by a freezing mixture, and the third contained hydrochloric acid, for the purpose of condensing the gaseous products. The first receiver now contained the bases dissolved in a small quantity of water, from which they were readily separated by potash; the second receiver contained only a drop or two of liquid; but in the third the hydrochloric acid was rapidly saturated, and required repeated renewal during the progress of the distillation.

The hydrochloric solution thus obtained contained a very large quantity of chloride of ammonium, along with a small proportion of another base, in order to obtain which the fluid was slowly evaporated, allowed to cool at intervals, and the sal-ammoniac which deposited was separated by straining through cloth and expression. After the separation of several crops of crystals, a dark-brown mother liquor was left, which refused to crystallize by evaporation on the water-bath, but on cooling solidified into a mass of long foliated crystals, which soon deliquesced in moist air. These crystals still contained traces of sal-ammoniac, for the separation of which they were evaporated to complete dryness on the water-bath, and dissolved in the smallest possible quantity of absolute alcohol,

with the aid of heat. The filtered fluid, on cooling, deposited a few tabular crystals mixed with a little sal-ammoniac, which was got rid of by a second filtration; and the filtrate, when treated with animal charcoal and further concentrated, solidified, on cooling, into a mass of large foliated crystals.

These crystals are long, transparent, and colourless plates, entirely without odour, and with a pungent and bitter taste. In moist air they deliquesce rapidly. Solid potash added to their concentrated solution causes the immediate escape of a gaseous base resembling ammonia, but distinguished by its peculiar putrid odour. This gas dissolves readily in water, and gives a powerfully alkaline solution. It gives with corrosive sublimate a fine white precipitate, soluble in hot water or spirit, and deposited on cooling in fine silvery plates; and its hydrochlorate gives, with bichloride of platinum, a soluble salt, depositing from its hot saturated solutions in beautiful golden-yellow scales. I selected this salt as a means of determining the constitution of its base.

I. { 6·885 grains of the platinochloride, dried at 212°, gave
1·243 ... of carbonic acid, and
1·648 ... of water.

II. { 6·189 grains of the salt gave
2·565 ... of platinum.

III. { 11·531 grains of another preparation gave
4·764 ... of platinum.

	Experiment.		Calculation.		
Carbon, . .	4·92	...	5·06	C_2	12
Hydrogen, . .	2·67	...	2·52	H_6	6
Nitrogen, . .	...	...	5·92	N	14
Chlorine, . .	...	...	44·89	Cl_3	106·5
Platinum, . .	41·31	41·44	41·61	Pt	98·7
			100·00		237·2

These analyses, then, correspond exactly with the formula $C_2 H_5 N HCl Pt Cl_2$; and the base is consequently methylamine, with which it and its salts agree in all respects.

The oily bases which had been separated from their solution in water by means of potass, were dried by the addition of successive portions of that substance, as long as it continued to become moist. The dry oil, which was very dark coloured, was then introduced into a large retort, furnished with a thermometer and a tubulated receiver kept cold by ice, and connected first with a U tube immersed in a freezing mixture, and then with a large vessel of water, in order to collect the gaseous bases which began to escape with effervescence almost as soon as heat had been applied. At a temperature under 150° Fahr. drops began to condense in the neck of the retort, and the fluid entered into rapid ebullition. At 212° the receiver was changed, and the oil distilling above that temperature was collected in receivers, which were changed at every ten degrees.

The quantity of bases which distilled under 212° was much less than I had anti-

cipated, and proportionably much smaller than that obtained when operating on a much smaller scale before; and I consequently found myself compelled to proceed very carefully, so as to avoid loss in the purification. By distilling the product which boiled under 212°, I collected fractions nearly equal in bulk at every five degrees, all very similar in their general properties. They were all limpid and colourless fluids, with high refractive power, and pungent odour, remarkably similar to that of ammonia in the lower fractions. They fumed strongly when a rod moistened with hydrochloric acid was brought near them, and presented all the properties of powerful bases. Exposed in the anhydrous state to a mixture of snow and salt, they remain perfectly fluid, but if a small quantity of water be added, beautiful white crystals of a hydrate are deposited. I attempted, by several successive distillations, to obtain fixed boiling points; but the quantity I had to work with was too small for an operation involving so much loss of material, and I therefore converted portions of the fractions which I had reason to suspect corresponded with particular bases into platinum salts. I selected, in the first place, the lowest fraction of all, that, namely, which boiled under 150°. It was dissolved in water, saturated with hydrochloric acid, and evaporated to dryness on the water-bath. The highly crystalline residue obtained was dissolved in water, and mixed with a solution of bichloride of platinum, when a yellow crystalline salt was slowly deposited, which dissolved readily in water even in the cold, and still more abundantly on boiling; and the solution on cooling deposited fine golden scales, scarcely to be distinguished in their appearance from those of methylamine or of petinine. These crystals were separated, and as the salt was highly soluble, and much remained in the mother liquor, a mixture of alcohol and ether was added, when the fluid rapidly filled with small shining scales. The analysis of this salt dried at 212° gave the following results:—

6·970 grains of platinochloride gave
3·392 ... of carbonic acid, and
2·434 ... of water.

6·475 grains of the salt gave 2·422 grains platinum.
8·257 ... ... 3·047 ...

	Experiment.		Calculation.		
Carbon, . .	13·27	...	13·57	C_6	36
Hydrogen, . .	3·88	...	3·77	H_{10}	10
Nitrogen, . .	...	...	5·27	N	14
Chlorine, . .	...	...	40·18	Cl_3	106·5
Platinum, . .	37·56	...	37·21	Pt	98·7
			100·00		265·2

From these results we arrive at the formula $C_6H_9N\,HCl\,Pt\,Cl_2$, which is that of the platinum salt of a base C_6H_9N. The base is therefore the substance I have

before described* as a product of the action of alkalies upon codeine, under the name of Metacetamine, but which I now prefer calling Propylamine, in accordance with the name now usually applied to the acid with which it corresponds. Unfortunately the quantity of propylamine obtained was too small to admit of my examining either its compounds or itself with accuracy. It is, however, a perfectly limpid and colourless fluid, with a strong pungent odour resembling that of petinine, but more ammoniacal. It gives an abundant white cloud when a rod dipped in hydrochloric acid is brought near it, and unites with the concentrated acids, with the evolution of much heat. Its hydrochlorate crystallizes in large plates closely similar to those of methylamine and petinine.

The discovery of methylamine and propylamine among these products naturally directed my attention to the probable presence of ethylamine, the intermediate term of the same series; but as I had not employed any very particular precautions in condensing the more volatile products during the successive rectifications to which I had subjected the crude oil, almost the whole of it appears to have escaped. By collecting, however, the first few drops passing over in the rectification of the portion boiling under 150° in hydrochloric acid, and forming a platinum salt, I obtained the following result:—

6·930 grains of platinochloride gave 2·649 grains platinum.

This corresponds to 38·22 per cent. Now the per-centage of platinum in the ethylamine salt is 39·60, and the result obtained, which is much too high for the propylamine salt, shows that I must have had a mixture of the two, which might have been separated had I possessed a sufficient quantity of the salt. It will readily be understood that a result of this kind could not in general be produced as evidence of the existence of ethylamine, but under the particular circumstances of the case, the next term of the same series on either side of it having been detected, it may be considered as sufficiently conclusive of its presence.

The occurrence of these bases enables us to establish, on satisfactory grounds, the constitution of petinine. In the first part of this paper, an analysis of that base is given, which agrees in the most perfect manner with the formula $C_8 H_{10} N$, which was also confirmed by that of its platinum salt. It cannot, however, for a moment be doubted that it is homologous with the bases with which I have now shewn it to be associated, that its true formula is $C_8 H_{11} N$, and that it is really butylamine, the corresponding base of the butyric group. The analysis of the platinum salt given in my former paper agrees equally well with this formula, and though that of the base differs from it to some extent, much less reliance is to be placed upon it, as it is scarcely possible, when operating upon so small a scale as that upon which I was compelled to work, to subject the bases to a sufficient number of distillations to effect their complete separation.

* Edinburgh Philosophical Transactions, vol. xx., p. 82.

I have thus then established the existence, among the products of destructive distillation, of ammonia, and the first four members of the series of bases homologous with it. I have every reason, however, to believe that the series does not end with petinine, for the fraction boiling about 200° yields a platinum salt in fine scales, and having all the characters of the salts of the same series of bases, and in all probability contains valeramine. I am not without hope also of obtaining caprylamine; but this I expect will be the last of the series, for when we reach the temperature of about 240°, the character of the bases changes, and we enter upon an entirely different series.

In the separation of the bases boiling above 240°, I have encountered very great difficulties. After the trial of many different processes, such as converting them into salts, exposing them to cold, partial saturation, and every other plan which appeared likely to answer, I have been compelled to return to fractionated distillation, as the method most likely to answer the end I had in view. But even with this process the difficulties are great, and I have been by no means so successful in obtaining fixed boiling points as I was when operating on a smaller scale in my former preparations. I subjected the whole of the oils boiling above 212° to a systematic course of fractionation, each fraction being distilled alone, and the product collected in a fresh series of bottles, and the receivers changed at every ten degrees. In the earlier rectifications each fraction spread itself over a very large number of degrees, and shewed little tendency towards concentration to fixed points. The distillations were repeated no less than fourteen times, but even after all this the indications of boiling points were extremely indistinct. Sometimes in one distillation certain fractions appeared larger than others, but their pre-eminence disappeared again in succeeding rectifications. Still a certain improvement was manifest, some of the fractions being confined more nearly to the range of degrees within which they had boiled at the previous rectification. It was obvious, from the whole phenomena of the distillation, that the separation of the different bases was going on, although with extreme slowness; and at this point I endeavoured, by the examination of the platinum salts obtained at different temperatures, to determine the constitution of the bases which these fractions contained; and as I knew from previous experiment, that the quantity boiling between 270° and 280° consisted of picoline, I had from this fact indications of the temperatures at which bases were likely to be found, and I have thus been enabled to determine the existence of two substances belonging to the same homologous series with that substance.

Pyridine.

The first of these bases, to which I give the name of pyridine, occurs in the fraction boiling about 240°. This fraction has an odour precisely similar to that of picoline, but more powerful and pungent. It is perfectly transparent and colour-

less, and does not become coloured by exposure to the air. It dissolves in water in all proportions, and is also readily soluble both in the fixed and volatile oils. It dissolves in the concentrated acids, with the evolution of much heat, and the formation of highly soluble salts. When bichloride of platinum is added to a solution of its hydrochlorate, a double salt is slowly deposited in flattened prisms, which are tolerably soluble in boiling water, less so in alcohol, and entirely insoluble in ether. When these crystals are boiled for a considerable time in water, they appear to undergo decomposition, with the formation of a platinum salt, crystallizing in golden scales. Two analyses of this salt were made, one upon the substance simply precipitated from the hydrochlorate; the other was the same salt redissolved in hot water, so as to leave a considerable proportion undissolved. In the last analysis the salt was mixed with the chromate of lead when still rather hot, and it immediately evolved a strong smell of the base, which accounts for the loss of carbon obtained in the experiment.

I. { 8·234 grains of the platinochloride gave
6·486 ... of carbonic acid, and
1·705 ... of water.

II. { 5·396 grains of the platinochloride gave
4·015 ... of carbonic acid, and
1·091 ... of water.

8·138 grains platinochloride gave 2·792 grains platinum.
4·956 ... ... 1·703 ...

	Experiment.		Calculation.		
Carbon, . .	21·48	20·29	21·03	C_{10}	60
Hydrogen, . .	2·30	2·24	2·10	H_6	6
Nitrogen, . .	...	...	4·93	N	14
Chlorine, . .	...	...	37·34	Cl_3	106·5
Platinum, . .	34·30	34·56	34·60	Pt	98·7
			100·00		285·2

The formula C_{10} H_5 N, HCl, Pt Cl_2 agrees very closely with these analyses; and the salt is consequently that of a base having the formula C_{10} H_5 N, which forms a term of the picoline series. I have not as yet directed further attention to this base, as the phenomena observed in the examination of the next base served to shew that, notwithstanding the correspondence of the salt with theory, much difficulty would be experienced in obtaining the base itself in a state of purity.

Lutidine.

In the fraction boiling about 310°, a base occurs which possesses precisely the constitution of toluidine, and to which I give the name of lutidine. When in the distillation of the mixed bases the temperature rises to about 305° to 310°, more distinct indications of a fixed boiling point are obtained than at any other temperature, and the base which distils presents sufficiently distinct characters from those obtained at lower points. The product is now much less soluble in water;

when dropped into a small quantity of that fluid it floats on the surface, and is only slowly dissolved on agitation. It possesses the remarkable property of immediately separating from its solution on the application of a gentle heat, and collecting on the surface in the form of an oily layer which dissolves again as the temperature falls. Its smell is less pungent and more aromatic than that of picoline, and it is also more oily in its characters. It unites with the acids and forms salts, all of which are highly soluble.

Analyses were made of the different portions of oil boiling about the temperature of 310°, with the following results :—

I. { 3·840 grains of the base, boiling between 310° and 315°, gave
11·007 ... of carbonic acid, and
3·060 ... of water.

II. { 4·012 grains of the base, boiling between 315° and 320°, gave
11·516 ... of carbonic acid, and
3·160 ... of water.

III. { 4·319 grains of the base, boiling between 316° and 320°, gave
12·430 ... of carbonic acid, and
3·576 ... of water.

IV. { 4·430 grains of the base, boiling between 320° and 324°, gave
12·812 ... of carbonic acid, and
3·405 ... water.

	I.	II.	III.	IV.
Carbon, .	78·17	78·28	78·48	78·87
Hydrogen, .	8·85	8·75	9·10	8·54
Nitrogen, .	12·98	12·97	12·42	12·59
	100·00	100·00	100·00	100·00

These results agree very closely with the formula $C_{14} H_9 N$, as is shewn by the following comparison of the mean experimental and calculated numbers.

	Mean.	Calculation.		
Carbon, . .	78·45	78·50	C_{14}	84
Hydrogen, . .	8·81	8·41	H_9	9
Nitrogen, . .	12·54	13·09	N	14
	100·00	100·00		107

Notwithstanding the close correspondence of these results, however, further experiment shewed that some of the fractions, especially those of lower boiling points, contained appreciable quantities of picoline, the presence of which was established by the analysis of the platinum salts. When, for instance, a portion of any of these fractions was saturated with dilute hydrochloric acid and bichloride of platinum added, fine prismatic crystals were slowly deposited, which, as the result of numerous experiments, were found to contain about 32·8 per cent. of platinum, which is exactly the quantity present in the picoline salt, of which the theoretical per-centage is 32·92. On evaporation of the mother liquor, crystals were deposited which gave quantities of platinum varying from 32·5 to 32·0 per

cent., and which were obviously mixtures of the picoline and lutidine salts. When the last mother liquor, however, was evaporated to a small bulk, and alcohol and ether added, another salt altogether distinct from that of picoline, and crystallizing in flattened tables, was deposited, which analysis proved to have the constitution of the lutidine salt.

This platinum salt crystallizes from its solutions in square tables, sometimes very distinct, at other times confused and irregular. It dissolves very readily in cold water, and still more abundantly in boiling, and appears also to be very easily soluble in excess of hydrochloric acid. Numerous analyses of this salt were made, of which the following are the results:—

No. 1. This was the analysis of the salt prepared from the oil distilling between 315° and 325° in the seventh rectification.

6·377 grains of the platinochloride gave
6·187 ... of carbonic acid, and
1·915 ... of water.

6·810 grains platinochloride gave 2·146 grains platinum.
6·476 ... ... 2·051 ...

No. 2. Portion of the oil distilling between 295° and 300° in the fourteenth rectification; the platinum salt of picoline was separated by crystallization, and the salt analysed precipitated by alcohol and ether.

7·906 grains gave 2·491 grains platinum.
7·835 ... of the salt recrystallized gave
2·470 ... of platinum.

No. 3. Another preparation from the same portion of oil.

7·330 grains of platinochloride gave
7·070 ... of carbonic acid, and
2·090 ... of water.

6·830 grains gave 2·155 grains platinum.

No. 4. Portion of the oil boiling between 300° and 305° in the thirteenth rectification.

7·401 grains gave 2·328 grains platinum.

No. 5. Portion boiling between 325° and 335° in the seventh rectification.

7·194 grains gave 2·256 grains platinum.

	I.		II.		III.	IV.	V.
Carbon,	26·41	...	...	...	26·30	...	...
Hydrogen,	3·33	...	...	...	3·16	...	...
Platinum,	31·51	31·67	31·50	31·52	31·55	31·45	31·35

These results correspond very closely with the formula C_{14} H_9 N HCl Pt Cl_2, of which the following is the calculated result compared with the mean of experiment.

	Mean.	Calculation.		
Carbon, . .	26·35	26·81	C_{14}	84
Hydrogen, .	3·23	3·19	H_{10}	10
Nitrogen, . .	...	4·49	N	14
Chlorine, . .	...	34·00	Cl_3	106·5
Platinum, . .	31·50	31·51	Pt	98·7
		100·00		313·2

It is clear, from these analyses, that the salt obtained is that of the base of which the analysis is given above; but it is equally evident, from the presence of small quantities of picoline, that the base itself was not obtained in a state of absolute purity, notwithstanding the close approximation of the experimental results with those required by theory. I have been struck throughout the whole course of the investigation by the fact, that when the fraction corresponding to the boiling point of any particular base has been analysed, results very nearly correct were obtained, even when the substance was very far from being pure. I found, for instance, in the earlier part of the investigation, that the fraction boiling between 270° and 280°, after one or two rectifications, gives precisely the results obtained from pure picoline, although on further rectification the fluid will begin to boil about 250°, and a small portion will still remain in the retort when the thermometer has risen to 300°. It is, however, readily intelligible, that this should be the case when we have to deal with a series of homologous bases, in which the per-centage of carbon goes on increasing as the boiling point rises, so that, as in this particular case, we have the excess of carbon in the less volatile base exactly counterbalancing the deficiency in the more volatile. Thus lutidine, containing 78·5 per cent. of carbon, and pyridine only 75·9, and each successive rectification removing equal quantities of the more and less volatile substances of which the boiling points are equidistant from that of the intermediate member of the series, must always leave a substance in which the quantities of the two impurities must be exactly sufficient to counterbalance the error which each will occasion.

Hydrargo-chloride of Lutidine.—I directed my attention to this compound, which is sparingly soluble and crystallizable, in hopes that it might be adapted to the purification of the base itself. I soon, however, abandoned it, as it turned out that it was not possible, in repeating its preparation, to obtain invariably the same substance, each base appearing, like aniline, to form different compounds with corrosive sublimate. When a solution of corrosive sublimate in alcohol is added to an alcoholic solution of lutidine, a curdy white precipitate falls immediately, unless the solutions be highly dilute, in which case it is slowly deposited in groups of radiated crystals. This salt dissolves in boiling water, with partial decomposition; it is still more soluble in spirit, and is deposited unchanged as the solution cools. The following analysis corresponds exactly with the formula $2\,Hg\,Cl + C_{14}\,H_9\,N$.

7·850 grains dried *in vacuo* gave
6·373 ... of carbonic acid, and
1·905 ... of water.

3·112 grains gave 2·32 grains of chloride of silver.
7·684 ... gave 4·090 grains mercury.

	Experiment.	Calculation.		
Carbon, . .	22·14	22·05	C_{14}	84
Hydrogen, . .	2·69	2·36	H_9	9
Nitrogen, . .	...	3·69	N	14
Chlorine, . .	18·43	18·64	Cl_2	71
Mercury, . .	53·22	53·26	Hg_2	202
		100·00		380

On another occasion results were obtained more nearly corresponding with the formula $3\,HgCl + C_{14}H_9N$; and intermediate results were also obtained, but as the existence of these different compounds appeared to me to be fatal to their employment as a means of purifying the base, I did not attempt to pursue the subject further. The separation of lutidine from the other bases was also attempted by forming other salts, but none were found to answer, all being highly soluble except the carbazotate, which crystallizes in beautiful, long, yellow needles, a property which, however, is unfortunately possessed by the carbazotates of all the other bases.

From all these experiments, it appears that I have been able to substantiate the existence of two bases, pyridine and lutidine, although it has been as yet impossible to obtain the bases themselves in a state of satisfactory purity. I am inclined, however, to think that the platinum salts, from their greater stability, and the ease and regularity with which they crystallize, will afford means of purification, but I have been hitherto deterred from trying this method on the large scale by the enormous quantity of platinum which would be requisite for the purpose.

It appears, then, that DIPPEL's oil contains two series of bases, one that is homologous with ammonia, the other a series peculiar to that oil, homologous with one another, and remarkable for their isomerism with the series of which aniline is the type. Thus we have—

Pyridine, . . .	$C_{10}H_5N$	
Picoline, . . .	$C_{12}H_7N$ . . .	Aniline.
Lutidine, . . .	$C_{14}H_9N$ . . .	Toluidine.

And it is probable that the series existing in DIPPEL's oil does not cease here, as I have found that the bases, with higher boiling points, give a steadily decreasing per-centage of platinum. It is impossible, in the present state of the investigation, to give any opinion as to the intimate constitution and relations of these two groups of what I may call isohomologous bases. The most obvious explanation, however, would be to suppose the new bases to be imidogen or nitrile bases,

which would enable us to understand why they differ from the aniline series, which we know to be amidogen bases. If, however, they belong to either of these classes, they must differ remarkably from any of those hitherto examined, all already formed being extremely unstable, and decomposed even by very feeble affinities, while picoline and its congeners are extremely stable, and resist even the action of nitric acid. Into these points, however, I shall not now enter, but reserve their discussion for a future part of this paper.

Pyrrol Bases.

I have already referred, at the commencement of this paper, to another series of bases, to which I have given the provisional name of pyrrol bases, and which distil away from the acid fluid by which the others are retained. They are obtained in the form of an oil, which is transparent and colourless at the moment of distillation, but rapidly acquires first a rose, then a reddish-brown, and finally an almost black colour, and the mixture gives, with hydrochloric acid and a piece of fir wood, the purple-red colour which RUNGE describes as characteristic of pyrrol. In fact, I imagined that I had at length obtained this substance, which had escaped me in my previous experiments, but I soon found that the product was really a mixture of several different bases. When distilled with the thermometer it began to boil at about 212°, and the temperature gradually rose to above 370°, and during the whole of the distillation pretty large fractions were obtained at every ten degrees, but those between 280° and 310° were decidedly larger than the others. These oils were all bases, with a peculiar and disgusting odour, quite different from, and much more disagreeable than, that of the picoline series of bases. They all acquire colour on standing, although more slowly than the crude oil. These substances dissolve easily in a small quantity of hydrochloric acid, and give, with bichloride of platinum, a precipitate which is at first yellow, but is rapidly converted into a black substance. When dissolved in an excess of acid, and heated along with it, they present a very remarkable character; the solution at a certain temperature becomes filled with red flocks, so abundant and bulky, that, if not too dilute, the fluid becomes perfectly solid, and the vessel can be inverted without anything escaping. The same change takes place, though more slowly, in the cold, and the substance deposited is then of a pale orange-colour, but becomes darker by boiling or exposure to the air. When this substance is collected on a filter, washed, and dried, it forms a reddish-brown and very light and porous mass. It is insoluble in water, acids, and alkalies, but soluble in alcohol, and the solution on evaporation leaves a dark resinous mass. When subjected to dry distillation, it leaves a bulky charcoal, while an exceedingly disgusting oil distils.

The acid fluid which has been separated from this substance by filtration, when supersaturated by an alkali, evolves the odour of the bases of the *picoline*

series. These pyrrol bases I conceive, therefore, to be substances formed by the coupling of the picoline series with some substance which yields the red matter to which I have alluded. I have not as yet, however, pursued the investigation of these bases, but shall communicate the result of their examination in a future paper.

The Non-basic Constituents of Bone Oil.

I have as yet directed very little attention to this branch of the subject. I have found, however, that when the most volatile part of the oil, after separation of the bases, is repeatedly rectified, it improves in odour, and at length there is obtained a substance which, when acted upon by nitric acid, and then by sulphide of ammonium, gives the reaction of aniline,—indicative of the presence of benzine in the oil. It is probable, therefore, that this series of homologous carbohydrogens forms a part of the oil, but not the whole of it, for I have found that when the oil is boiled for some time with potass, ammonia is evolved, and on supersaturating the potash solution with sulphuric acid, the odour of butyric acid, or at all events of one of the fatty acids, becomes apparent; from which phenomena I draw the conclusion that it also contains the nitriles of these acids.

XV.—*On the Dynamical Theory of Heat, with numerical results deduced from* Mr Joule's *equivalent of a Thermal Unit, and* M. Regnault's *Observations on Steam.* By William Thomson, M.A., Fellow of St Peter's College, Cambridge, and Professor of Natural Philosophy in the University of Glasgow.

(Read 17th March 1851.)

INTRODUCTORY NOTICE.

1. Sir Humphrey Davy, by his experiment of melting two pieces of ice by rubbing them together, established the following proposition:—"The phenomena of repulsion are not dependent on a peculiar elastic fluid for their existence, or caloric does not exist." And he concludes that heat consists of a motion excited among the particles of bodies. "To distinguish this motion from others, and to signify the cause of our sensation of heat," and of the expansion or expansive pressure produced in matter by heat, "the name *repulsive* motion has been adopted."*

2. The Dynamical Theory of Heat, thus established by Sir Humphrey Davy, is extended to radiant heat by the discovery of phenomena, especially those of the polarization of radiant heat, which render it excessively probable that heat propagated through vacant space, or through diathermane substances, consists of waves of transverse vibrations in an all-pervading medium.

3. The recent discoveries made by Mayer and Joule,† of the generation of heat through the friction of fluids in motion, and by the magneto-electric excitation of galvanic currents, would, either of them be sufficient to demonstrate the immateriality of heat; and would so afford, if required, a perfect confirmation of Sir Humphrey Davy's views.

* From Davy's first work, entitled "An Essay on Heat, Light, and the Combinations of Light," published in 1799, in "Contributions to Physical and Medical Knowledge, principally from the West of England, collected by Thomas Beddoes, M.D.," and republished in Dr Davy's edition of his brother's collected works, vol. ii. Lond. 1836.

† In May 1842, Mayer announced in the "Annalen" of Wöhler and Liebig, that he had raised the temperature of water from 12° to 13° cent. by agitating it. In August 1843, Joule announced to the British Association, "That heat is evolved by the passage of water through narrow tubes;" and that he had "obtained one degree of heat per lb. of water from a mechanical force capable of raising 770 lbs. to the height of one foot;" and that heat is generated when work is spent in turning a magneto-electric machine, or an electro-magnetic engine. (See his paper "On the Calorific Effects of Magneto-Electricity, and on the Mechanical Value of Heat." Phil. Mag. vol. xxiii. 1843.)

4. Considering it as thus established, that heat is not a substance, but a dynamical form of mechanical effect, we perceive that there must be an equivalence between mechanical work and heat, as between cause and effect. The first published statement of this principle appears to be in MAYER'S "Bemerkungen über die Kräfte der unbelebten Natur," * which contains some correct views regarding the mutual convertibility of heat and mechanical effect, along with a false analogy between the approach of a weight to the earth and a diminution of the volume of a continuous substance, on which an attempt is founded to find numerically the mechanical equivalent of a given quantity of heat. In a paper published about fourteen months later, "On the Calorific Effects of Magneto-Electricity and the Mechanical Value of Heat," † Mr JOULE of Manchester expresses very distinctly the consequences regarding the mutual convertibility of heat and mechanical effect which follow from the fact, that heat is not a substance but a state of motion; and investigates on unquestionable principles the "absolute numerical relations," according to which heat is connected with mechanical power; verifying experimentally, that whenever heat is generated from purely mechanical action, and no other effect produced, whether it be by means of the friction of fluids or by the magneto-electric excitation of galvanic currents, the same quantity is generated by the same amount of work spent, and determining the actual amount of work, in foot-pounds, required to generate a unit of heat, which he calls "the mechanical equivalent of heat." Since the publication of that paper, Mr JOULE has made numerous series of experiments for determining with as much accuracy as possible the mechanical equivalent of heat so defined, and has given accounts of them in various communications to the British Association, to the Philosophical Magazine, to the Royal Society, and to the French Institute.

5. Important contributions to the Dynamical Theory of Heat have recently been made by RANKINE and CLAUSIUS; who, by mathematical reasoning analogous to CARNOT'S on the motive power of heat, but founded on an axiom contrary to his fundamental axiom, have arrived at some remarkable conclusions. The researches of these authors have been published in the Transactions of this Society, and in POGGENDORFF'S Annalen, during the past year; and they are more particularly referred to below in connection with corresponding parts of the investigations at present laid before the Royal Society.

6. The object of the present paper is threefold,—

(1.) To shew what modifications of the conclusions arrived at by CARNOT, and by others who have followed his peculiar mode of reasoning regarding the motive

* "Annalen" of WÖHLER and LIEBIG, May 1842.

† British Association, August 1843, and Philosophical Magazine, September 1843.

power of heat, must be made when the hypothesis of the Dynamical Theory, contrary as it is to CARNOT's fundamental hypothesis, is adopted.

(2.) To point out the significance in the Dynamical Theory of the numerical results deduced from REGNAULT's Observations on Steam, and communicated about two years ago to the Society, with an account of CARNOT's Theory, by the author of the present paper; and to shew that by taking these numbers (subject to correction when accurate experimental data regarding the density of saturated steam shall have been afforded), in connection with JOULE's mechanical equivalent of a thermal unit, a complete theory of the motive power of heat, within the temperature limits of the experimental data, is obtained.

(3.) To point out some remarkable relations connecting the physical properties of all substances, established by reasoning analogous to that of CARNOT, but founded in part on the contrary principle of the Dynamical Theory.

PART I.—FUNDAMENTAL PRINCIPLES IN THE THEORY OF THE MOTIVE POWER OF HEAT.

7. According to an obvious principle, first introduced, however, into the theory of the motive power of heat by CARNOT, mechanical effect produced in any process cannot be said to have been derived from a purely thermal source, unless at the end of the process all the materials used are in precisely the same physical and mechanical circumstances as they were at the beginning. In some conceivable "thermo-dynamic engines," as for instance FARADAY's floating magnet, or BARLOW's "wheel and axle," made to rotate and perform work uniformly by means of a current continuously excited by heat communicated to two metals in contact, or the thermo-electric rotatory apparatus devised by MARSH, which has been actually constructed; this condition is fulfilled at every instant. On the other hand, in all thermo-dynamic engines, founded on electrical agency, in which discontinuous galvanic currents, or pieces of soft iron in a variable state of magnetization, are used; and in all engines founded on the alternate expansions and contractions of media; there are really alterations in the condition of materials; but, in accordance with the principle stated above, these alterations must be strictly periodical. In any such engine, the series of motions performed during a period, at the end of which the materials are restored to precisely the same condition as that in which they existed at the beginning, constitutes what will be called a complete cycle of its operations. Whenever in what follows, *the work done*, or *the mechanical effect produced*, by a thermo-dynamic engine is mentioned without qualification, it must be understood that the mechanical effect produced, either in a non-varying engine, or in a complete cycle or any number of complete cycles of a periodical engine is meant.

8. The *source of heat* will always be supposed to be a hot body at a given constant temperature, put in contact with some part of the engine; and when any part of the engine is to be kept from rising in temperature (which can only be done by drawing off whatever heat is deposited in it), this will be supposed to be done by putting a cold body, which will be called the refrigerator, at a given constant temperature, in contact with it.

9. The whole theory of the motive power of heat is founded on the two following propositions, due respectively to JOULE, and to CARNOT and CLAUSIUS.

Prop. I. (JOULE).—When equal quantities of mechanical effect are produced by any means whatever, from purely thermal sources, or lost in purely thermal effects, equal quantities of heat are put out of existence, or are generated.

Prop. II. (CARNOT and CLAUSIUS).—If an engine be such that, when it is worked backwards, the physical and mechanical agencies in every part of its motions are all reversed; it produces as much mechanical effect as can be produced by any thermo-dynamic engine, with the same temperatures of source and refrigerator, from a given quantity of heat.

10. The former proposition is shewn to be included in the general "principle of mechanical effect," and is so established beyond all doubt by the following demonstration.

11. By whatever direct effect the heat gained or lost by a body, in any conceivable circumstances, is tested, the measurement of its quantity may always be founded on a determination of the quantity of some standard substance, which it or any equal quantity of heat could raise from one standard temperature to another; the test of equality between two quantities of heat being their capability of raising equal quantities of any substance from any temperature to the same higher temperature. Now, according to the dynamical theory of heat, the temperature of a substance can only be raised by working upon it in some way so as to produce increased thermal motions within it, besides effecting any modifications in the mutual distances or arrangements of its particles which may accompany a change of temperature. The work necessary to produce this total mechanical effect is of course proportional to the quantity of the substance raised from one standard temperature to another; and therefore when a body, or a group of bodies, or a machine, parts with or receives heat, there is in reality mechanical effect produced from it, or taken into it, to an extent precisely proportional to the quantity of heat which it emits or absorbs. But the work which any external forces do upon it, the work done by its own molecular forces, and the amount bywhich the half *vis viva* of the thermal motions of all its parts is diminished, must together be equal to the mechanical effect produced from it; and consequently, to the mechanical equivalent of the heat which it emits (which will be positive or negative, according as the sum of those terms is positive or

negative). Now, let there be either no molecular change or alteration of temperature in any part of the body, or, by a cycle of operations, let the temperature and physical condition be restored exactly to what they were at the beginning; the second and third of the three parts of the work which it has to produce vanish; and we conclude that the heat which it emits or absorbs will be the thermal equivalent of the work done upon it by external forces, or done by it against external forces; which is the proposition to be proved.

12. The demonstration of the second proposition is founded on the following axiom:—

*It is impossible, by means of inanimate material agency, to derive mechanical effect from any portion of matter by cooling it below the temperature of the coldest of the surrounding objects.**

13. To demonstrate the second proposition, let A and B be two thermo-dynamic engines, of which B satisfies the conditions expressed in the enunciation; and let, if possible, A derive more work from a given quantity of heat than B, when their sources and refrigerators are at the same temperatures, respectively. Then, on account of the condition of complete *reversibility* in all its operations which it fulfils, B may be worked backwards, and made to restore any quantity of heat to its source, by the expenditure of the amount of work which, by its forward action, it would derive from the same quantity of heat. If, therefore, B be worked backwards, and made to restore to the source of A (which we may suppose to be adjustable to the engine B) as much heat as has been drawn from it during a certain period of the working of A, a smaller amount of work will be spent thus than was gained by the working of A. Hence, if such a series of operations of A forwards and of B backwards be continued, either alternately or simultaneously, there will result a continued production of work without any continued abstraction of heat from the source; and, by Prop. I., it follows that there must be more heat abstracted from the refrigerator by the working of B backwards than is deposited in it by A. Now, it is obvious that A might be made to spend part of its work in working B backwards, and the whole might be made self-acting. Also, there being no heat either taken from or given to the source on the whole, all the surrounding bodies and space, except the refrigerator, might, without interfering with any of the conditions which have been assumed, be made of the same temperature as the source, whatever that may be. We should thus have a self-acting machine, capable of drawing heat constantly from a body surrounded by others at a higher temperature, and converting it into mechanical effect. But this is contrary to the axiom, and, therefore, we conclude that the hypothesis that A

* If this axiom be denied for all temperatures, it would have to be admitted that a self-acting machine might be set to work and produce mechanical effect by cooling the sea or earth, with no limit but the total loss of heat from the earth and sea, or, in reality, from the whole material world.

derives more mechanical effect from the same quantity of heat, drawn from the source, than B, is false. Hence no engine whatever, with source and refrigerator at the same temperatures, can get more work from a given quantity of heat introduced than any engine which satisfies the condition of reversibility, which was to be proved.

14. This proposition was first enunciated by CARNOT, being the expression of his criterion of a perfect thermo-dynamic engine.* He proved it by demonstrating that a negation of it would require the admission that there might be a self-acting machine constructed which would produce mechanical effect indefinitely, without any source either in heat or the consumption of materials, or any other physical agency; but this demonstration involves, fundamentally, the assumption that, in "a complete cycle of operations," the medium parts with exactly the same quantity of heat as it receives. A very strong expression of doubt regarding the truth of this assumption, as a universal principle, is given by CARNOT himself;† and that it is false, where mechanical work is, on the whole, either gained or spent in the operations, may (as I have tried to shew above) be considered to be perfectly certain. It must, then, be admitted that CARNOT'S original demonstration utterly fails, but we cannot infer that the proposition concluded is false. The truth of the conclusion appeared to me, indeed, so probable, that I took it in connection with JOULE'S principle, on account of which CARNOT'S demonstration of it fails, as the foundation of an investigation of the motive power of heat in air-engines or steam-engines through finite ranges of temperature, and obtained, about a year ago, results, of which the substance is given in the second part of the paper at present communicated to the Royal Society. It was not until the commencement of the present year that I found the demonstration given above, by which the truth of the proposition is established upon an axiom (§ 12) which I think will be generally admitted. It is with no wish to claim priority that I make these statements, as the merit of first establishing the proposition upon correct principles is entirely due to CLAUSIUS, who published his demonstration of it in the month of May last year, in the second part of his paper on the Motive Power of Heat.‡ I may be allowed to add, that I have given the demonstration exactly as it occurred to me before I knew that CLAUSIUS had either enunciated or demonstrated the proposition. The following is the axiom on which CLAUSIUS' demonstration is founded:—

It is impossible for a self-acting machine, unaided by any external agency, to convey heat from one body to another at a higher temperature.

It is easily shewn that, although this and the axiom I have used are different

* "Account of CARNOT'S Theory," § 13. † Ibid., § 6.

‡ POGGENDORFF'S Annalen, referred to above.

in form, either is a consequence of the other. The reasoning in each demonstration is strictly analogous to that which CARNOT originally gave.

15. A complete theory of the motive power of heat would consist of the application of the two propositions demonstrated above, to every possible method of producing mechanical effect from thermal agency.* As yet this has not been done for the electrical method, as far as regards the criterion of a perfect engine, implied in the second proposition, and probably cannot be done without certain limitations; but the application of the first proposition has been very thoroughly investigated, and verified experimentally, by Mr JOULE, in his researches "On the Calorific Effects of Magneto-Electricity;" and on it is founded one of his ways of determining experimentally the mechanical equivalent of heat. Thus, from his discovery of the laws of generation of heat in the galvanic circuit,† it follows that, when mechanical work by means of a magneto-electric machine is the source of the galvanism, the heat generated in any given portion of the fixed part of the circuit is proportional to the whole work spent; and from his experimental demonstration that heat is developed in any moving part of the circuit at exactly the same rate as if it were at rest, and traversed by a current of the same strength, he is enabled to conclude—

(1.) That heat may be created by working a magneto-electric machine.

(2.) That if the current excited be not allowed to produce any other than thermal effects, the total quantity of heat produced is, in all circumstances, exactly proportional to the quantity of work spent.

16. Again, the admirable discovery of PELTIER, that cold is produced by an electrical current passing from bismuth to antimony, is referred to by JOULE, as shewing how it may be proved that, when an electrical current is continuously produced from a purely thermal source, the quantities of heat evolved electrically in the different homogeneous parts of the circuit are only compensations for a loss from the junctions of the different metals, or that, when the effect of the current is entirely thermal, there must be just as much heat emitted from the parts not affected by the source as is taken in from the source.

17. Lastly,‡ when a current produced by thermal agency is made to work an

* "There are [at present known] two, and only two, distinct ways in which mechanical effect can be obtained from heat. One of these is by the alterations of volume which bodies experience through the action of heat, the other is through the medium of electric agency."—*Account of* CARNOT'S *Theory*, § 4. (Transactions, Vol. XVI., Part V.) . . . . . A paper by Mr JOULE, containing demonstrations of these laws, and of others on the relations of the chemical and thermal agencies concerned, was communicated to the Royal Society on the 17th December 1840, but was not published in the Transactions. (See abstract containing a statement of the laws quoted above, in the Philosophical Magazine, vol. xviii., p. 308). It was published in the Philosophical Magazine in October 1841 (vol. xix., p. 260).

† That, in a given fixed part of the circuit, the heat evolved in a given time is proportional to the square of the strength of the current, and for different fixed parts, with the same strength of current, the quantities of heat evolved in equal times are as the resistances.

‡ This reasoning was suggested to me by the following passage contained in a letter which I

engine and produce mechanical effect, there will be less heat emitted from the parts of the circuit not affected by the source than is taken in from the source, by an amount precisely equivalent to the mechanical effect produced; since JOULE demonstrates experimentally that a current from any kind of source, driving an engine, produces in the engine just as much less heat than it would produce in a fixed wire exercising the same resistance as is equivalent to the mechanical effect produced by the engine.

18. The equality of thermal effects, resulting from equal causes through very different means, is beautifully illustrated by the following statement, drawn from Mr JOULE'S paper on magneto-electricity.

Let there be three equal and similar galvanic batteries, furnished with equal and similar electrodes: let A_1 and B_1 be the terminations of the electrodes (or wires connected with the two poles) of the first battery; A_2 and B_2 the terminations of the corresponding electrodes of the second; and A_3 and B_3 of the third battery. Let A_1 and B_1 be connected with the extremities of a long fixed wire; let A_2 and B_2 be connected with the "poles" of an electrolytic apparatus for the decomposition of water; and let A_3 and B_3 be connected with the *poles* (or *ports* as they might be called) of an electro-magnetic engine. Then if the length of the wire between A_1 and B_1, and the speed of the engine between A_3 and B_3, be so adjusted that the strength of the current (which, for simplicity, we may suppose to be continuous and perfectly uniform in each case) may be the same in the three circuits, there will be more heat given out in any time in the wire between A_1 and B_1 than in the electrolytic apparatus between A_2 and B_2, or the working engine between A_3 and B_3. But if the hydrogen were allowed to burn in the oxygen, within the electrolytic vessel, and the engine to waste all its work without producing any other than thermal effects (as it would do, for instance, if all its work were spent in continuously agitating a limited fluid mass), the total heat emitted would be precisely the same in each of these two pieces of apparatus as in the wire between

received from Mr JOULE on the 8th of July 1847. "In PELTIER'S experiment on cold produced at the bismuth and antimony solder, we have an instance of the conversion of heat into the mechanical force of the current," which must have been meant as an answer to a remark I had made, that no evidence could be adduced to shew that heat is ever put out of existence. I now fully admit the force of that answer, but it would require a proof that there is more heat put out of existence at the heated soldering than is created at the cold soldering, to make the "evidence" be *experimental*. That this is the case I think is certain, because the statements of § 16 in the text are demonstrated consequences of the first fundamental proposition; but it is still to be remarked, that neither in this nor in any other case of the production of mechanical effect from purely thermal agency, has the ceasing to exist of an equivalent quantity of heat been demonstrated otherwise than theoretically. It would be a very great step in the experimental illustration (or *verification*, for those who consider such to be necessary) of the dynamical theory of heat, to actually shew, in any one case, a loss of heat; and it might be done by operating through a very considerable range of temperatures with a good air-engine or steam-engine, not allowed to waste its work in friction. As will be seen in Part II. of this paper, no experiment of any kind could shew a considerable loss of heat without employing bodies differing considerably in temperature; for instance, a loss of as much as ·098, or about one-tenth of the whole heat used, if the temperature of all the bodies used be between 0° and 30° cent.

A_1 and B_1. It is worthy of remark that these propositions are *rigorously* true, being demonstrable consequences of the fundamental principle of the dynamical theory of heat, which have been discovered by JOULE, and illustrated and verified most copiously in his experimental researches.

19. Both the fundamental propositions may be applied in a perfectly rigorous manner to the second of the known methods of producing mechanical effect from thermal agency. This application of the first of the two fundamental propositions has already been published by RANKINE and CLAUSIUS; and that of the second, as CLAUSIUS shewed in his published paper, is simply CARNOT'S unmodified investigation of the relation between the mechanical effect produced and the thermal circumstances from which it originates, in the case of an expansive engine working within an infinitely small range of temperatures. The simplest investigation of the consequences of the first proposition in this application, which has occurred to me, is the following, being merely the modification of an analytical expression of CARNOT'S axiom regarding the permanence of heat, which was given in my former paper,* required to make it express, not CARNOT'S axiom, but JOULE'S.

20. Let us suppose a mass† of any substance, occupying a volume v, under a pressure p uniform in all directions, and at a temperature t, to expand in volume to $v + dv$, and to rise in temperature to $t + dt$. The quantity of work which it will produce will be

$$p\,dv;$$

and the quantity of heat which must be added to it to make its temperature rise during the expansion to $t + dt$ may be denoted by

$$M\,dv + N\,dt.$$

The mechanical equivalent of this is

$$J\,(M\,dv + N\,dt),$$

if J denote the mechanical equivalent of a unit of heat. Hence the mechanical measure of the total external effect produced in the circumstances is

$$(p - J\,M)\,dv - J\,N\,dt.$$

The total external effect, after any finite amount of expansion, accompanied by any continuous change of temperature, has taken place, will consequently be, in mechanical terms,

$$\int\{(p - J\,M)\,dv - J\,N\,dt\};$$

where we must suppose t to vary with v, so as to be the actual temperature of the medium at each instant, and the integration with reference to v must be performed between limits corresponding to the initial and final volumes. Now if, at any subsequent time, the volume and temperature of the medium become what

* "Account of CARNOT'S Theory," foot-note on § 26.

† This may have parts consisting of different substances, or of the same substance in different states, provided the temperature of all be the same. See below Part III., §§ 53–56.

they were at the beginning, however arbitrarily they may have been made to vary in the period, the total external effect must, according to Prop. I., amount to nothing; and hence

$$(p - \mathrm{J\,M})\,dv - \mathrm{J\,N}\,dt$$

must be the differential of a function of two independent variables, or we must have

$$\frac{d\,(p - \mathrm{J\,M})}{dt} = \frac{d\,(-\mathrm{J\,N})}{dv} \quad \ldots\ldots\ldots (1),$$

this being merely the analytical expression of the condition, that the preceding integral may vanish in every case in which the initial and final values of v and t are the same, respectively. Observing that J is an absolute constant, we may put the result into the form

$$\frac{dp}{dt} = \mathrm{J}\left(\frac{d\mathrm{M}}{dt} - \frac{d\mathrm{N}}{dv}\right) \quad \ldots\ldots\ldots (2).$$

This equation expresses, in a perfectly comprehensive manner, the application of the first fundamental proposition to the thermal and mechanical circumstances of any substance whatever, under uniform pressure in all directions, when subjected to any possible variations of temperature, volume, and pressure.

21. The corresponding application of the second fundamental proposition is completely expressed by the equation

$$\frac{dp}{dt} = \mu\,\mathrm{M} \quad \ldots\ldots\ldots (3),$$

where μ denotes what is called "CARNOT's function," a quantity which has an absolute value, the same for all substances for any given temperature, but which may vary with the temperature in a manner that can only be determined by experiment. To prove this proposition, it may be remarked in the first place that Prop. II. could not be true for every case in which the temperature of the refrigeration differs infinitely little from that of the source, without being true universally. Now, if a substance be allowed first to expand from v to $v + dv$, its temperature being kept constantly t; if, secondly, it be allowed to expand farther, without either emitting or absorbing heat till its temperature goes down through an infinitely small range, to $t - \tau$; if, thirdly, it be compressed at the constant temperature $t - \tau$, so much (actually by an amount differing from dv by only an infinitely small quantity of the second order), that when, fourthly, the volume is further diminished to v without the medium's being allowed to either emit or absorb heat, its temperature may be exactly t; it may be considered as constituting a thermo-dynamic engine which fulfils CARNOT's condition of complete reversibility. Hence, by Prop II., it must produce the same amount of work for the same quantity of heat absorbed in the first operation, as any other substance similarly operated upon through the same range of temperatures. But $\frac{dp}{dt}\tau.\,dv$ is obviously the whole work done in the complete cycle, and (by the definition of

M, in § 20) M dv is the quantity of heat absorbed in the first operation. Hence the value of

$$\frac{\frac{dp}{dt}\tau . dv}{M\, dv} \quad \text{or} \quad \frac{\frac{dp}{dt}}{M}\tau$$

must be the same for all substances, with the same values of t and τ; or, since τ is not involved except as a factor, we must have

$$\frac{\frac{dp}{dt}}{M} = \mu \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4),$$

where μ depends only on t; from which we conclude the proposition which was to be proved.

22. The very remarkable theorem that $\frac{\frac{dp}{dt}}{M}$ must be the same for all substances at the same temperature, was first given (although not in precisely the same terms) by CARNOT, and demonstrated by him, according to the principles he adopted. We have now seen that its truth may be satisfactorily established without adopting the false part of his principles. Hence all CARNOT's conclusions, and all conclusions derived by others from his theory, which depend merely on equation (3), require no modification when the dynamical theory is adopted. Thus, all the conclusions contained in Sections I., II., and III. of the Appendix to my Account of CARNOT's Theory, and in the paper immediately following it in the Transactions, entitled "Theoretical Considerations on the Effect of Pressure in Lowering the Freezing Point of Water," by my elder brother, still hold. Also, we see that CARNOT's expression for the mechanical effect derivable from a given quantity of heat by means of a perfect engine in which the range of temperatures is infinitely small, expresses truly the greatest effect which can possibly be obtained in the circumstances; although it is in reality only an infinitely small fraction of the whole mechanical equivalent of the heat supplied; the remainder being irrecoverably lost to man, and therefore "wasted," although not *annihilated.*

23. On the other hand, the expression for the mechanical effect obtainable from a given quantity of heat entering an engine from a "source" at a given temperature, when the range down to the temperature of the cold part of the engine or the "refrigerator" is finite, will differ most materially from that of CARNOT; since, a finite quantity of mechanical effect being now obtained from a finite quantity of heat entering the engine, a finite fraction of this quantity must be converted from heat into mechanical effect. The investigation of this expression, with numerical determinations founded on the numbers deduced from REGNAULT's observations on steam, which are shewn in Tables I. and II. of

my former paper, constitutes the second part of the paper at present communicated.

Part II.—On the Motive Power of Heat through Finite Ranges of Temperature.

24. It is required to determine the quantity of work which a perfect engine, supplied from a source at any temperature, S, and parting with its waste heat to a refrigerator at any lower temperature, T, will produce from a given quantity, H, of heat drawn from the source.

25. We may suppose the engine to consist of an infinite number of perfect engines, each working within an infinitely small range of temperature, and arranged in a series of which the source of the first is the given source, the refrigerator of the last the given refrigerator, and the refrigerator of each intermediate engine is the source of that which follows it in the series. Each of these engines will, in any time, emit just as much less heat to its refrigerator than is supplied to it from its source, as is the equivalent of the mechanical work which it produces. Hence, if t and $t + dt$ denote respectively the temperatures of the refrigerator and source of one of the intermediate engines; and if q denote the quantity of heat which this engine discharges into its refrigerator in any time, and $q + dq$ the quantity which it draws from its source in the same time, the quantity of work which it produces in that time will be $J\,dq$ according to Prop. I., and it will also be $q\,\mu\,dt$ according to the expression of Prop. II., investigated in § 21; and therefore we must have

$$J\,dq = q\,\mu\,dt.$$

Hence, supposing that the length of time considered is that during which the quantity, H, of heat is supplied from the first source, we find by integration

$$\log\frac{H}{q} = \frac{1}{J}\int_t^s \mu\,dt.$$

But the value of q, when $t = T$, is the final remainder discharged into the refrigerator at the temperature T; and therefore, if this be denoted by R, we have

$$\log\frac{H}{R} = \frac{1}{J}\int_T^S \mu\,dt \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5);$$

from which we deduce

$$R = H\,\epsilon^{-\frac{1}{J}\int_T^S \mu\,dt} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6).$$

Now, the whole amount of work produced will be the mechanical equivalent of the quantity of heat lost; and, therefore, if this be denoted by W, we have

$$W = J\,(H - R), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7),$$

and consequently, by (6),

$$W = J H \left\{1 - \epsilon^{-\frac{1}{J}\int_T^S \mu\, dt}\right\} \quad \ldots \ldots \ldots \quad (8).$$

26. To compare this with the expression $H\int_T^S \mu\, dt$, for the duty, indicated by CARNOT's theory,* we may expand the exponential in the preceding equation, by the usual series. We thus find

$$\left.\begin{aligned} W &= \left(1 - \frac{\theta}{1.2} + \frac{\theta^2}{1.2.3} - \&c.\right) . H\int_T^S \mu\, dt \\ \text{where} \quad \theta &= \frac{1}{J}\int_T^S \mu\, dt \end{aligned}\right\} \quad \ldots \quad (9).$$

This shews that the work really produced, which always falls short of the duty indicated by CARNOT's theory, approaches more and more nearly to it as the range is diminished, and ultimately, when the range is infinitely small, is the same as if CARNOT's theory required no modification, which agrees with the conclusion stated above in § 22.

27. Again, equation (8) shews that the real duty of a given quantity of heat supplied from the source increases with every increase of the range; but that instead of increasing indefinitely in proportion to $\int_T^S \mu\, dt$, as CARNOT's theory makes it do, it never reaches the value J H, but approximates to this limit, as $\int_T^S \mu\, dt$ is increased without limit. Hence CARNOT's remark† regarding the practical advantage that may be anticipated from the use of the air-engine, or from any method by which the range of temperatures may be increased, loses only a part of its importance, while a much more satisfactory view than his of the practical problem is afforded. Thus we see that, although the full equivalent of mechanical effect cannot be obtained even by means of a perfect engine, yet when the actual source of heat is at a high enough temperature above the surrounding objects, we may get more and more nearly the whole of the admitted heat converted into mechanical effect, by simply increasing the effective range of temperature in the engine.

28. The preceding investigation (§ 25) shews that the value of CARNOT's function, μ, for all temperatures within the range of the engine, and the absolute value of JOULE's equivalent, J, are enough of data to calculate the amount of mechanical effect of a perfect engine of any kind, whether a steam-engine, an air-engine, or even a thermo-electric engine, since, according to the axiom stated in § 12, and the demonstration of Prop. II., no inanimate material agency could

* "Account," &c., Equation 7, § 31.
† "Account," &c. Appendix, Section IV.

produce more mechanical effect from a given quantity of heat, with a given available range of temperatures, than an engine satisfying the criterion stated in the enunciation of the proposition.

29. The mechanical equivalent of a thermal unit Fahrenheit, or the quantity of heat necessary to raise the temperature of a pound of water from 32° to 33° Fahr., has been determined by JOULE in foot-pounds at Manchester, and the value which he gives as his best determination is 772·69. Mr RANKINE takes, as the result of JOULE's determination, 772, which he estimates must be within $\frac{1}{300}$ of its own amount, of the truth. If we take $772\frac{2}{9}$ as the number, we find, by multiplying it by $\frac{9}{5}$, 1390 as the equivalent of the thermal unit centigrade, which is taken as the value of J in the numerical applications contained in the present paper.

30. With regard to the determination of the values of μ for different temperatures, it is to be remarked that equation (4) shews that this might be done by experiments upon any substance whatever of indestructible texture, and indicates exactly the experimental data required in each case. For instance, by first supposing the medium to be air; and again, by supposing it to consist partly of liquid water and partly of saturated vapour, we deduce, as is shewn in Part III. of this paper, the two expressions (6), given in § 30 of my former paper, for the value of μ at any temperature. As yet no experiments have been made upon air which afford the required data for calculating the value of μ through any extensive range of temperature; but for temperatures between 50° and 60° Fahrenheit, JOULE's experiments* on the heat evolved by the expenditure of a given amount of work on the compression of air kept at a constant temperature, afford the most direct data for this object which have yet been obtained; since, if Q be the quantity of heat evolved by the compression of a fluid subject to "the gaseous laws" of expansion and compressibility, W the amount of mechanical work spent, and t the constant temperature of the fluid, we have, by (11) of § 49 of my former paper,

$$\mu = \frac{W \cdot E}{Q(1+E\,t)} \quad . \quad . \quad . \quad . \quad . \quad . \quad (10),$$

which is in reality a simple consequence of the other expression for μ in terms of data with reference to air. Remarks upon the determination of μ by such experiments, and by another class of experiments on air originated by JOULE, are reserved for a separate communication, which I hope to be able to make to the Royal Society on another occasion.

31. The second of the expressions (6), in § 30 of my former paper, or the equivalent expression (32), given below in the present paper, shews that μ may be determined for any temperature from determinations for that temperature of;

* "On the Changes of Temperature produced by the Rarefaction and Condensation of Air," Phil. Mag., vol. xxvi. May 1845.

(1.) The rate of variation with the temperature, of the pressure of saturated steam.

(2.) The latent heat of a given weight of saturated steam.

(3.) The volume of a given weight of saturated steam.

(4.) The volume of a given weight of water.

The last mentioned of these elements may, on account of the manner in which it enters the formula, be taken as constant, without producing any appreciable effect on the probable accuracy of the result.

32. REGNAULT'S observations have supplied the first of the data with very great accuracy for all temperatures between $-32°$ cent. and 230°.

33. As regards the second of the data, it must be remarked that all experimenters, from WATT, who first made experiments on the subject, to REGNAULT, whose determinations are the most accurate and extensive that have yet been made, appear to have either explicitly or tacitly assumed the same principle as that of CARNOT, which is overturned by the dynamical theory of heat; inasmuch as they have defined the "total heat of steam" as the quantity of heat required, to convert a unit of weight of water at 0°, into steam in the particular state considered. Thus REGNAULT, setting out with this definition for "the total heat of saturated steam," gives experimental determinations of it for the entire range of temperatures from 0° to 230°; and he deduces the "latent heat of saturated steam" at any temperature, from the "total heat," so determined, by subtracting from it the quantity of heat necessary to raise the liquid to that temperature. Now, according to the dynamical theory, the quantity of heat expressed by the preceding definition depends on the manner (which may be infinitely varied) in which the change of state defined is effected; differing in different cases by the thermal equivalents of the differences of the external mechanical effect produced in the expansion. For instance, the final quantity of heat required to evaporate a quantity of water at 0°, and then, keeping it always in the state of saturated vapour,* bring it to the temperature 100°, cannot be so much as three-fourths of the quantity required, first, to raise the temperature of the liquid to 100°, and then evaporate it at that temperature; and yet either quantity is expressed by what is generally received as a *definition* of the "total heat" of the saturated vapour. To find what it is that is really determined as "total heat" of saturated steam in REGNAULT'S researches, it is only necessary to

* See below (Part III. § 58), where the "negative" specific heat of saturated steam is investigated. If the mean value of this quantity between 0° and 100° were $-1{\cdot}5$ (and it cannot differ much from this) there would be 150 units of heat emitted by a pound of saturated vapour in having its temperature raised (by compression) from 0° to 100°. The latent heat of the vapour at 0° being 606·5, the final quantity of heat required to convert a pound of water at 0° into saturated steam at 100°, in the first of the ways mentioned in the text, would consequently be 456·5, which is only about $\frac{5}{7}$ of the quantity 637 found as "the total heat" of the saturated vapour at 100°, by REGNAULT.

remark, that the measurement actually made is of the quantity of heat emitted by a certain weight of water in passing through a calorimetrical apparatus, which it enters as saturated steam, and leaves in the liquid state, the result being reduced to what would have been found, if the final temperature of the water had been exactly 0°. For there being no external mechanical effect produced (other than that of sound, which it is to be presumed is quite inappreciable), the only external effect is the emission of heat. This must, therefore, according to the fundamental proposition of the dynamical theory, be independent of the intermediate agencies. It follows that, however the steam may rush through the calorimeter, and at whatever reduced pressure it may actually be condensed,* the heat emitted externally must be exactly the same as if the condensation took place under the full pressure of the entering saturated steam, and we conclude that *the total heat* as actually determined from his experiments by REGNAULT, is the quantity of heat that would be required, first to raise the liquid to the specified temperature, and then to evaporate it at that temperature; and that the principle on which he determines the latent heat is correct. Hence, through the range of his experiments, that is from 0° to 230°, we may consider the second of the data required for the calculation of μ as being supplied in a complete and satisfactory manner.

34. There remains only the third of the data, or the volume of a given weight of saturated steam, for which accurate experiments through an extensive range are wanting; and no experimental researches bearing on the subject having been made since the time when my former paper was written, I see no reason for supposing that the values of μ which I then gave are not the most probable that can be obtained in the present state of science; and, on the understanding stated in § 33 of that paper, that accurate experimental determinations of the densities of saturated steam at different temperatures may indicate considerable errors in the densities which have been assumed according to the "gaseous laws," and may

* If the steam have to rush through a long fine tube, or through a small aperture within the calorimetrical apparatus, its pressure will be diminished before it is condensed, and there will, therefore, in two parts of the calorimeter be saturated steam at different temperatures (as, for instance, would be the case if steam from a high pressure boiler were distilled into the open air); yet, on account of the heat developed by the fluid friction, which would be precisely the equivalent of the mechanical effect of the expansion wasted in the rushing, the heat measured by the calorimeter would be precisely the same as if the condensation took place at a pressure not appreciably lower than that of the entering steam. The circumstances of such a case have been overlooked by CLAUSIUS (POGGENDORFF'S Annalen, 1850, No. 4, p. 510), when he expresses with some doubt the opinion that the latent heat of saturated steam will be truly found from REGNAULT'S "total heat," by deducting the sensible heat; and gives as a reason that, in the actual experiments, the condensation must have taken place "under the same pressure, or nearly under the same pressure," as the evaporation. The question is not, *Did the condensation take place at a lower pressure than that of the entering steam?* but, *Did* REGNAULT *make the steam work an engine in passing through the calorimeter, or was there so much noise of steam rushing through it as to convert an appreciable portion of the total heat into external mechanical effect?* And a negative answer to this is a sufficient reason for adopting *with certainty* the opinion that the principle of his determination of the latent heat is correct.

consequently render considerable alterations in my results necessary, I shall still continue to use Table I. of that paper, which shews the values of μ for the temperatures $\frac{1}{2}$, $1\frac{1}{2}$, $2\frac{1}{2}$ . . . $230\frac{1}{2}$, or, the mean values of μ for each of the 230 successive centigrade degrees of the air-thermometer above the freezing point, as the basis of numerical applications of the theory. It may be added, that any experimental researches, sufficiently trustworthy in point of accuracy, yet to be made, either on air or on any other substance, which may lead to values of μ differing from those, must be admitted as proving a discrepancy between the true densities of saturated steam, and those which have been assumed.*

35. Table II. of my former paper, which shews the values of $\int_o^t \mu\, dt$ for $t = 1$, $t = 2$, $t = 3$, ... $t = 231$, renders the calculation of the mechanical effect derivable from a given quantity of heat by means of a perfect engine, with any given range included between the limits 0 and 231, extremely easy; since the quantity to be divided by J † in the index of the exponential in expression (8) will be found by subtracting the number in that Table corresponding to the value of T, from that corresponding to the value of S.

36. The following Tables shew some numerical results which have been obtained in this way, with a few (contained in the lower part of the second Table) calculated from values of $\int_o^t \mu\, dt$ estimated for temperatures above 230°, roughly, according to the rate of variation of that function within the experimental limits.

37. *Explanation of the Tables.*

Column I. in each Table shews the assumed ranges.

Column II. shews ranges deduced by means of Table II. of the former paper, so that the value of $\int_T^S \mu\, dt$ for each may be the same as for the corresponding range shewn in Column I.

Column III. shews what would be the duty of a unit of heat if CARNOT'S theory required no modification [or the actual duty of a unit of heat with addi-

* I cannot see that any hypothesis, such as that adopted by CLAUSIUS fundamentally in his investigations on this subject, and leading, as he shews, to determinations of the densities of saturated steam at different temperatures, which indicate enormous deviations from the gaseous laws of variation with temperature and pressure, is more probable, or is probably nearer the truth, than that the density of saturated steam does follow these laws as it is usually assumed to do. In the present state of science it would perhaps be wrong to say that either hypothesis is more probable than the other.

† It ought to be remarked that, as the unit of force implied in the determinations of μ is the weight of a pound of matter at Paris, and the unit of force in terms of which J is expressed is the weight of a pound at Manchester, these numbers ought in strictness to be modified so as to express the values in terms of a common unit of force; but as the force of gravity at Paris differs by less than $\frac{1}{200}$ of its own value from the force of gravity at Manchester, this correction will be much less than the probable errors from other sources, and may therefore be neglected.

tions through the range, to compensate for the quantities converted into mechanical effect].

Column IV. shews the true duty of a unit of heat, and a comparison of the numbers in it with the corresponding numbers in Column III. shews how much the true duty falls short of CARNOT's theoretical duty in each case.

Column VI. is calculated by the formula

$$R = \epsilon^{-\frac{1}{1390}\int_T^S \mu\, dt}$$

where $\epsilon = 2{\cdot}71828$, and for $\int_T^S \mu\, dt$ the successive values shewn in Column III. are used.

Column IV. is calculated by the formula

$$W = 1390\,(1 - R$$

from the values of $1 - R$ shewn in Column V.

38. *Table of the Motive Power of Heat.*

Range of Temperatures.				III.	IV.	V.	VI.
I.		II.		Duty of a Unit of Heat through the whole range.	Duty of a Unit of Heat supplied from the source.	Quantity of Heat converted into mechanical effect.	Quantity of Heat wasted.
S	T	S	T	$\int_T \mu\, dt$	W	1 − R	R
				Ft.-lbs.	Ft.-lbs.		
1°	0°	31·08°	30°	4·960	4·948	·00356	·99644
10	0	40·86	30	48·987	48·1	·0346	·9654
20	0	51·7	30	96·656	93·4	·067	·933
30	0	62·6	30	143·06	136·	·098	·902
40	0	73·6	30	188·22	176·	·127	·873
50	0	84·5	30	232·18	214·	·154	·846
60	0	95·4	30	274·97	249·	·179	·821
70	0	106·3	30	316·64	283·	·204	·796
80	0	117·2	30	357·27	315·	·227	·773
90	0	128·0	30	396·93	345·	·248	·752
100	0	138·8	30	435·69	374·	·269	·731
110	0	149·1	30	473·62	401·	·289	·711
120	0	160·3	30	510·77	427·	·308	·692
130	0	171·0	30	547·21	452·	·325	·675
140	0	181·7	30	582·98	476·	·343	·657
150	0	192·3	30	618·14	499·	·359	·641
160	0	203·0	30	652·74	521·	·375	·625
170	0	213·6	30	686·80	542·	·390	·610
180	0	224·2	30	720·39	562·	·404	·596
190	0	190·	0	753·50	582·	·418	·582
200	0	200·	0	786·17	600·	·432	·568
210	0	210·	0	818·45	619·	·445	·555
220	0	220·	0	850·34	636·	·457	·542
230	0	230·	0	881·87	653·	·470	·530

39. *Supplementary Table of the Motive Powers of Heat.*

Range of Temperatures.				III.	IV.	V.	VI.
I.		II.		Duty of a Unit of Heat through the whole range.	Duty of a Unit of Heat supplied from the source.	Quantity of Heat converted into mechanical effect.	Quantity of Heat wasted.
S	T	S	T	$\int_T^S \mu\, dt$	W	1 − R	R
				Ft.-lbs.	Ft.-lbs.		
101°·1	0°	140°	30°	439·9	377	·271	·729
105·8	0	230	100	446·2	382	·275	·725
300	0	300	0	1099	757	·545	·455
400	0	400	0	1395	879	·632	·368
500	0	500	0	1690	979	·704	·296
600	0	600	0	1980	1059	·762	·238
∞	0	∞	0	∞	1390	1·000	·000

40. Taking the range 30° to 140° as an example suitable to the circumstances of some of the best steam-engines that have yet been made (See Appendix to Account of CARNOT's Theory, sec. v.), we find in Col. III. of the supplementary Table, 377 ft.-lbs. as the corresponding duty of a unit of heat instead of 440, shewn in Col. III., which is CARNOT's theoretical duty. We conclude that the recorded performance of the Fowey-Consols engine in 1845, instead of being only $57\frac{1}{2}$ per cent. amounted really to 67 per cent., or $\frac{2}{3}$ of the duty of a perfect engine with the same range of temperature; and this duty being ·271 (rather more than $\frac{1}{4}$) of the whole equivalent of the heat used; we conclude farther, that $\frac{1}{5 \cdot 19}$, or 18 per cent. of the whole heat supplied, was actually converted into mechanical effect by that steam-engine.

41. The numbers in the lower part of the supplementary Table shew the great advantage that may be anticipated from the perfecting of the air-engine, or any other kind of thermodynamic engine in which the range of the temperature can be increased much beyond the limits actually attainable in steam-engines. Thus an air-engine, with its hot part at 600°, and its cold part at 0° cent., working with perfect economy, would convert 76 per cent. of the whole heat used into mechanical effect; or working with such economy as has been estimated for the Fowey-Consols engine, that is, producing 67 per cent. of the theoretical duty corresponding to its range of temperature, would convert 51 per cent. of all the heat used into mechanical effect.

42. It was suggested to me by Mr JOULE, in a letter dated December 9, 1848,

that the true value of μ might be "inversely at the temperatures from zero;" * and values for various temperatures calculated by means of the formula,

$$\mu = J \frac{E}{1+Et} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11),$$

were given for comparison with those which I had calculated from data regarding steam. This formula is also adopted by CLAUSIUS, who uses it fundamentally in his mathematical investigations. If μ were correctly expressed by it, we should have

$$\int_T^S \mu \, dt = J \log \frac{1+ES}{1+ET};$$

and therefore equations (1) and (2) would become

$$W = J \frac{S-T}{\frac{1}{E}+S} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12),$$

$$R = \frac{\frac{1}{E}+T}{\frac{1}{E}+S} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13).$$

43. The reasons upon which Mr JOULE's opinion is founded, that the preceding equation (11) may be the correct expression for CARNOT's function, although the values calculated by means of it differ considerably from those shewn in Table I. of my former paper, form the subject of a communication, which I hope to have an opportunity of laying before the Royal Society previously to the close of the present session.

PART III.—APPLICATIONS OF THE DYNAMICAL THEORY TO ESTABLISH RELATIONS BETWEEN THE PHYSICAL PROPERTIES OF ALL SUBSTANCES.

44. The two fundamental equations of the dynamical theory of heat, investigated above, express relations between quantities of heat required to produce changes of volume and temperature in any material medium whatever,

* If we take $\mu = k \frac{E}{1+Et}$ where k may be any constant, we find

$$W = J \left(\frac{S-T}{\frac{1}{E}+S}\right)^{\frac{k}{J}};$$

which is the formula I gave when this paper was communicated. I have since remarked, that Mr JOULE's hypothesis implies essentially, that the coefficient k must be as it is taken in the text, the mechanical equivalent of a thermal unit. Mr RANKINE, in a letter dated March 27, 1851, informs me that he has deduced, from the principles laid down in his paper communicated last year to this Society, an approximate formula for the ratio of the maximum quantity of heat converted into mechanical effect to the whole quantity expended, in an expansive engine of any substance, which, on comparison, I find agrees exactly with the expression (12) given in the text as a consequence of the hypothesis suggested by Mr JOULE regarding the value of μ at any temperature.—[*April* 4, 1851.]

subjected to a uniform pressure in all directions, which lead to various remarkable conclusions. Such of these as are independent of JOULE's principle (expressed by equation (2) of § 20), being also independent of the truth or falseness of CARNOT's contrary assumption regarding the permanence of heat, are common to his theory and to the dynamical theory; and some of the most important of them,* have been given by CARNOT himself, and other writers who adopted his principles and mode of reasoning without modification. Other remarkable conclusions on the same subject might have been drawn from the equation $\frac{d\,\mathrm{M}}{d\,t}-\frac{d\,\mathrm{N}}{d\,v}=0$, expressing CARNOT's assumption (of the truth of which experimental tests might have been thus suggested); but I am not aware that any conclusion deducible from his assumption besides that which CARNOT gives regarding the motive power of heat through finite ranges of temperature, has yet been actually obtained and published.

45. The recent writings of CARNOT and CLAUSIUS contain some of the consequences of the fundamental principle of the dynamical theory (expressed in the first fundamental proposition above) regarding physical properties of various substances; among which may be mentioned especially a very remarkable discovery regarding the specific heat of saturated steam (investigated also in this paper in § 58 below), made independently by the two authors, and a property of water at its freezing point, deduced from the corresponding investigation regarding ice and water under pressure by CLAUSIUS; according to which he finds that, for each $\frac{1}{10}^{\circ}$ cent. that the solidifying point of water is lowered by pressure, its latent heat, which, under atmospheric pressure is 79, is diminished by ·081. The investigations of both these writers involve fundamentally various hypotheses which may be or may not be found by experiment to be approximately true; and which render it difficult to gather from their writings what part of their conclusions, especially with reference to air and gases, depend merely on the necessary principles of the dynamical theory.

46. In the remainder of this paper, the two fundamental propositions, expressed by the equations

$$\frac{d\,\mathrm{M}}{d\,t}-\frac{d\,\mathrm{N}}{d\,v}=\frac{1}{\mathrm{J}}\frac{d\,p}{d\,t} \qquad \text{(2) of § 20}$$

$$\text{and } \mathrm{M}=\frac{1}{\mu}\cdot\frac{d\,p}{d\,t} \qquad \text{(3) of § 21,}$$

are applied to establish properties of the specific heats of any substance whatever; and then special conclusions are deduced for the case of a fluid following strictly the "gaseous laws" of density, and for the case of a medium consisting of parts

* See above, § 22.

in different states at the same temperature, as water and saturated steam, or ice and water.

47. In the first place, it may be remarked, that, by the definition of M and N in § 20, N must be what is commonly called the "specific heat at constant volume" of the substance, provided the quantity of the medium be the standard quantity adopted for specific heats, which, in all that follows, I shall take as the unit of weight. Hence the fundamental equation of the dynamical theory, (2) of § 20, expresses a relation between this specific heat and the quantities for the particular substance denoted by M and p. If we eliminate M from this equation, by means of equation (3) of § 21, derived from the expression of the second fundamental principle of the theory of the motive power of heat, we find

$$\frac{d\mathrm{N}}{dv} = \frac{d\left(\frac{1}{\mu}\frac{dp}{dt}\right)}{dt} - \frac{1}{\mathrm{J}}\frac{dp}{dt} \quad . \quad . \quad . \quad . \quad (14),$$

which expresses a relation between the variation in the specific heat at constant volume, of any substance produced by an alteration of its volume at a constant temperature, and the variation of its pressure with its temperature when the volume is constant; involving a function, μ, of the temperature, which is the same for all substances.

48. Again, let K denote the specific heat of the substance under constant pressure. Then, if dv and dt be so related that the pressure of the medium when its volume and temperature are $v+dv$ and $t+dt$, respectively, is the same as when they are v and t, that is, if

$$0 = \frac{dp}{dv}dv + \frac{dp}{dt}dt;$$

we have

$$\mathrm{K}\,dt = \mathrm{M}\,dv + \mathrm{N}\,dt.$$

Hence we find

$$\mathrm{M} = \frac{-\frac{dp}{dv}}{\frac{dp}{dt}}(\mathrm{K}-\mathrm{N}) \quad . \quad . \quad . \quad . \quad . \quad (15),$$

which merely shews the meaning, in terms of the two specific heats, of what I have denoted by M. Using in this for M its value given by (3) of § 21, we find

$$\mathrm{K}-\mathrm{N} = \frac{\left(\frac{dp}{dt}\right)^2}{\mu \times -\frac{dp}{dv}} \quad . \quad . \quad . \quad . \quad . \quad (16),$$

an expression for the difference between the two specific heats, derived without hypothesis, from the second fundamental principle of the theory of the motive power of heat.

49. These results may be put into forms more convenient for use, in applications to liquid and solid media, by introducing the notation:—

$$\left.\begin{aligned} \kappa &= v \times -\frac{dp}{dv} \\ e &= \frac{1}{\kappa}\frac{dp}{dt} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17),$$

where κ will be the reciprocal of the compressibility, and e the coefficient of expansion with heat.

Equations (14), (16), and (3), thus become

$$\frac{dN}{dv} = \frac{d\left(\frac{\kappa e}{\mu}\right)}{dt} - \frac{\kappa e}{J} \quad . \quad . \quad . \quad . \quad . \quad . \quad (18),$$

$$K - N = v\frac{\kappa e^2}{\mu} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19),$$

$$M = \frac{1}{\mu} . \kappa e \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20);$$

the third of these equations being annexed to shew explicitly the quantity of heat developed by the compression of the substance kept at a constant temperature. Lastly, if θ denote the rise in temperature produced by a compression from $v+dv$ to v, before any heat is emitted, we have

$$\theta = \frac{1}{N} . \frac{\kappa e}{\mu} . dv = \frac{\kappa e}{\mu K - v\kappa e^2} dv \quad . \quad . \quad . \quad (21).$$

50. The first of these expressions for θ shews that, when the substance contracts as its temperature rises (as is the case, for instance, with water between its freezing point and its point of maximum density), its temperature would become lowered by a sudden compression. The second, which shews, in terms of its compressibility and expansibility, exactly how much the temperature of any substance is altered by an infinitely small alteration of its volume, leads to the approximate expression

$$\theta = \frac{\kappa e}{\mu K}$$

if, as is probably the case for all known solids and liquids, e be so small that $e . v\kappa e$ is very small compared with μK.

51. If, now, we suppose the substance to be a gas, and introduce the hypothesis that its density is strictly subject to the "gaseous laws," we should have, by Boyle and Mariotte's law of compression,

$$\frac{dp}{dv} = -\frac{p}{v} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22);$$

and by Dalton and Gay Lussac's law of expansion,

$$\frac{dv}{dt} = \frac{Ev}{1+Et} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (23);$$

from which we deduce

$$\frac{dp}{dt} = \frac{Ep}{1+Et}.$$

Equation (14) will consequently become

$$\frac{dN}{dv} = \frac{d\left\{\frac{Ep}{\mu(1+Et)} - \frac{p}{J}\right\}}{dt} \quad . \quad . \quad . \quad . \quad . \quad (24),$$

a result peculiar to the dynamical theory; and equation (16),

$$K - N = \frac{E^2 p v}{\mu(1+Et)^2} \quad . \quad . \quad . \quad . \quad . \quad (25),$$

which agrees with the result of § 53 of my former paper.

If V be taken to denote the volume of the gas at the temperature 0°, under unity of pressure, (25) becomes

$$K - N = \frac{E^2 V}{\mu(1+Et)} \quad . \quad . \quad . \quad . \quad . \quad (26).$$

52. All the conclusions obtained by CLAUSIUS, with reference to air or gases, are obtained immediately from these equations, by taking

$$\mu = J\frac{E}{1+Et},$$

which will make $\frac{dN}{dv}=0$, and by assuming, as he does, that N, thus found to be independent of the density of the gas, is also independent of its temperature.

53. As a last application of the two fundamental equations of the theory, let the medium, with reference to which M and N are defined, consist of a weight $1-x$ of a certain substance in one state, and a weight x in another state at the same temperature, containing more latent heat. To avoid circumlocution and to fix the ideas, in what follows, we may suppose the former state to be liquid, and the latter gaseous; but the investigation, as will be seen, is equally applicable to the case of a solid in contact with the same substance in the liquid or gaseous form.

54. The volume and temperature of the whole medium being, as before, denoted respectively by v and t; we shall have

$$\lambda(1-x)+\gamma x = v \quad . \quad . \quad . \quad . \quad . \quad (27),$$

if λ and γ be the volumes of unity of weight of the substance in the liquid and the gaseous states respectively; and p, the pressure, may be considered as a function of t, depending solely on the nature of the substance. To express M and N for this mixed medium, let L denote the latent heat of a unit of weight of the vapour; c the specific heat of the liquid; and h the specific heat of the vapour when kept in a state of saturation. We shall have

$$M\,dv = L\frac{dx}{dv}\,dv$$

$$N\,dt = c(1-x)\,dt + hx\,dt + L\frac{dx}{dt}\,dt.$$

Now, by (27), we have

$$(\gamma-\lambda)\frac{dx}{dv}=1 \quad . \quad . \quad . \quad . \quad . \quad 28),$$

and

$$(\gamma-\lambda)\frac{dx}{dt}+(1-x)\frac{d\lambda}{dt}+x\frac{d\gamma}{dt}=0 \quad . \quad . \quad (29).$$

Hence

$$M=\frac{L}{\gamma-\lambda} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (30),$$

$$N=c(1-x)+hx-L\frac{(1-x)\frac{d\lambda}{dt}+x\frac{d\gamma}{dt}}{\gamma-\lambda} \quad . \quad . \quad (31).$$

55. The expression of the second fundamenta proposition in this case becomes, consequently,

$$\mu=\frac{(\gamma-\lambda)\frac{dp}{dt}}{L} \quad . \quad . \quad . \quad . \quad . \quad . \quad (32),$$

which agrees with CARNOT's original result, and is the formula that has been used (referred to above in § 31) for determining μ by means of REGNAULT's observations on steam.

56. To express the conclusion derivable from the first fundamental proposition, we have, by differentiating the preceding expressions for M and N with reference to t and v respectively,

$$\frac{dM}{dt}=\frac{1}{\gamma-\lambda}\cdot\frac{dL}{dt}-\frac{L}{(\gamma-\lambda)^2}\cdot\frac{d(\gamma-\lambda)}{dt}$$

$$\frac{dN}{dv}=\left(h-c-L\frac{\frac{d\gamma}{dt}-\frac{d\lambda}{dt}}{\gamma-\lambda}\right)\frac{dx}{dv}$$

$$=\left\{\frac{h-c}{\gamma-\lambda}-\frac{L}{(\gamma-\lambda)^2}\right\}\frac{d(\gamma-\lambda)}{dt}$$

Hence equation (2) of § 20 becomes

$$\frac{\frac{dL}{dt}+c-h}{\gamma-\lambda}=\frac{1}{J}\frac{dp}{dt} \quad . \quad . \quad . \quad . \quad . \quad (33).$$

Combining this with the conclusion (32) derived from the second fundamental proposition, we obtain

$$\frac{dL}{dt}+c-h=\frac{L\mu}{J} \quad . \quad . \quad . \quad . \quad . \quad (34).$$

The former of these equations agrees precisely with one which was first given by CLAUSIUS, and the preceding investigation is substantially the same as the investigation by which he arrived at it. The second differs from another given by CLAUSIUS only in not implying any hypothesis as to the form of CARNOT's function, μ.

57. If we suppose μ and L to be known for any temperature, equation (32) enables us to determine the value of $\frac{dp}{dt}$ for that temperature; and thence deducing a value of dt, we have

$$dt = \frac{\gamma - \lambda}{\mu L} dp \quad . \quad . \quad . \quad . \quad . \quad . \quad (35);$$

which shews the effect of pressure in altering the "boiling point" if the mixed medium be a liquid and its vapour, or the melting point if it be a solid in contact with the same substance in the liquid state. This agrees with the conclusion arrived at by my elder brother in his Theoretical Investigation of the Effect of Pressure in Lowering the Freezing Point of Water.* His result, obtained by taking as the value for μ, that derived from Table I. of my former paper for the temperature 0°, is that the freezing point is lowered by ·0075° cent. by an additional atmosphere of pressure. CLAUSIUS, with the other data the same, obtains ·00733° as the lowering of temperature produced by the same additional pressure, which differs from my brother's result only from having been calculated from a formula which implies the hypothetical expression $J \frac{E}{1+Et}$ for μ. It was by applying equation (33) to determine $\frac{dL}{dt}$ for the same case, that CLAUSIUS arrived at the curious result regarding the latent heat of water under pressure, mentioned above (§ 45).

58. Lastly, it may be remarked that every quantity except h, which appears in equation (33), is known with tolerable accuracy for saturated steam through a wide range of temperature; and we may therefore use this equation to find h, which has never yet been made an object of experimental research. Thus we have

$$-h = \frac{\gamma - \lambda}{J} \frac{dp}{dt} - \left(\frac{dL}{dt} + c\right)$$

For the value of γ the best data regarding the density of saturated steam that can be had must be taken. If for different temperatures we use the same values for the density of saturated steam (calculated according to the gaseous laws, and REGNAULT's observed pressure from $\frac{1}{1693\cdot5}$, taken as the density at 100°), the values obtained for the first term of the second member of the preceding equation are the same as if we take the form

$$-h = \frac{L\mu}{J} - \left(\frac{dL}{dt} + c\right)$$

derived from (34), and use the values of μ shewn in Table I. of my former paper. The values of $-h$ in the second column in the following table have been so calcu-

* Transactions, Vol. xvi., Part v. His paper was republished, with some slight modifications, in the Cambridge and Dublin Mathematical Journal, New Series, Vol. V.—Nov. 1850.

lated, with, besides, the following data afforded by REGNAULT from his observations on the total heat of steam, and the specific heat of water

$$\frac{d\,\mathrm{L}}{d\,t} + c = \cdot 305.$$

$$\mathrm{L} = 606{\cdot}5 + {\cdot}305\,t - ({\cdot}00002\,t^2 + {\cdot}0000003\,t^3).$$

The values of $-h$ shewn in the third column are those derived by CLAUSIUS from an equation which is the same as what (34) would become if $\mathrm{J}\,\frac{\mathrm{E}}{1+\mathrm{E}\,t}$ were substituted for μ.

t	$-h$ according to Table I. of "Account of CARNOT's Theory."	$-h$ according to CLAUSIUS.
0	1·863	1·916
50	1·479	1·465
100	1·174	1·133
150	0·951	0·879
200	0·780	0·676

59. From these results, it appears that through the whole range of temperatures at which observations have been made, the value of h is negative; and, therefore, if a quantity of saturated vapour be compressed in a vessel containing no liquid water, heat must be continuously abstracted from it in order that it may remain saturated as its temperature rises; and conversely, if a quantity of saturated vapour be allowed to expand in a closed vessel, heat must be supplied to it to prevent any part of it from becoming condensed into the liquid form as the temperature of the whole sinks. This very remarkable conclusion was first announced by Mr RANKINE, in his paper communicated to this Society on the 4th of February last year. It was discovered independently by CLAUSIUS, and published in his paper in POGGENDORFF's Annalen in the months of April and May of the same year.

60. It might appear at first sight, that the well-known fact, that steam rushing from a high-pressure boiler through a small orifice into the open air, does not scald a hand exposed to it, is inconsistent with the proposition, that steam expanding from a state of saturation must have heat given to it to prevent any part from becoming condensed; since the steam would scald the hand unless it were dry, and consequently above the boiling point in temperature. The explanation of this apparent difficulty, given in a letter which I wrote to Mr JOULE last October, and which has since been published in the Philosophical Magazine,*

* This explanation has been objected to as incorrect in principle by CLAUSIUS, in an article recently published in POGGENDORFF's Annalen. I trust that, on reconsidering the subject (and, should this meet his eye, on reading the statement in the text, and the remarks in § 33 above), he will perceive that my explanation, as originally stated, is perfectly correct.

is, that the steam in rushing through the orifice produces mechanical effect which is immediately wasted in fluid friction, and, consequently reconverted into heat, so that the issuing steam at the atmospheric pressure would have to part with as much heat to convert it into water at the temperature 100° as it would have had to part with to have been condensed at the high pressure and then cooled down to 100°, which, for a pound of steam, initially saturated at the temperature t, is, by REGNAULT'S modification of WATT'S law, $\cdot 305\,(t - 100^{\circ})$ more heat than a pound of saturated steam at 100° would have to part with to reduce it to the same state; and the issuing steam must, therefore, be above 100° in temperature, and dry.

XVI.—*On a Method of Discovering experimentally the Relation between the Mechanical Work spent, and the Heat produced by the Compression of a Gaseous Fluid.* By WILLIAM THOMSON, M.A., Fellow of St Peter's College, Cambridge, and Professor of Natural Philosophy in the University of Glasgow.

(Read 21st April 1851.)

1. The important researches of JOULE on the thermal circumstances connected with the expansion and compression of air, and the admirable reasoning upon them, expressed in his paper* "On the Changes of Temperature produced by the Rarefaction and Condensation of Air," especially the way in which he takes into account any mechanical effect that may be externally produced, or internally lost, in fluid friction, have introduced an entirely new method of treating questions regarding the physical properties of fluids. The object of the present paper is to show how, by the use of this new method, in connection with the principles explained in my preceding paper, a complete theoretical view may be obtained of the phenomena experimented on by JOULE; and to point out some of the objects to be attained by a continuation and extension of his experimental researches.

2. The Appendix to my Account of CARNOT's Theory† contains a theoretical investigation of the heat developed by the compression of any fluid fulfilling the laws‡ of BOYLE and MARIOTTE and of DALTON and GAY LUSSAC. It has since been shown that that investigation requires no modification when the Dynamical Theory is adopted, and therefore the formula obtained as the result may be regarded as being established for a fluid of the kind assumed, independently of any hypothesis whatever. We may obtain a corresponding formula applicable to a fluid not fulfilling the gaseous laws of density, or to a solid pressed uniformly on all sides, in the following manner.

3. Let $M\,dv$ be the quantity of heat absorbed by a body kept at a constant temperature t, when its volume is increased from v to $v+dv$; let p be the uniform pressure which it experiences from without, when its volume is v and its temperature t; and let $p+\frac{dp}{dt}dt$ denote the value p would acquire if the temperature were raised to $t+dt$, the volume remaining unchanged. Then, by equation (3) of

* Philosophical Magazine, May 1845, vol. xxvi., p. 369.

† Transactions, vol. xvi., part V.

‡ To avoid circumlocution these laws will, in what follows, be called simply, the *gaseous laws*, or the *gaseous laws* of density.

§ 21 of my former paper, derived from CLAUSIUS's extension of CARNOT's theory, we have

$$M = \frac{1}{\mu} \cdot \frac{dp}{dt} \quad . \quad . \quad . \quad . \quad . \quad . \quad (a)^*$$

where μ denotes CARNOT's *function*, the same for all substances at the same temperature.

Now let the substance expand from any volume V to V′, and, being kept constantly at the temperature t, let it absorb a quantity, H, of heat. Then

$$H = \int_V^{V'} M \, dv = \frac{1}{\mu} \frac{d}{dt} \int_V^{V'} p \, dv \quad . \quad . \quad . \quad (b).$$

But, if W denote the mechanical work which the substance does in expanding, we have

$$W = \int_V^{V'} p \, dv \quad . \quad . \quad . \quad . \quad . \quad . \quad (c),$$

and therefore

$$H = \frac{1}{\mu} \frac{dW}{dt} \quad . \quad . \quad . \quad . \quad . \quad . \quad (d).$$

This formula, established without any assumption admitting of doubt, expresses the relation between the heat developed by the compression of any substance whatever, and the mechanical work which is required to effect the compression; as far as it can be determined without hypothesis, by purely theoretical considerations.

4. The preceding formula leads to that which I formerly gave for the case of fluids subject to the gaseous laws; since for such we have

$$p v = p_0 v_0 (1 + E t) \quad . \quad . \quad . \quad . \quad . \quad (1),$$

from which we deduce, by (c),

$$W = p_0 v_0 (1 + E t) \log \frac{V'}{V} \quad . \quad . \quad . \quad (2),$$

and

$$\frac{dW}{dt} = E p_0 v_0 \cdot \log \frac{V'}{V} = \frac{E}{1 + E t} \cdot W \quad . \quad . \quad . \quad (3);$$

and therefore, by (d),

$$H = \frac{E}{\mu (1 + E t)} \cdot W \quad . \quad . \quad . \quad . \quad (4),$$

which agrees with equation (11) of § 49 of the former paper.

5. Hence we conclude that the heat evolved by any fluid fulfilling the gaseous laws, is proportional to the work spent in compressing it, at any given constant temperature; but that the quantity of work required to produce a unit of heat

* Throughout this paper formulæ, which involve no hypothesis whatever, are marked with italic letters; formulæ which involve BOYLE's and DALTON's laws are marked with Arabic numerals; and formulæ involving, besides, MAYER's hypothesis, are marked with Roman numerals.

is not constant for all temperatures, unless CARNOT's function for different temperatures vary inversely as $1+Et$, and that it is not the simple mechanical equivalent of the heat, as it was unwarrantably* assumed by MAYER to be, unless this function have precisely the expression

$$\mu = J \cdot \frac{E}{1+Et} \quad . \quad . \quad . \quad . \quad . \quad . \quad (I.)$$

This formula was suggested to me by Mr JOULE, in a letter dated December 9th, 1848, as probably a true expression for μ, being required to reconcile the expression derived from CARNOT's theory (which I had communicated to him), for the heat evolved in terms of the work spent in the compression of a gas, with the hypothesis that the latter of these is exactly the mechanical equivalent of the former, which he had adopted in consequence of its being, at least approximately, verified by his own experiments. This, which will be called MAYER's hypothesis, from its having been first assumed by MAYER, is also assumed by CLAUSIUS without any reason from experiment; and an expression for μ the same as the preceding, is consequently adopted by him as the foundation of his mathematical deductions from elementary reasoning regarding the motive power of heat. The preceding formulæ show that if it be true at a particular temperature for any one fluid fulfilling the gaseous laws, it must be true for every such fluid at the same temperature.

6. Of the various experimental researches which might be suggested as suitable for testing MAYER's hypothesis, it appears, from the preceding formula, that any which would give data for the determination of the values of μ through a wide range of temperatures, would, with a single accurate determination of J, afford a complete test. Thus an experimental determination of the density of saturated steam for temperatures from 0° to 230° cent., would complete the data, of which a part have been so accurately determined by REGNAULT, for the calculation of the values of μ between those wide limits, and would contribute more, perhaps, than any set of experimental researches that could at present be proposed, to advance the mechanical theory of heat.

7. The values of μ, given in Table I. of my Account of CARNOT's Theory, which were calculated from REGNAULT's observations on steam, with the assumption of $\frac{1}{1693{\cdot}5}$, (the maximum density of water being unity) for the density of saturated steam at 100° cent., and of the gaseous laws for calculating it by means of REGNAULT's observed pressures, at other temperatures, are far from verifying equation (I.), as appears from the Table of the values of $\frac{\mu(1+Et)}{E}$, given in the preceding paper, § 51; or as the following comparative Table shows:—

* In violation of CARNOT's important principle, that thermal agency and mechanical effect, or mechanical agency and thermal effect, cannot be regarded in the simple relation of cause and effect, when any other effect, such as the alteration of the density of a body, is finally concerned.

Col. 1. Temperature. t	Col. 2. Values of μ according to assumed density of saturated steam. $[\mu]$	Col. 3. Values of μ according to JOULE's formula. $J\frac{E}{1+Et}$	Col. 4. Values of μ according to modified assumption for density of saturated steam $\frac{1717\cdot6}{1693\cdot5}\times[\mu]$
0	4·967	5·087	5·038
10	4·832	4·908	4·901
20	4·703	4·740	4·769
30	4·578	4·584	4·643
40	4·456	4·438	4·519
50	4·337	4·300	4·399
60	4·221	4·171	4·281
70	4·114	4·050	4·172
80	4·013	3·935	4·070
90	3·921	3·827	3·977
100	3·833	3·724	3·887
110	3·753	3·627	3·806
120	3·679	3·535	3·731
130	3·611	3·447	3·662
140	3·546	3·364	3·596
150	3·487	3·284	3·536
160	3·432	3·209	3·481
170	3·382	3·136	3·430
180	3·335	3·067	3·382
190	3·289	3·001	3·336
200	3·247	2·937	3·293
210	3·208	2·876	3·254
220	3·171	2·818	3·216
230	3·135	2·762	3·179

Mr JOULE, when I pointed out these discrepancies to him in the year 1848, suggested that even between 0° and 100°, the inaccuracy of the data regarding steam might be sufficient to account for them. I think it will be generally admitted that there can be no such inaccuracy in REGNAULT's part of the data, and there remains only the uncertainty regarding the density of saturated steam, to prevent the conclusion that μ cannot be expressed by $J\frac{E}{1+Et}$; so that MAYER's hypothesis would be confirmed if, and overturned unless, the density of saturated steam, instead of following the gaseous laws, were truly expressed by the equations

$$\left.\begin{aligned} \sigma &= \frac{\left(\frac{1}{E}+t\right)[\mu]}{J}\cdot[\sigma] \\ [\sigma] &= \frac{1}{1693\cdot5}\cdot\frac{1+E\times100}{1+Et}\cdot\frac{p}{\Pi} \end{aligned}\right\} \quad . \quad . \quad . \quad \text{(II.)}$$

where $[\mu]$ denotes the quantity tabulated for the temperatures 0°, 1°, 2°, . . . 230° in Table I. of my Account of CARNOT's Theory; and $[\sigma]$ denotes the density of saturated steam which was assumed in the calculation of that Table, the values of $\frac{p}{\Pi}$

in the expression for it being obtained by dividing the numbers tabulated at the end of REGNAULT's eighth Mémoire by 760. The considerableness of the deviations from the gaseous laws which equation (II.) indicates, is seen at once by comparing the numbers in Col. 2 with those in Col. 3 of the preceding Table, and observing that the coefficient of $[\sigma]$ in (II.) is, for each temperature shown in that Table, obtained by dividing the corresponding number in Col. 2 by that in Col. 3. Col. 4 shows what the values of μ would be if the density of saturated steam at 100° were $\frac{1}{1717{\cdot}6}$ instead of $\frac{1}{1693{\cdot}5}$, and, for other temperatures, varied according to the gaseous laws.

8. This subject has been very carefully examined by CLAUSIUS, who has indicated the great deviations from the gaseous laws of density that MAYER's hypothesis requires in saturated steam, and has given an empirical formula for the density of saturated steam founded on that hypothesis, and on REGNAULT's observations on the pressure and latent heat. In this direction theory can go no farther, for want of experimental data, although, from what we know of gases and saturated vapours, it may be doubted whether such excessive deviations, in the case of steam, from the laws of a "perfect gas" are rendered probable by a hypothesis resting on no experimental evidence whatever.*

9. To JOULE we are indebted for a most important series of experimental researches on the relation between the thermal effects, the external mechanical effects, and the internal mechanical effects (*vis viva* destroyed by fluid friction) due to compressions and expansions of air in various circumstances.† These researches afford actual tests which, so far as they go, are verifications of the truth of MAYER's hypothesis for temperatures between 50° and 60° Fahr., founded on two distinct methods, either of which is perfect in principle, and might be made the foundation of experiments at any temperature whatever.

10. The first of these methods consists simply in determining, by direct experiment, the heat evolved by the expenditure of a given amount of work in compressing air, and comparing it with the quantity of heat created by the same amount of work in JOULE's original experiments on the heat developed by magneto-electricity, and by the friction of fluids in motion.

11. The second method is especially remarkable, as affording, in each experiment, an independent test of the truth of MAYER's hypothesis for air at the temperature used, without requiring any knowledge of the absolute value of the mechanical equivalent of heat. In JOULE's actual experiments, the test is simply

* JOULE's experimental verification of MAYER's law for temperatures of from 50° to 60° Fahr., shews, if rigorously exact, that the density of saturated steam at about 10° centigrade must be $\frac{1693{\cdot}5}{1717{\cdot}6}$ of what was assumed for it in the calculations of my former paper, but does not go towards indicating any deviation from the gaseous laws of variation in the density of saturated steam at different temperatures.

† Philosophical Magazine, May 1845.

this:—The total external thermal effect is determined when air is allowed to expand, through a small orifice, from one vessel into another previously exhausted by an air-pump. Here the first mechanical effect produced by the expanding gas, is *vis viva* generated in the rushing of the air. By the time equilibrium is established, all this mechanical effect has been lost in fluid friction (there being no appreciable mechanical effect produced externally in sound, which is the only external mechanical effect, other than heat, that can be produced by the motions of a fluid within a fixed rigid vessel); and no truth in physical science can be more certain than that by the time thermal as well as mechanical equilibrium is established at the primitive temperature, the contents of the two vessels must have parted with just as much more heat than they would have parted with, had the air in expanding pushed out a piston against an external resisting force, as is equivalent to the mechanical effect thus produced externally. Hence, if the two vessels and the tube connecting them be immersed (as they are in JOULE's first set of experiments with this apparatus) in one vessel of water, and if, after time is allowed for the pressure and temperature of the air to become the same in the two vessels, the water be found to have neither gained nor lost heat (it being understood, of course, that the air and all other matter external to the water are at an absolutely constant temperature during the experiment), then, for the temperature of the experiment, MAYER's hypothesis is perfectly confirmed; but any final elevation or depression of temperature in the water, would show that the work due to the expansion is either greater than or less than the absolute equivalent of the heat absorbed.

12. Mr JOULE's second experiment on the same apparatus, in which he examined separately the external thermal effects round each of the two vessels, and round a portion of the tube containing the small orifice (a stop-cock) has suggested to me a method which appears still simpler, and more suitable for obtaining an excessively delicate test of MAYER's hypothesis for any temperature. It consists merely in dispensing with the two vessels in JOULE's apparatus, and substituting for them two long spirals of tube (instead of doing this for only one of the vessels, as JOULE does in his third experiment with the same apparatus); and in forcing air continuously through the whole. The first spiral portion of the tube, up to a short distance from the orifice, ought to be kept as nearly as possible at the temperature of the atmosphere surrounding the portion containing the orifice, and serves merely to fix the temperature of the entering air. The following investigation shows what conclusions might be drawn by experimenting on the thermal phenomena of any fluid whatever treated in this manner.

13. Let p be the uniform pressure of the fluid in the first spiral, up to a short distance from the orifice, and let p' be the pressure a short distance from the orifice on the other side, which will be uniform through the second spiral. Let t be the constant external temperature, and let the air in both spirals be kept as

closely as possible at the same temperature. If there be any elevation or depression of temperature of the fluid in passing through the orifice, it may only be after passing through a considerable length of the second spiral that it will again arrive sensibly at the temperature, t, and the spiral must be made at least so long that the fluid issuing from the open end of it, when accurately tested, may be found not to differ appreciably from the primitive temperature, t.

14. Let H be the total quantity of heat emitted from the portion of the tube containing the orifice, and the second spiral, during the passage of a volume u through the first spiral, or of an equivalent volume u' through the parts of the second where the temperature is sensibly t. This will consist of two parts; one (positive) the heat produced by the fluid friction, and the other (negative) the heat emitted by that portion of the fluid which passes from one side to the other of the orifice, in virtue of its expansion. To find these two parts, let us first suppose the transference of the fluid to take place without loss of mechanical effect in fluid friction, as it would do if, instead of the partition with a small orifice, there were substituted a moveable piston, and if a volume u of fluid, on the side where the pressure is higher (p), were enclosed between that and another piston, and allowed to slide through the tube till the second piston should take the place of the first, and to expand till its volume should be u'. If we adopt the same notation with reference to the volume, v, of the substance between the pistons, kept at a constant temperature, t, as has been used uniformly in this and the preceding paper; we shall have, for the quantity of heat absorbed during the motion of the piston,

$$\int_u^{u'} \mathrm{M}\, dv;$$

or, by the second fundamental equation of the theory, (3) of § 21 of the preceding paper,

$$\frac{1}{\mu}\int_u^{u'} \frac{d\varpi}{dt}\, dv,$$

where ϖ denotes the actual pressure (intermediate between p and p') of the substance when its volume is v. Again, the work done by the pistons will be given by the equation

$$\mathrm{W} = \int_u^{u'} \varpi\, dv + p\,u - p'\,u' \quad . \quad . \quad . \quad . \quad (e).$$

If now the transference of the substance from the one portion of the tube, where the pressure is p, to the other, where the pressure is p', take place through a small orifice, exactly that amount, W, of work will be lost as external mechanical effect, and will go to generate thermal *vis viva*. The quantity of heat thus produced will be

$$\frac{1}{\mathrm{J}}\left\{\int_u^{u'} \varpi\, dv + p\,u - p'\,u'\right\}.$$

Hence the total quantity of heat emitted will be the excess of this above the amount previously found to be absorbed when the mechanical effect is all external; and therefore we have

$$H=\frac{1}{J}\left\{\int_u^{u'}\varpi\,dv+p\,u-p'\,u'\right\}-\frac{1}{\mu}\int_u^{u'}\frac{d\varpi}{dt}\,dv \quad . \quad . \quad (f).$$

Whatever changes of temperature there may actually be of the air in or near the orifice, this expression will give rigorously the total quantity of heat emitted by that portion of tube which contains the orifice and the whole of the second spiral during the passage of a volume u through the first spiral, or u' through any portion of the second spiral where the temperature is sensibly t.

15. To apply this result to the case of a gas fulfilling the gaseous laws, we may put

$$p\,u=p'\,u'.$$

Hence (e) becomes

$$W=\int_u^{u'}\varpi\,dv=p\,u\log\frac{u'}{u}=p'\,u'\log\frac{p}{p'} \quad . \quad . \quad . \quad (5),$$

and, by (3), we have

$$\frac{dW}{dt}=\frac{E\,p\,u}{1+E\,t}\log\frac{u'}{u}=\frac{E\,W}{1+E\,t}.$$

Hence the expression (f) for the heat emitted becomes

$$H=\left\{\frac{1}{J}-\frac{E}{\mu(1+E\,t)}\right\}W \quad . \quad . \quad . \quad . \quad . \quad (6).$$

16. Lastly, if MAYER'S hypothesis be fulfilled for the gas used in the experiment, the coefficient of W vanishes, by (I.), and therefore

$$H=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (III.)$$

17. From equation (III.) it follows that, if MAYER'S hypothesis be true, there is neither emission nor absorption of heat, on the whole, required to reduce the temperature of the air after passing through the orifice, to its primitive value, t. Hence, although no doubt those portions of the air in the intermediate neighbourhood of the orifice, which are communicating, by their expansion, *vis viva* to those contiguous to them, will be becoming colder, and those which are the means of occasioning the portions contiguous to them to lose *vis viva*, through fluid friction, will be becoming warmer at each instant; yet very near the orifice on each side, where the motion of the air is uniform, the temperature would be constantly equal to t. Hence the simplest conceivable test of the truth of MAYER'S hypothesis would be to try whether the temperature of the air is exactly the same on the two sides of the orifice. This might be done by very delicate thermometers adjusted in the tube at sufficient distances on each side of the orifice to be quite out of the *rush* which there is of air in the immediate neighbourhood of the orifice; but it might be done in a still more refined manner by means of a deli-

cate galvanometer, and a small thermo-electric battery arranged so that one set of the solderings might be within the tube on the side of the entering current of air, and the other set within the tube on the side of the current from the orifice. The tube on each side of the orifice would need to be bent so as to bring two parts of it, at small distances from the orifice on each side, near enough one another to admit of the battery being so placed. The only difficulty I can perceive in the way of making the necessary arrangements is what might be experienced in fitting the two ends of the battery air-tight into the two parts of the tube. It first occurred to me that the little battery itself might be placed entirely within the tube, and the difference of pressure kept up in the two parts by the middle of the battery being fitted nearly air-tight in the tube, by means of wax, or otherwise; but this arrangement would not be satisfactory, as portions of the bars of the battery, if not the ends themselves directly, would be altered in temperature, even if MAYER's hypothesis were rigorously true, on account of the rushing of the air among them. No part of the battery ought to be exposed to the rushing of the air in the neighbourhood of the orifice, and therefore the middle of the battery would have to be external to the tube, the ends being cemented into the tube by some indulating cement sufficiently strong and compact to hold perfectly air-tight on the side where the pressure is different from the atmospheric pressure. By such means as these I think a very satisfactory series of experiments might easily be performed to test MAYER's hypothesis for air through a very wide range of temperatures.

18. Should the differential method of experimenting just described indicate any difference of temperature whatever on the two sides of the orifice, MAYER's hypothesis would be shown to be not exactly fulfilled, and, according as the air leaving the orifice is found to be warmer or colder than the entering air, we should infer that the heat absorbed, when air expands at a constant temperature, is less than or greater than the mechanical effect produced by the expansion.

19. Calorimetrical methods, like those used by JOULE, might then be followed for actually determining the heat emitted or absorbed by the air in the neighbourhood of the orifice, or in the second spiral, in acquiring the temperature of the air in the entering stream, and by careful experimenting, it is probable that excessively accurate results might be thus obtained for a wide range of temperature.

20. The result of each experiment would be a value of μ, in terms of JOULE's mechanical equivalent; to be calculated by the following expression, derived from equations (5) and (6).

$$\mu = \frac{\dfrac{\mathrm{J\,E}}{1+\mathrm{E}\,t}}{1-\mathrm{J}\,.\,\dfrac{\mathrm{H}}{p'u'\log\dfrac{p}{p'}}} \qquad . \quad . \quad . \quad . \quad (7).$$

In the second member of this equation p' denotes the pressure of the air through the second spiral, which would be the atmospheric pressure, or excessively near it, if, as in JOULE's third experiment, mentioned above (described by the author in p. 378 of the volume* containing his paper), the air leaving the second spiral be measured by means of a pneumatic trough: p denotes the pressure in the first spiral, which ought to be constant, and must be carefully measured: u' denotes the volume of air which leaves the apparatus in any time; and H denotes the quantity of heat emitted in the same time. The experiment might be continued for any length of time, and each one of these four quantities might be determined with great accuracy, so that probably very accurate direct results of observations might be obtained. If so, no way of experimenting could be better adapted than this to the determination of CARNOT's function, for different temperatures, in terms of JOULE's mechanical equivalent of heat.

* Phil. Mag., vol. xxvi.

XVII.—*On the Weight of Aqueous Vapour which is condensed on a Cold Surface, under given conditions.* By James Dalmahoy, Esq.

(Read 3d March 1851.)

In the accompanying tables are contained the results of some experiments respecting the rate at which aqueous vapour condenses on a cold surface.

These results are not so consistent as could be desired, but having been obtained by a definite and carefully-conducted process, they may claim at least to be received as approximately true.

In planning the experiments, it was assumed that $c=m(f''-f''')$, where c is the weight of moisture which is condensed on a surface of given area in a given time; f'' the tension of vapour at the dew-point; f''' its tension at the temperature of the condensing surface; m a coefficient varying with the velocity of the current of air.

It was found, however, in the course of the experiments, that the coefficient m was not constant for calm air, as was at first supposed. The results under this state of the air indicated that the formula should be changed to $c=\mathrm{M}(t-t''')(f''-f''')$, in which t is the temperature of the air; t''' the temperature of the condensing surface; M a constant coefficient; and f'', f''', and c have their former values.

The object of the experiments in connection with these formulæ was to determine the values of the coefficients m and M from those of their equivalent expressions $\frac{c}{f''-f'''}$ and $\frac{c}{(t-t''')(f''-f''')}$.

As it would have been difficult, if not impossible, to have kept the condensing surface steadily at any low temperature except that of melting ice, the value of f''' throughout the experiment was equal to 0·2 inch, the tension of vapour at 32° Fahr.

In each experiment, therefore, there were only two things to be determined, namely, f'' the tension of vapour at the dew-point, and c the weight of condensed moisture.

The value of f'' was deduced by means of Dr Apjohn's formula, from the indications of the wet and dry bulb thermometers. The data and results connected with this quantity will be found in Table I., from the fifth to the eighth column inclusive. The instruments used were two standard thermometers, made by Messrs Adie and Son, of Edinburgh.

The value of c was ascertained by a simple process, which, however, as the success of the experiments entirely depended on it, must be described in detail.

The aqueous vapour was condensed on the inner surface of a nearly cylindrical copper vessel, the depth of which was 0·5 of an inch; the diameter at the bottom 3 inches, the diameter at the mouth 3·075 inches; consequently the inner area was equal to 11·8 square inches. This vessel had a thin copper lid, the catch of which adhered to the inner surface, and so did not interfere with the thorough removal of moisture from the outer surface.

The temperature of the condensing surface, it has been already remarked, was uniformly 32° Fahr. This temperature was maintained by filling with pounded ice a cylindrical copper box, and burying in it the condensing vessel, all except its upper edge, which was made to fit into, and slightly project above, a circular opening in the top of the box.

The following are the steps of the process by which the weight of the moisture condensed on the cold surface was ascertained.

The condensing vessel and its lid were first carefully dried and then weighed in a balance, which was sensible to ·01 of a grain, when each scale was loaded with 800 grains. The vessel, with its lid closely applied, was now placed among ice, as before described, and allowed to cool for about five minutes. The lid was then removed, and the condensing surface exposed either to a current of air of known velocity, or to calm air. After the exact interval of five minutes the lid was replaced, and the vessel was taken out from among the ice; its temperature was then raised above the dew-point by the heat of the hand, while its outer surface was carefully dried. Lastly, it was again weighed, and the excess of this above the former result was equal to the weight of moisture which had been condensed.

The results thus obtained will be found in Table I., from column twelfth to column seventeenth inclusive.

It appears from column twelfth, that when the air was calm, and the mouth of the condensing vessel was directed upwards, the quantity of condensed vapour was so small and variable as to render it probable that, under this arrangement, there would have been no sensible condensation at all, had it not been for those irregular currents, which are known to prevail even in air apparently calm.

Column thirteenth shews that when the air was calm, and the mouth of the vessel was turned downwards, much more vapour was condensed than when the mouth was upwards. This was evidently the effect of a current produced by contact of the warm air with the cold condensing surface, and which, for obvious reasons, could not take place when the mouth of the vessel was directed upwards. The effect of this current on the rate of condensation seemed to vary nearly as $t-t'''$, at least it would be difficult, except on this assumption, to reconcile the four last with the preceding results in the column.

The last four columns exhibit the results of experiments, in which the condensing surface was exposed to an artificial current of air.

The current was produced by means of a common fire-blower, and the velo-

city of it was regulated by the number of revolutions made by the handle of the instrument in the course of ten seconds. The results correspond respectively to 5, 10, 18, and 25 of these revolutions, or on a rough estimate to actual velocities of 4·12, 8·24, 14·8, and 20·6 feet per second.

With such an instrument it was difficult to maintain a rapid current at a uniform rate, and hence, probably, the reason why the results due to five and ten revolutions of the handle are less discordant than those corresponding to eighteen and twenty-five revolutions.

The second table contains the values of $M=\frac{c}{(t-t'')(f''-f''')}$ and of $m=\frac{c}{f''-f'''}$, the former corresponding to the experiments in the thirteenth column, and the latter to the experiments in the fourteenth, fifteenth, sixteenth, and seventeenth columns of the first table. The results in column twelfth, for reasons which will be obvious from the remarks on that column, are omitted in the second table.

Taking the mean of the numbers in each column of this table, and changing the unit of surface from 11·8 to 100 square inches, and the unit of time from five minutes to one minute, it appears that the value of M for calm air is 0·12, and that the values of m for velocities of 4·12, 8·24, 14·8, and 20·6 feet per second are respectively 18·3, 26·5, 39·7, and 44·6.

These results may be useful in various meteorological investigations; but at present it is proposed to apply them only to one question connected with the theory of rain.

The admirable observations of Professor Phillips and Mr Gray, published in the Reports* of the British Association, shew that at York there fell in the course of three years, into a rain-gauge placed on the ground, 65·430 inches of rain; into a gauge placed at the height of 43·7 feet, 52·169 inches; and into a gauge at the height of 213 feet, 38·972 inches.

Professor Phillips has proposed, in explanation of these anomalous results, an hypothesis which may be thus enunciated. As rain falls from a considerable height in the atmosphere, its temperature is less than that of the dew-point of the vapour through which it passes in the course of its descent; and this gives rise to a continuous deposition of moisture on the surface of each rain-drop sufficient, in the aggregate, to account for the difference between the quantities of rain received in the higher and lower gauges.

This ingenious explanation has been so ably advocated by its proposer, and by other highly competent judges, that it is not without hesitation I venture to object to it, on the ground that the rate of condensation which it assumes, when compared with the rate deduced from experiment, is too great.

I shall now endeavour to offer some proof in support of this conclusion.

* Vol. ii., p. 401; vol. iii., p. 560; vol. iv., p. 171.

At York, during the winter quarters of the years 1832–33, 1833–34, 1834–35, a rain-gauge placed on the ground received 17·32 inches of rain, and a similar gauge placed at the height of 43·7 feet received only 12·17 inches of rain.

In this case, according to the hypothesis, a rain-drop, while falling through the height of 43·7 feet condensed on its surface a quantity of moisture, the weight of which was to the weight of the drop, before the condensation began, in the ratio of ·42 to 1.

In order to make a fair comparison between this assumed rate of condensation and the rate deduced from experiment, it would be necessary to know the mean temperature of the rain, the mean temperature of the dew-point, the mean size of the drops of rain, and the velocity of their fall.

But as the available data are not sufficient to furnish mean values of these quantities, it is necessary, in order to obviate any objection on this ground to the proposed mode of comparison, to assume for the temperature of the rain, the temperature of the dew-point, and the size of the rain-drops, values the most favourable to the hypothesis which the case admits of.

First, therefore, we are sure that in assuming the temperature of the rain to be 32°, we ascribe to the condensing surface the greatest cold compatible with its being a surface of fluid water.

Again, since the mean temperature of the air during the winter months of the three years of observation was 36°·3, and the mean range of temperature was equal to 8·6 degrees, it follows that the mean maximum temperature of the air was $36{\cdot}3+\frac{8{\cdot}6}{2}=40{\cdot}6$. The temperature of the dew-point, therefore, cannot possibly be assumed as having been higher than 40°·6.

Further, as Sir John Leslie states that the diameters of the drops of rain vary from ·04 to ·25 of an inch; and as it is more favourable to the hypothesis to suppose them small than great, it is proposed to assume that the diameter of each drop was only ·05 of an inch. The weight of a drop of rain of this magnitude is ·0165 of a grain, and the area of its surface is equal to ·00786 of a square inch.

Lastly, since a drop of rain having its diameter equal to ·05 of an inch would soon attain its terminal velocity, it may be assumed that the drops fell with an uniform velocity of 14·6 feet per second, and consequently occupied $\frac{43{\cdot}7}{14{\cdot}6}=3$ seconds in falling through the height of 43·7 feet.

It now remains to ascertain what rate of condensation is deducible from these data, as compared with the rate assumed by the hypothesis.

In order to do this, let $f''=\cdot27$, the tension of vapour at 40°·6; $f'''=\cdot2$, the tension at 32°; $m=39{\cdot}7$, the coefficient due to a current of air having a velocity of 14·8 feet per second, which is nearly the same as the terminal velocity with which the drops of rain have been supposed to fall.

Next, let these values be substituted in the formula $c=m(f''-f''')$, which then becomes $c=39{\cdot}7\,({\cdot}27-{\cdot}2)=2{\cdot}779$ grains.

But as c represents the weight of moisture which would be condensed on a surface of 100 square inches in 60 seconds, and we require to know the quantity which would be condensed on a surface equal to ·00786 of a square inch in the lapse of three seconds, it will be necessary, in order to obtain this latter result, to multiply 2·779 grains, the value of c, by $\frac{{\cdot}00786}{100}\times\frac{3}{60}$, which gives ·000011 of a grain as the weight of moisture condensed on the surface of the rain-drop, while falling through 43·7 feet.

Now, as the original weight of the drop was ·0165 of a grain, it follows that the weight of moisture condensed on the drop was to the weight of the drop itself in the ratio of ·000011 to ·0165, that is, in the ratio of ·00066 to 1.

But, as was before shewn, the hypothesis assumes the ratio of these quantities to be that of ·42 to 1.

Hence, unless there be some fallacy in the mode of arriving at the conclusion, the rate of condensation assumed by the hypothesis is $\frac{{\cdot}42}{{\cdot}00066}=635$ times greater than the rate deduced from experiment.

Table I.

Date and Hour.	No. of Experiment.	Duration of Experiment—minutes.	Barometer.	Temperature of the air during Experiment.	Indication of moist-bulb thermometer.	Temperature of the dew-point.	Tension of vapour at the dew-point.	Difference between the tension of vapour at the dew-point and 32°.	Difference between the temperature of the air and of the condensing surface.	Weight of the copper vessel when dry.	Difference between the weight of the copper vessel before and after the condensation of moisture.						Remarks.
											In calm air, mouth of vessel upwards.	In calm air, mouth of vessel downwards.	In a current of 2·8 miles per hour, or 4·12 feet per 1″.	5·6 miles per hour, or 8·24 feet per 1″.	10 miles per hour, or 14·6 feet per 1″.	14 miles per hour, or 20·6 feet per 1″.	
										Grains.							
1849. Dec. 15,	...	...	29·468	...	...	...	...	...	...	...	...	...	...	...	...	...	
11 17 A.M.,	1	5	...	51·0	47·9	44·7	·312	·112	...	803·90†	...	..	...	...	...	2·60	
11 42 ...	2	5	...	51·5	47·9	44·2	·306	·106	...	803·89	...	...	...	...	...	2·62	
12 13 P.M.,	3	5	...	51·5	48·2	44·8	·313	·113	...	803·89	...	...	...	...	...	2·88	
12 39 ...	4	5	...	51·4	48·1	44·7	·312	·112	...	803·88	...	...	...	..	...	2·35	
1 10 ...	5	5	...	51·5	48·2	44·8	·313	·113	...	803·87	...	...	...	...	...	2·36	
1 35 ...	6	5	...	51·4	48·3	45·2	·316	·116	...	803·87	...	...	...	...	...	2·78	
1 58 ...	7	5	...	51·5	48·3	45·1	·316	·116	...	803·86	...	...	...	...	...	2·81	
2 24 ...	8	5	...	51·4	48·1	44·7	·312	·112	...	803·85	...	...	...	...	2·51	...	
2 48 ...	9	5	...	51·4	48·3	45·2	·316	·116	...	803·85	...	...	...	...	2·53	...	
3 13 ..	10	5	...	51·4	48·3	45·2	·316	·116	...	803·84†	...	...	...	...	2·56	...	
1849. Dec. 17,	...	...	29·168	...	...	...	...	...	...	...	...	...	...	...	...	...	
10 44 A.M.,	11	10	...	52·4	49·5	46·7	·334	·134	...	803·78†	0·05	...	...	...	...	...	
11 34 ...	12	5	...	52·4	49·5	46·7	·334	·134	20·4	803·77	...	0·23	...	...	...	...	
11 58 ...	13	5	...	52·8	49·9	47·2	·338	·138	20·8	803·76	...	0·17	...	...	...	...	
12 29 P.M.,	14	5	...	53·5	50·1	46·9	·336	·136	21·5	803·76	...	0·22	...	...	...	..	
12 48 ..	15	5	...	53·1	50·1	47·3	·340	·140	21·1	803·75	...	0·21	...	...	...	...	
1 10 ...	16	5	...	53·0	50·0	47·2	·339	·139	...	803·74	0·06	...	...	...	...	...	
1 32 ...	17	5	...	52·9	50·1	47·5	·343	·143	...	803·73	...	...	1·60	...	...	...	
1 55 ...	18	5	...	52·9	50·1	47·5	·343	·143	...	803·72	...	...	1·65	...	...	...	
2 15 ...	19	5	...	52·7	49·9	47·2	·339	·139	...	803·72	...	...	1·28	..	...	...	
2 38 ...	20	5	...	52·5	49·7	47·1	·338	·138	...	803·71	...	...	1·32	...	...	...	
3 11 ...	21	5	...	52·6	49·7	46·9	·336	·136	...	803·70	...	...	1·27	...	...	...	
1850. Feb. 1,	...	...	29·122	...	...	...	...	...	...	...	...	...	...	...	...	...	
11 40 A.M.,	22	5	...	50·1	47·4	44·6	·311	·111	...	803·70†	...	...	1·07	...	...	...	
12 5 P.M.,	23	5	...	50·0	47·4	44·8	·312	·112	...	803·69	...	...	1·27	...	...	...	
12 30 ...	24	5	...	50·0	47·6	45·1	·316	·116	...	803·69	...	...	1·27	...	...	...	
1 0 ...	25	5	...	50·2	47·5	44·7	·312	·112	...	803·68	...	...	...	...	2·82	...	
1 24 ...	26	5	...	50·4	47·4	44·4	·308	·108	...	803·67	...	...	...	...	2·49	...	
1 44 ...	27	5	...	50·1	47·4	44·6	·311	·111	...	803·67	...	...	...	...	2·94	...	
2 8 ...	28	5	...	49·9	47·4	44·9	·313	·113	...	803·66	...	...	...	...	2·83	...	
2 25 ...	29	5	..	49·9	47·5	45·0	·315	·115	...	803·65	...	...	...	...	2·71	...	
2 48 ...	30	5	...	50·0	47·4	44·8	·312	·112	...	803·65	...	...	...	...	...	3·19	
3 8 ...	31	5	...	50·0	47·4	44·8	·312	·112	...	803·64	...	...	...	...	...	3·43	
3 29 ...	32	5	...	50·2	47·6	44·9	·314	·114	...	803·64	...	...	...	...	...	3·32	
3 47 ...	33	5	...	50·2	47·6	44·9	·314	·114	...	803·63†	...	...	...	...	...	3·09	
1850. Feb. 19,	..	...	29·470	..	...	...	..	...	...	...	...	...	...	...	...	...	The greatness of the result in experiment 34 was probably owing to an imperceptible current.
10 50 A.M.,	34	5	...	54·3	50·2	46·3	·328	·128	...	803·63†	0·14	...	...	...	...	...	
11 17 ...	35	5	...	54·3	50·2	46·3	·328	·128	...	803·62	0·03	...	...	...	...	...	
11 43 ...	36	5	..	54·5	50·5	46·7	·334	·134	...	803·62	0·06	...	...	...	...	...	
12 12 P.M.	37	5	...	54·6	50·7	47·2	·338	·138	22·6	803·61	...	0·25	...	...	...	...	
12 44 ...	38	5	...	54·5	50·4	46·5	·331	·131	22·5	803·61	...	0·26	...	...	...	...	
1 20 ...	39	5	...	54·6	50·6	46·8	·335	·135	22·6	803·60	...	0·26	..	...	...	...	
1 45 ...	40	5	...	54·6	50·6	46·8	·335	·135	...	803·60	...	...	...	2·04	...	...	
2 11 ...	41	5	...	55·1	50·8	46·7	·334	·134	...	803·59	...	...	...	1·78	...	...	
2 39 ...	42	5	...	54·8	50·8	47·2	·338	·138	...	803·59	...	...	...	2·08	...	...	
3 3 ..	43	5	...	54·7	50·6	46·7	·334	·134	...	803·58	...	...	...	1·89	...	...	
3 30 ...	44	5	...	54·6	50·6	46·8	·335	·135	...	803·57	...	...	...	1·97	...	...	
3 53 ...	45	5	...	54·5	50·6	46·9	·336	·136	...	803·57†	...	...	...	...	...	3·47	
1850. Mar. 14,	...	...	30·385	...	...	...	...	...	...	...	...	...	...	...	...	...	
10 38 A.M.,	46	5	...	56·6	51·1	45·8	·324	·124	24·6	803·56†	...	0·18	..	...	...	...	
10 59 ...	47	5	...	56·7	51·2	45·9	·325	·125	24·7	803·55	...	0·14	...	...	...	...	
11 24 ...	48	5	...	56·8	51·3	46·0	·326	·126	24·8	803·54	...	0·16	...	...	...	...	
11 44 ...	49	5	...	56·8	51·3	46·0	·326	·126	24·8	803·53	...	0·16	...	...	...	...	
12 3 P.M.,	...	...	...	...	...	...	...	...	...	803·52	...	...	...	...	...	...	The experiment spoiled.
12 15 ...	50	5	...	56·8	51·3	46·0	·326	·126	...	803·51	0·06	...	...	...	...	...	
12 35 ...	51	5	...	56·8	51·5	46·5	·331	·131	...	803·49	0·07	...	...	...	...	...	
1850. Aug. 14,	...	...	29·844	...	...	...	...	...	...	803·48†	...	...	...	...	...	...	The condensing vessel repaired.
10 19 A.M.,	...	...	...	...	...	...	...	..	..	792·40†	...	...	...	...	...	...	The experiment spoiled.
10 28 ...	52	5	...	67·3	60·4	55·6	·451	·251	35·3	792·39	...	0·71	...	...	...	...	
10 52 ...	53	5	...	67·4	60·5	55·7	·452	·252	35·4	792·38	...	0·72	...	...	...	...	
11 12 ...	54	5	...	67·5	60·6	55·8	·455	·255	35·5	792·37	...	0·67	...	...	...	...	
11 37 ..	55	5	...	67·7	60·7	55·8	·455	·255	35·7	792·36	...	0·70	...	...	...	...	
11 59 ...	56	5	...	67·8	60·8	56·0	·458	·258	...	792·35	...	...	7·11	...	...	...	It is evident that in the 56th experiment some extraneous water must have fallen on the condensing surface.
12 25 P.M.,	57	5	...	67·9	60·9	56·1	·459	·259	...	792·34	...	...	3·02	...	...	...	
12 47 ...	58	5	...	68·1	61·0	56·2	·460	·260	...	792·33	...	...	3·17	...	...	...	
1 15 ...	59	5	...	68·3	61·3	56·4	·465	·265	...	792·33	...	...	3·17	...	...	...	
1 39 ...	60	5	...	68·6	61·6	56·8	·471	·271	...	792·32	...	...	...	4·98	...	...	
2 7 ...	61	5	...	68·7	61·7	57·1	·474	·274	...	792·31	...	...	...	4·39	...	...	
2 28 ...	62	5	...	68·8	61·8	57·2	·476	·276	...	792·31	...	...	...	5·07	...	...	
2 53 ...	63	5	...	69·0	61·9	57·2	·476	·276	...	792·30	...	...	...	...	...	7·40	
....	...	...	...	...	...	...	...	...	...	792·30†	...	...	...	...	...	...	

Remarks on Table I.

The experiments, the results of which are contained in this table, were all consecutive. No experiment was rejected after what appeared to be a satisfactory mode of experimenting had once been adopted.

The indications of the barometer in the fourth column are reduced to 32° Fahr.

The indications of the dry and wet bulb thermometers recorded in the fifth and sixth columns, are the means of observations made during the intervals of the experiments. As the moist-bulb thermometer was observed in calm air, it was necessary to apply to its indication a subtractive correction, the amount of which was determined by ascertaining, at the end of each day's experiments, how much the moist-bulb was cooled by fanning it for a short time.

The tenth column exhibits the weight of the condensing vessel with its lid. It was found that the friction used in drying the vessel reduced its weight by about ·01 of a grain at each experiment. In order to make the suitable correction on this account, the vessel was weighed at the beginning and end of each day's experiments, and the loss of weight was equally distributed over the intervening experiments. The numbers marked with a cross thus †, express the weights which were actually determined.

Table II.

No. of Experiment in preceding Table.	Values of M in calm air, when the condensing surface is downwards.	Values of m in a current of 2·8 miles an hour, or 4·12 feet per 1″.	Values of m in a current of 5·6 miles an hour, or 8·24 feet per 1″.	Values of m in a current of 10 miles an hour, or 14·8 feet per 1″.	Values of m in a current of 14 miles an hour, or 20·6 feet per 1″.	Remarks.
1	...	...	...	...	23·21	The experiments contained in the twelfth column of the first Table are omitted, as affording no definite results. The fifty-sixth experiment is also omitted, having obviously been spoiled.
2	...	...	.	...	24·72	
3	...	. .	...	..	25·49	
6	...	...	...	...	23·97	
7	...	...	...	...	24·23	
8	...	..	...	22·41	...	
9	..	...	...	21·81	...	
10	...	...	...	20·07	...	
12	·084	...	...	..	...	
13	·059	...	...	...	...	
14	·075	.	...	...	...	
15	·071	...	...	...	...	
17	...	11·19	...	...	...	
18	..	11·54	...	...	...	
19	.	9·21	...	...	...	
20	..	9·56	...	...	...	
21	...	9·34	...	...	...	
22	.	9·64	...	...	...	
23	...	11·34	...	...	...	
24	...	10·95	...	...	.	
25	...	...	...	25·18	...	
26	...	...	...	23·05	...	
27	.	...	...	26·48	...	
28	...	...	...	25·04	..	
29	...	...	...	23·56	...	
30	...	...	...	...	28·47	
31	...	...	...	...	30·62	
32	..	...	...	...	29·12	
33	...	...	...	...	27·10	
37	·082	...	...	...	...	
38	·088	...	...	...	...	
39	·085	...	...	...	...	
40	...	...	15·11	...	...	
41	...	...	13·28	...	..	
42	...	...	15·07	...	...	
43	...	...	14·10	. .	...	
44	...	...	14·70	...	...	
45	..	...	...	..	25·51	
46	·059	...	. .	...	...	
47	·045	...	...	...	...	
48	·051	..	...	...	...	
49	·051	...	...	...	...	
52	·080	..	...	...	...	
53	·089	. .	...	...	...	
54	·074	...	...	..	...	
55	·077	...	...	...	...	
57	...	11·66	...	...	...	
58	..	12·19	...	...	...	
59	...	11·96	...	...	...	
60	...	...	18·38	...	...	
61	...	...	16·02	...	...	
62	...	...	18·37	. .	...	
63	...	...	...	...	26·81	
	0·07	10·78	15·63	23·45	26·30	The average values of M and m for a surface of 11·8 square inches and five minutes of time.
	0·12	18·3	26·5	39·7	44·6	The average values of M and m for a surface of 100 square inches and one minute of time.

NATURAL HISTORY

MARINE INVERTEBRATA.

NATURAL HISTORY

XVIII.—*On some remarkable Marine Invertebrata new to the British Seas.* By Edward Forbes, F.R.S., F.L.S., Professor of Botany, King's College, London: and J. Goodsir, F.R.S.S.L. and E., Professor of Anatomy, University of Edinburgh.

(Read 20th January and 3d February 1851.)

The animals, either wholly new, or new to Britain, described in the following communication, were taken during a yachting cruise, with our indefatigable friend Mr Macandrew, among the Hebrides, in the month of August 1850. During this voyage, which lasted three weeks, a series of observations were conducted by means of the dredge and the towing-net. Not a single new form of testaceous mollusk was procured; our exertions were amply rewarded, however, by the discovery of several remarkable Ascidians and Radiata, some of them so curious in themselves, and so important in their zoological bearings, that we have thought it desirable to lay an account of their characters and anatomy before the Royal Society of Edinburgh.

The most remarkable of them is the largest of compound Ascidians yet discovered in the Atlantic. Its nearest described ally is the genus *Diazona* of Savigny, between which animal and *Clavellina* it constitutes a link; one of considerable zoological importance, since it binds together more closely the *truly compound Ascidians* or *Botryllidæ*, with the *social Ascidians* or *Clavellinidæ*, which latter in their turn pass into the family of *Ascidiadæ*, through the anomalous *Cynthia aggregata*. The discovery of a creature thus filling up a gap in the animal series, was of itself a sufficient harvest from our autumn tour; in this instance our pleasure was enhanced by the beauty and singularity, as well as novelty, of the remarkable animal we have first to describe.

The Syntethys, for so we propose to designate the Ascidian, presents itself in the form of a compact gelatinous mass of half a foot, and sometimes more in diameter, and very nearly an equal height. It is affixed to the rock or stone by a short slightly spreading base of various breadth, whence rises as an inverted pyramid the body of the mass, irregularly circular and slightly lobed, spreading out at its summit. It is of a translucent apple-green hue; the surface is nearly smooth. The whole of the expanded disk is thickly studded with individual ascidians growing out, as it were, from the common mass. They are arranged in irregular rows, with a tendency to concentric order. Each individual measures, when full grown, nearly two inches in length, and has the shape of an elongated ampulla, with two terminal orifices, set well apart, but not very prominent, and

nearly on the same level. The outer tunic is a smooth and transparent softly cartilaginous sac of a pale emerald green tint, slightly swelling out above the centre, and contracted, but not pedunculated at the base. The inner tunic is clearly seen through the walls of the outer; it is rather less in dimensions than the outer, and its shape is plainly indicated by the opaque white lines which mark its boundaries. The orifices of the outer tunic are both quite plain; the branchial one is rather longer than the anal, as is also the case with the openings of the inner tunic. The branchial orifice of the latter is fringed with a circle of pointed tentacula more than twelve in number; its anal orifice is at the end of a short tube, and has no tentacula, but six conspicuous white ocelli. Beneath the branchial orifice are two crescentic white lines, at the summit of a single white line which runs down the branchial side of the body; under the anal orifice there is a short oblique central white line running from the neighbourhood of a large ganglion to the summit of two white lines uniting in a loop at the point of junction, and running down the visceral side of the body. The chief visceral mass is seen at the base of this line imbedded in the common pedicle.

When the entire mass was first dredged up, many of the tests appeared as if emptied of their contents, or as if the inner tunic and viscera had not become developed. After it had for some time remained at rest in a vessel of sea-water, to our great surprise we found all the sacs filled up again. On closer examination, we found that the inner tunic is exceedingly irritable, and can withdraw itself like the finger of a glove, entirely independent of the outer tunic, and hide itself in the common mass or peduncles. This is done very rapidly sometimes, at other times rather slowly; most rapidly when the ganglionic mass between the orifices is pinched or otherwise irritated. When we squeezed it with the forceps, the withdrawal of the common branchial sacs was almost instantaneous.

The genus *Syntethys* differs from *Diazona* in the structure of the branchial and anal orifices, which, instead of being six-rayed, as in the latter genus, are simple and even-edged as in *Clavelina;* moreover, instead of having a pedunculated, it has a sessile abdomen. The structure and form of the common mass is similar, making a strong distinction between it and *Clavelina*. The following summary of the characters of *Syntethys* will serve to compare them with those of the genera described by Savigny.

Common mass sessile, gelatinous, forming a single orbicular system. *Individuals* very prominent, arranged subconcentrically. *Branchial* and *anal* orifices simple, and not cut into rays.

Thorax oblong and cylindrical. *Branchial chamber* with thirteen transverse rows of oblong openings, fringed with ciliated epithelium; hooked fleshy tubercles at the intersections of the branchial meshes, each mesh presenting one of the ciliated openings; the tubercles give the internal surface of the chamber a dotted appearance.

Œsophagus elongated, situated on the left side. Stomach cubical, spongy, or glandular. Intestinal loop large and open, reaching to the bottom of the muscular tunic; its ascending portion glandular, probably hepatic; the rectum passes from the ventral to the right side of the œsophagus; the anus is on the dorsal edge of the sac about its middle. The *ovary* is in the loop of the intestine, but was not in season in the specimens taken.

Testis white, ramifying on the surface of the ovary; the *vas deferens* runs up on the œsophagus and rectum to the side of the anus. The *heart* is in the loop of the intestine and ovary.

Sp. *Syntethys hebridicus*.—All the specimens were dredged in thirty fathoms water, close to Croulin Island, near Applecross. The locality in which they occurred is remarkable for the assemblage of boreal mollusca there congregated, so that we may reasonably expect that this extraordinary ascidian will be found hereafter in the Norwegian seas. It is probably a member of the boreal type of the British fauna.

Holothuria intestinalis. Ascan. and Rathke.—From a depth of thirty fathoms in the Minch, and from the same depth off Croulin Island, we dredged a twenty-tentaculated *Holothuria*, undescribed as a member of the British fauna. It has an elongated cylindrical body of a pinkish-grey colour, and very soft in texture of skin. The tentacula are short and orbicular, compactly frondose, and of a dark orange colour. The surface of the body is thickly covered with slender suckers, dilated at their bases, and rather more numerous on the ventral than on the dorsal aspect. By means of these suckers, the animal invests itself with fragments of shells and stones in the manner of *Thyone*. It grows to the length of half a foot. Judging from the description given in the "Ofversigt af Skandinaviens Echinodermer," by Duben and Koren, this appears to be the *Holothuria intestinalis* of Ascanius and Rathke, *H. mollis* of Sars. We have not been able to compare it with the original figures. It constitutes a second British species of *Holothuria* proper; the first being the animal described by Mr Peach, under the name of "Nigger," given to it by the Cornish fishermen.

Sarcodictyon agglomerata. Sp. nov.—Examples of a new species of this curious genus of Asteroid zoophytes were dredged in thirty fathoms water off Croulin Island, and also between Rasa and Scalpa.

Like its congener the *Sarcodictyon catenata*, it invests the surface of stones and shells, and is also found adhering to corallines. It differs essentially, however, in having the polype cells, instead of being arranged in single file, grouped together in assemblages of from three to five, each group connected with its neighbour by a stolon-like extension of the polypidom. The texture of its surface is not so smooth, and the colour invariably ochraceous yellow. The polypes are white, and exactly resemble those of *Sarcodictyon catenata*. Each polype cell measures about two-tenths of an inch across.

In the synopsis of the genera of zoophytes, by Milne Edwards and Haime, the genus *Sarcodictyon* is placed in the family *Cornularinæ*, among the *Alcyonaria*. We can hardly assent to its removal from the immediate neighbourhood of *Alcyonium*, for it differs merely in the stoloniferous method of growth. The new species now announced, goes far to confirm our view of its affinities. This genus may be said to bear much the same relation to *Alcyonium*, which our new genus *Syntethys* (among the Ascidians) bears to *Clavelina*.

Arachnactis albida. Sars.—In the first part of the very beautiful and valuable work by M. Sars, entitled "Fauna Littoralis Norvegiæ," published at Christiania in 1846, a new genus and species of Helianthoid zoophytes is described at length, and figured in detail, under the name of *Arachnactis albida*.

In the "Travels in Lycia," by Professor E. Forbes and Captain Spratt, R.N., published in 1847, a swimming *Actinea* is noticed and figured from the Egean. This was clearly a species of *Arachnactis*.

When Dr Balfour visited the Island of Lewis with his pupils in August 1841, they procured a number of mutilated specimens of a radiate animal found floating in the Minch. These were too imperfect for determination at the time. This year, however, we have discovered a species of *Arachnactis* evidently identical with the Norwegian one in the Minch, and the remains of Dr Balfour's animal have proved identical with it.

The definition of the genus given by Sars is,—"Animal liberum, molle, natans; corpus breviter cylindricum, parvum, basi rotundata, disco suctorio carente; os seriebus tentaculorum non retractilium duabus circumdatum, exterioribus longissimis, interioribus brevibus."

The number of the larger tentacula were eight to ten, of the smaller, according to Sars, twelve; we have observed them as many as sixteen. The shape of the body is pyriform; its colour dusky white, tinged with tawny. The outer tentacula are very long, tawny and white; the inner, much shorter. The length of the body is about one inch. The outer tentacula can be extended to three or four times the length of the body. The creature swims freely, and habitually in the manner of a medusa.

There is a point, however, of consequence which Sars did not observe, *it can convert its posterior extremity into a suctorial disk, and fix itself to bodies in the manner of an Actinea*. Aristotle states in several places in his History of Animal, that the Actinea (ακαλήφη) can detach itself from the rock and swim. Thus, in Book iv. 6, speaking of these animals, he writes,—"προσπέφυκε μεν γαρ ταις πετραις ὥσπερ ἕνια των ὀστρακοδερμων, ἀπολύεται δ' ἐνίοτε." Commentators have supposed that he confounded Actineæ with Medusæ. But he mentions the latter animals under another name. The discovery of the *Arachnactis*, and its abundance in the Grecian seas explain the difficulty, and shew the accuracy of his observations.

Plancia. New Genus.—Umbrella hemispherical; radiating vessels four,

simple; no conspicuous genital glands; two long marginal tentacles, and numerous intermediate rudimentary tubercles, all with ocelli at their bases; stomach at the end of a very long, extensile, cylindrical tubular proboscidiform peduncle, with a simple or obscurely lobed orifice.

We have deemed it necessary to constitute this genus for a very curious little naked-eyed medusa of the family *Sarsiadæ*, so distinct in its characters, as not to be referable to any of the defined groups. We have dedicated it to Janus Plancus, who, in his tract "De Conchis Minus Notis," published in 1739, was the first naturalist who figured a naked-eyed medusa.

Plancia gracilis. Sp. Nov.—Disk hemispherical, depressed, colourless, smooth, its margin furnished with two long tubular tentacula, one on each side opposite the terminations of gastrovascular canals; at the origin of each of these is a small fixed tentacular process, connecting the umbrella with the subumbrella, as in the genus *Steenstrupia*. The remainder of the margin is occupied by about sixty minute tubercles or rudimentary tentacula, beside each of which on an oblong process is a minute black ocellus. Four simple gastrovascular canals, connected with a marginal canal, divide the disk into as many equal segments. The entrance of the cavity is protected by a broad veil. The peduncle is very long and extensile, resembling in shape that of *Sarsia;* it is very acute at its base, and is of a general pink hue, with darker lines, as if of genital glands lining its tube. It is terminated by a short orange-coloured campanulate stomach, opening by an irregularly four-lobed orifice.

This is an active and elegant little creature. Its disk measures rather less than a quarter of an inch across. When swimming, it carries its two tentacula streaming behind it for a great length. We procured several examples in the Sound of Mull and off Staffa.

Oceania ducalis. Sp. Nov.—Umbrella campanulate, subglobose, round above, smooth, colourless, transparent. Subumbrella rather small in proportion, its orifice protected by a conspicuous veil; its margin edged with rose-colour, and bearing 16 ($3 \times 4 + 4$) pinkish tentacula, springing from bulbous bases, each of which is marked by a conspicuous crimson or purple crescentic ocellus: between each pair of tentacles is a minute tubercular process. Down the sides of the subumbrella run the four simple gastrovascular canals, tinged with red. From its centre depends the oblong, massive, reddish-tawny peduncle, in the upper part of which are obscurely seen the convoluted reproductive glands. The orifice of the peduncle is campanulate, and bordered by four slightly-fimbriated lips. The height of the body is less than a quarter of an inch.

It was taken at Tobermory. We had previously met with the same species on the coast of Dorsetshire.

Slabberia catenata.—Hitherto only a single species of the genus *Slabberia*, one of the most curious types of the Medusa Gymnopthalmata, has been met with,

namely, the *S. halterata*, a native of the coasts of Cornwall. We have the pleasure of adding a second and very distinct species, to which the name of *Slabberia catenata* may be applied.

It differs from the former, among other characters, most conspicuously in having its poiser-like marginal tentacula thickened for half their length by a series of rings or bulbs, charged with pigment cells, and ranged in succession above the terminal bulb with a dark nucleus, so characteristic of the genus.

The umbrella is deeply campanulate or subglobular, smooth and colourless. The subumbrella is much less in proportion than in its described ally; it is divided into equal portions by four canals, which open into a central marginal vessel. The very minute linear genital glands can scarcely be traced on the upper part of these vessels. The border of the general cavity is provided with a shelf-like veil. The tentacula are stout, four in number, colourless, and cylindrical in their upper, nodulose and annulated in their lower half. There are five or six ring-like or bulbous thickenings, besides the terminal bulb. Each is of an orange hue, and the lower ones are larger than the upper. The next above the terminal bulb is largest. The terminal bulb is also orange, but has a dark nucleus. The tentacula spring from ocellated bulbs. These are somewhat triangular in shape, pale yellow above, marked across the centre by a band of dark orange, below which, on a pale yellow ground, is the small black ocellus. The peduncle, or stomach, is longer than the tentacula when expanded to its full dimensions. It is highly contractile, and is of a dull olive hue, with indications of darker cylindrical bands. The height of the umbrella was about two-tenths of an inch. This curious medusa was taken off Tobermory, and afterwards near Loch Laigh in Mull.

Hippocrene pyramidata. Sp. Nov.—During our cruise, we had the good fortune to add no fewer than three new and very distinct species to the beautiful and singular genus *Hippocrene* or *Bougainvillia*. They, like their congeners, were all exceedingly minute.

The first of which we name *H. pyramidata*, is distinguished conspicuously by the form of the ovarian lobes of the peduncle; instead of being quadrate, as in all known species, they are triangular, so that the entire peduncle assumes the shape of an inverted pyramid.

The umbrella is transparent, smooth, colourless, and subglobular. The subumbrella is comparatively small and quadrately campanulate; its opening is protected by a four-lobed veil. At the four angles are the groups of connate eye-tubercles. Each group forms an oblong mass, the general colour of which is yellowish below and orange above. From four to six tubercles go to a mass, and the orange-coloured portion is lobed according to their number. On the lobed yellowish part below is a black eye-speck, one to each tubercle. One, or at most two transparent tentacula, were seen to protrude from each of the masses. The

peduncle is pyramidal, and composed of four triangular lobes, corresponding with the four gastrovascular canals. Each lobe is of a tawny-yellow colour, with a dark orange centre, and as it is narrow, the four combined, when seen from above, appear as a small yellow cross, with an inner cross of orange. From the dependant apex of the peduncle hangs a short and narrow colourless stomach. the lips of which are produced into bifurcated tentacular processes of no great length.

Several examples of this animal were taken off Loch Laigh in Mull.

Hippocrene crucifera. Sp. Nov.—This new form of *Hippocrene* differs from all its congeners in the very long genital lobes springing from the peduncle, and running down one-half the length of the canals, so as to remind us of the ovaries of the *Thaumantias.* It was taken off Tobermory.

The umbrella is globular, colourless, and smooth; the subumbrella rather large. The four fascicles of tentacular bulbs are each of an oblong and somewhat crescentic shape, tawny-orange above and colourless below. Each is composed of six bulbs, bearing black ocelli on their pale portions, and corresponding to as many short transparent colourless tentacula. The peduncle is rather short, but its lobes, which are of a tawny-yellow colour, with a double line of orange in their centres, are very long and narrow, somewhat undulated, and prolonged for half the length of the subumbrella, appearing like so many arms. The anal lobes are colourless; they are produced into short and proportionally minute labial tentacula, each of the four presenting a simple bifurcation.

Hippocrene simplex. Sp. Nov.—This species is more nearly allied to the *H. britannica* than the others, and connects that well-known form with *H. nigritella*, but is very distinct from both.

The umbrella is globular, colourless, and smooth; the subumbrella large in proportion. The four fascicles of tentacular bulbs are each oblong, yellow below and orange above; each is composed of four bulbs, and is acutely four-lobed, bearing four black ocelli on as many projections. Only one yellowish tentacle (as in *H. nigritella*) springs from each mass. The peduncle resembles that of *H. britannica*, is quadrate, massive, four-lobed, and of a dull orange hue. The stomach is short and wide, terminating in four colourless labial tentacles, which twice bifurcate. Several specimens were taken at Tobermory.

Thaumantias undulata. Sp. Nov.—When sailing through the Minch on a very warm day, when the sea was very calm, we met with a number of small medusæ, each measuring about an inch and a-half in diameter, and conspicuous in the water, owing to the undulated pink cross which marked their subumbrella. On capturing some, they proved to belong to an undescribed, and very curious form of the genus *Thaumantias.*

The umbrella is hemispherical, smooth, and colourless. Its margin is fringed with very numerous slender coloured tentacula, which are often carried coiled up

spirally. Their formula is $40 \times 4 + 4$. Each of these springs from a bulbous base, bearing a small but distinct black ocellus. Between each pair of tentacula is a minute transparent mobile pedunculated tubercle. Down the four gastrovascular canals, very nearly from their divergence, to the margin of the umbrella, run the four linear genital glands, tinged with rose colour. They are very peculiarly formed, each hanging from the surface of the subumbrella in the shape of a pair of undulated membranous curtains, strikingly reminding us of the appearance presented by *Staurophora* (so well described and figured by Professor Agassiz in his Memoir on the Naked-Eyed Medusæ of Massachusetts), but differing in their nature; for, in the animal we are describing, they are assuredly quite distinct from the stomach-lobes. The stomach is rather large and quadrangularly campanulate, rose coloured, and slightly fimbriated at the margins.

Thaumantias confluens.—To find a new species of the genus *Thaumantias* sufficiently distinct from the numerous and very similar described forms, was scarcely to be looked for. In the one before us, however, we have found such a desideratum.

The *Thaumantias confluens* differs from all its British allies in having the genital glands continued so high up on the gastrovascular canals, that they all meet on the vertex of the umbrella, and form an unbroken cross.

The umbrella is hemispheric, smooth, and colourless. Its margin is fringed with pale pinkish tentacula; the formula of their number being $14 \times 4 + 4$. Their bases bear very minute black ocelli; the intertentacular spaces have minute tubercle-like bodies on each, some of them being shortly pedunculated. The marginal veil is broad. The genital glands are of a pale pink colour, very narrow and linear, confluent at their bases, and continued down the upper third of each of the four gastrovascular canals. When the creature is in the water, they present the appearance of a pink cross. The stomach is very short and narrow, and terminates in four lanceolate acute lips. The disk measures nearly half-an-inch across.

This delicate and pretty animal was met with not unfrequently off Tobermory, and afterwards near Skye.

EXPLANATION OF THE PLATES.

PLATE IX.

Fig. 1. *Holothuria intestinalis*, natural size.

Fig. 2 *a*. *Arachnactis albida*, natural size; and 2 *b*. the same with some of the outer tentacula removed in order to shew the inner circle and the globular form at times assumed by the body.

Fig. 3 *a*. *Sarcodictyon agglomerata*, natural size; 3 *b*. the same magnified; and 3 *c*. the polypes.

Fig. 4 *a*. *Syntethys hebridicus*, less than the natural size; 4 *b*. one of the individual Ascidians of the size of nature; 4 *c*. the same with the inner tunic retracted; 4 *d*. the orifices of the inner and outer tunics much magnified.

PLATE X.

Fig. 1 *a*. *Plancia gracilis*, three times the natural size, seen in profile; 1 *b*. the same, seen from above; 1 *c*. its peduncle; 1 *d*. orifice of the peduncle; 1 *e*. extremity of a tentacle.

Fig. 2 *a*. *Oceania ducalis*, much magnified, and seen in profile; 2 *b*. the peduncle, ovaries, and mouth; 2 *c*. one of its tentacula.

Fig. 3 *a*. *Slabberia catenata*, natural size; 3 *b*. the same, magnified; 3 *c*. its peduncle; 3 *d*. genital glands on the side of a gastro-vascular canal; 3 *e*. one of the tentacula.

Fig. 4 *a*. Natural size of *Hippocrene pyramidata*; 4 *b*. the same, magnified; 4 *c*. as seen from above; 4 *d*. the genital lobes and peduncle; 4 *e*. genital cross; 4 *f*. one of the tentacular fascicles.

Fig. 5 *a*. Natural size of *Hippocrene cruciata*; 5 *b*. lateral view, magnified; 5 *c*. view from above: 5 *d*. genital lobes and peduncle; 5 *e*. one of the tentacular fascicles.

Fig. 6 *a*. Natural size of *Hippocrene simplex*; 6 *b*. lateral view magnified; 6 *c*. the view from below; 6 *d*. one of the tentacular fascicles; 6 *e*. genital lobes and peduncle.

Fig. 7 *a*. *Thaumantias*, natural size; 7 *b*. diagram of same, seen from above; 7 *c*. section shewing the genital folds and stomach; 7 *d*. margin and tentacula.

Fig. 8 *a*. *Thaumantias confluens*, side view magnified; 8 *b*. diagram of view from above; 8 *c*. peduncle and buccal lobes; 8 *d*. margin and tentacula.

XIX.—*On the Total Intensity of Interfering Light.* By Professor Stokes.

[*Extracted from a Letter addressed to Professor Kelland.*]

Pembroke College, Cambridge.

My dear Sir,

* * * *

In reading your paper in the Transactions of the Royal Society of Edinburgh, vol. xv., p. 315, some years ago, it occurred to me to try whether it would not be possible to give a general demonstration of the theorem, applying to apertures of all forms. I arrived at a proof, which I wrote out, but have never published. As I think it will interest you I will communicate it. You may make any use you please of it.

Case I. Aperture in front of a lens; light thrown on a screen at the focus, or received through an eye-piece, through which the luminous point is seen in focus.

The expression for the intensity is given in Airy's Tract, Prop. 20. If the intensity of the incident light at the distance of the aperture be taken for unity, and D be the quantity by which any element of the area of the aperture must be divided in forming the expression for the vibration, that expression becomes

$$\frac{1}{D}\iint \sin\frac{2\pi}{\lambda}\left(vt - B + \frac{px+qy}{b}\right)dx\,dy,$$

the integration being extended over the whole aperture. If it should be necessary to suppose a change of phase to take place in the act of diffraction, such change may be included in the constant B. If, then, I be the intensity,

$$D^2 I = \left(\iint \sin\frac{2\pi}{\lambda}\frac{px+qy}{b}dx\,dy\right)^2 + \left(\iint \cos\frac{2\pi}{\lambda}\frac{px+qy}{b}dx\,dy\right)^2;$$

and if $\bar{I}$ be the total illumination,

$$\bar{I} = \int_{-\infty}^{\infty}\int_{-\infty}^{\infty} I\,dp\,dq.$$

Now, $$\left\{\iint f(x,y)\,dx\,dy\right\}^2 = \iiiint f(x,y)f(x',y')\,dx\,dy\,dx'\,dy',$$

the limits of x', y' being the same as those of x, y. Hence,

$$D^2 I = \iiiint \cos\frac{2\pi}{b\lambda}\left(p\overline{x'-x} + q\overline{y'-y}\right)dx\,dy\,dx'\,dy'.$$

In the present shape of the integral, we must reserve the integration with respect to p and q till the end; but if we introduce the factor $\epsilon^{\mp\alpha p \mp\beta q}$, where the sign $-$ or $+$ is supposed to be taken according as p or q is positive or negative, we shall evidently arrive at the same result as before, provided we suppose in the end α and β to vanish. When this factor is introduced, we may, if we please, integrate with respect to p and q first. We thus get

$$\mathrm{D}^2\,\overline{\mathrm{I}} = \text{limit of} \iiiiiint \epsilon^{\mp\alpha p \mp\beta q} \cos\frac{2\pi}{b\lambda}\left(p\overline{x'-x} + q\overline{y'-y}\right) dx\,dy\,dx'\,dy'\,dp\,dq.$$

Now,
$$\int_{-\infty}^{\infty} \epsilon^{\mp\alpha p}\cos(kp-\mathrm{Q})\,dp = \cos\mathrm{Q}\int_{-\infty}^{\infty}\epsilon^{\mp\alpha p}\cos kp\,dp + \sin\mathrm{Q}\int_{-\infty}^{\infty}\epsilon^{\mp\alpha p}\sin kp\,dp$$
$$= 2\cos\mathrm{Q}\int_0^{\infty}\epsilon^{-\alpha p}\cos kp\,dp = \frac{2\alpha\cos\mathrm{Q}}{\alpha^2+k^2}.$$

A similar formula holds good for q, whence

$$\mathrm{D}^2\,\overline{\mathrm{I}} = \text{limit of}\iiiint \frac{4\alpha\beta}{\left\{\alpha^2+\left(\frac{2\pi(x'-x)}{b\lambda}\right)^2\right\}\left\{\beta^2+\left(\frac{2\pi(y'-y)}{b\lambda}\right)^2\right\}} dx\,dy\,dx'\,dy'.$$

Let now
$$\frac{2\pi(x'-x)}{b\lambda} = \alpha u, \text{ whence } dx' = \frac{b\lambda\alpha}{2\pi}du,$$

and the limits of u are ultimately $-\infty$ and $+\infty$, since α ultimately vanishes. Hence

$$\text{limit of}\int \frac{2\alpha\,dx'}{\alpha^2+\left(\frac{2\pi(x'-x)}{b\lambda}\right)^2} = \frac{b\lambda}{\pi}\int_{-\infty}^{\infty}\frac{du}{1+u^2} = b\lambda.$$

A similar formula holds good for y', and we have, therefore,

$$\mathrm{D}^2\,\overline{\mathrm{I}} = b^2\lambda^2\iint dx\,dy = b^2\lambda^2\mathrm{A},$$

if A be the whole area of the aperture or apertures.

Now $\overline{\mathrm{I}}$ ought to be equal to A, and, therefore,

$$\mathrm{D} = b\lambda.$$

Case II. Aperture in front of a screen.

The formula for the illumination is given in Airy's Tract, Art. 73. We have as before,

$$\mathrm{D}^2\,\overline{\mathrm{I}} = \text{limit of}\iiiiiint \epsilon^{\mp\alpha p\mp\beta q}\cos\frac{\pi(a+b)}{\lambda a b}\left\{\left(x'-\frac{ap}{a+b}\right)^2 - \left(x-\frac{ap}{a+b}\right)^2 + \left(y'-\frac{aq}{a+b}\right)^2 - \left(y-\frac{aq}{a+b}\right)^2\right\} dx\,dy\,dx'\,dy'\,dp\,dq$$
$$= 1 \text{ imit of}\iiiiiint \epsilon^{\mp\alpha p\mp\beta q}\cos\left\{\frac{\pi(a+b)}{\lambda a b}[x'^2-x^2+y'^2-y^2]\right.$$

$$-\frac{2\pi p}{\lambda b}(x'-x)-\frac{2\pi q}{\lambda b}(y'-y)\Big\}\,dx\,dy\,dx'\,dy'\,dp\,dq$$

$$=\text{limit of}\iiiint\frac{2\alpha}{\alpha^2+\left(\frac{2\pi(x'-x)}{\lambda b}\right)^2}\cdot\frac{2\beta}{\beta^2+\left(\frac{2\pi(y'-y)}{\lambda b}\right)^2}\cdot$$

$$\cos\frac{\pi(a+b)}{\lambda a b}(x'^2-x^2+y'^2-y^2)\,dx\,dy\,dx'\,dy'.$$

Now, when α vanishes, the whole of the integral

$$\int_{-\infty}^{\infty}\frac{2\alpha\,dx'}{\alpha^2+\left(\frac{2\pi(x'-x)}{\lambda b}\right)^2}$$

is ultimately comprised between limits for which x' is infinitely close to x, and similarly with respect to y'; so that ultimately

$$\cos\frac{\pi(a+b)}{\lambda a b}(x'^2-x^2+y'^2-y^2)=1$$

within the limits for which the quantity under the integral sign does not vanish. Hence, passing to the limit, we get

$$D^2\,\overline{I}=\lambda^2 b^2\iint dx\,dy=\lambda^2 b^2 A, \text{ as before.}$$

Case III. Everything the same as in Case II., except that the phase of vibration is retarded by ρ, where ρ is some function of x and y.

This case is very general. It includes, as particular cases, those numbered I. and II. The experiment with Fresnel's mirrors or a flat prism is also included as a particular case.*

From what precedes, it is plain that we should have in this case

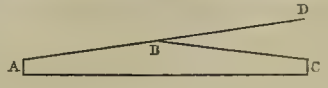
$$D^2\,\overline{I}=\text{limit of}\iiiint\frac{2\alpha}{\alpha^2+\left(\frac{2\pi(x'-x)}{\lambda b}\right)^2}\cdot\frac{2\beta}{\beta^2+\left(\frac{2\pi(y'-y)}{\lambda b}\right)^2}$$

$$\cos\left\{\frac{\pi(a+b)}{\lambda a b}[x'^2-x^2+y'^2-y^2]-\rho'+\rho\right\}dx\,dy\,dx'\,dy',$$

where ρ' is the same function of x' and y' that ρ is of x and y. The same reasoning as before leads to the same result.

I do not regard the preceding demonstration of a result which you were the first to announce, as of any physical interest after what you have yourself done. Still it may not seem wholly uninteresting, in an analytical point of view, to demonstrate the proposition for any form of aperture.

* Thus, in the case of the flat prism, if P, Q be the virtual images corresponding to the halves A B, B C, if we produce A B to D, we may suppose the light which falls on B C, instead of coming from Q, to come from P, and to have been accelerated by the passage through the wedge D B C of air instead of the same wedge of glass.



Of course, by comparing the result $\lambda^2 b^2 A$ with that obtained, in particular cases, by integrating in the straightforward way, we may arrive at the values of various definite integrals.

I am, dear Sir,

Yours very truly,

G. G. Stokes.

XX.—*Some Observations on the Charr* (Salmo umbla), *relating chiefly to its Generation and Early Stage of Life.* By JOHN DAVY, M.D., F.R.SS., L. & E., Inspector-General of Army Hospitals, &c.

(Read 15th March 1852.)

The natural history of the Charr, especially as regards its generation and the early period of its life, is admitted to be very defective, partly, no doubt, arising from the peculiar habits of the fish withdrawing it from observation, and in part, and more, to the circumstance that it is comparatively of rare occurrence, being found only in a limited number of the deepest lakes of this country, and, with few exceptions, seldom taken by the angler, and consequently a good deal removed from the notice of the naturalist.

Residing for several years in the neighbourhood of Windermere,—a lake in which this fish, though decreasing in number, is still pretty abundant,—I endeavoured to collect information respecting its breeding, the time required for the hatching of its ova, and the peculiarities of the young fish after its exclusion, but in a great measure in vain. The fishermen of the lake were acquainted with its spawning season and the spawning localities; but none of them had ever seen a young charr after its quitting the egg, nor till it had attained a notable size.

Artificial breeding—that process of fecundation which was first tried by Count GOLSTEIN in the middle of the last century, and has since been so successfully employed both in propagating some of the more valuable species of the Salmonidæ, and in illustrating their history—occurred to me as the only likely means of affording the information desired.

About the same time, viz., in the autumn of 1850, a gentleman, MORRIS REYNOLDS, Esq., living near the lake,—through whose garden a small stream of good water descends from the hill above, very favourably circumstanced for carrying on the process of artificial breeding,—commenced the attempt, after the manner recommended by JACOBI. This process is now so well known as hardly to require description. I may briefly mention, that two wooden boxes, communicating, were used, through which a small current of water was allowed to pass by a grating of perforated zinc, over a bed of gravel laid on the bottom of each compartment. In these boxes the roe of the fish, for trial, after admixture with the fluid milt, was deposited, each obtained from individuals in the act of spawning, or mature for that act, as denoted by both the roe and milt being yielded under gentle

pressure applied to the abdomen, soon after the fish were taken from the water,—the roe in detached ova, the milt in the state of a milk-like fluid.

It was from these boxes that I obtained, through the kindness of their proprietor, most of the subjects of the following observations; and to him, too, I was indebted for exact particulars, without which the observations would have been almost valueless.

1. *Of the Roe and Milt of the Charr.*

The ova of the charr, at their full time, that is, when they are detached from their ovaries, and are loose in the cavity of the abdomen, ready for expulsion, are, like those of the other Salmonidæ, almost, if not quite, spherical. Those I have examined, I have found to vary in diameter from ·16 to ·18 and ·20 of an inch; and in weight (after the removal of adhering moisture by wiping) from ·7 grain to 1 grain each. Their colour is a light yellow, lighter than that of the ova of the salmon or lake trout with which I have compared them, and thus distinguishable, as well as by their somewhat smaller size. The matter of which they consist may be described as an almost colourless, transparent, viscid fluid, containing suspended in it very many oil globules of various sizes, hardly distinguishable without the aid of the microscope, of a yellow colour, to which the colour of the egg is principally owing. This matter may be considered as corresponding to the yolk of the egg of the bird: it is more than doubtful that the ova of the charr have any part corresponding to the albumen of the bird's egg. The matter of the charr's egg, I may remark, like that of the ova of the Salmonidæ generally, is peculiar in some of its properties; being coagulable on admixture with water, as I believe was first pointed out by M. Vogt, in the instance of the Coregonus of the Lake of Neuchâtel,*—in being, as I have found, not coagulable by heat, even at a temperature of 212 Fahr., if water be excluded,—in being, after coagulation by water, soluble in a solution of common salt and in other saline solutions, and also in such of the vegetable acids as were tried, for instance, the tartaric, acetic, oxalic, and citric. For a fuller account of these experiments, I may refer to a paper expressly on this subject, which has been communicated to the Royal Society of London, the results of which would seem to show that the substance of the egg of the Salmonidæ may be viewed as a distinct species of albumen,—as much so, perhaps, as the coagulable lymph of the blood compared with the serum of that fluid.

The shell of the egg of the charr may be briefly noticed. Nearly transparent and colourless, it is of considerable strength, and until thinned and weakened in the process of hatching, is not easily ruptured. Five emptied of their contents, but not deprived of their moisture by drying, weighed one-tenth of a grain; tho-

* See "Embryologie des Salmones. Par C. Vogt." Neuchâtel, 1842, 4to, p. 11.

roughly dried, so as to expel this moisture, they were reduced from ·10 to ·07 of a grain, thereby denoting a large proportion of solid matter, viz., 70 per cent.

Whether this shell in its sound state, before putrefaction has commenced, is pervious to water, seems to me questionable; and also, whether the internal vitelline membrane, after fecundation, is altogether impermeable by it. M. Vogt holds that the shell is at all times so permeable, but the vitelline membrane, after impregnation, never, so long as the ovum retains its vitality; losing which, the membrane, he infers, no longer resists the transmission of water, and the coagulation of the fluid yolk takes place as an unavoidable consequence. I might assign reasons for the doubts I venture to entertain on these points; but not sure that they would be considered satisfactory, or that the points themselves, though not without interest, require here to be discussed, I shall avoid bringing them forward. That the death of the impregnated ovum, as pointed out by M. Vogt, is clearly indicated by the coagulation of the yolk, from the penetration of water into its substance, is certain. But there is another indication of the event, and not less certain, viz., the adherence of the lighter oil globules to the vitelline membrane, preventing thereby their change of place with a change of position of the ovum, and that tendency to ascend in the heavier yolk fluid which is observable whilst vitality lasts, and which may perhaps be considered as a characteristic of it. The adhesion of the oil globules alluded to, not unfrequently takes place in eggs which retain their transparency. In no instance have I observed any traces of fœtal development after these have become fixed, or, if commenced, any further progress. Why these ova do not become opaque, why their membranes should remain impervious to water, I am ignorant; but that they are so, must be inferred from the circumstance, that when ruptured, and their contents mixed with water, coagulation is immediately effected.

Relative to the milt or spermatic fluid of the charr, I have but few observations to offer, the examination I have hitherto made of it not having been minute, except very partially. Like that of the Salmonidæ generally, in its mature state when ready to be shed, it is a milk-like fluid, slightly viscid, heavier than water, and containing, diffused through it (the cause of its milkiness) a vast number of granules (spermatozoa). These minute bodies are nearly spherical in form, are about $\frac{1}{10,000}$th of an inch in diameter, and seem to move spontaneously, as seen under the microscope, for a short time after the expulsion of the fluid from the live fish. Though they are of greater specific gravity than water, yet, owing to their minuteness, they are easily diffused and suspended in this fluid. After a rest of two hours, water rendered turbid by the addition of a small quantity of spermatic fluid had not become clear, even towards its surface. A drop placed under the microscope was found to abound in spermatozoa. Another property of the spermatic fluid, not unworthy of mention, is the remarkable manner in which it resists putrefaction. Whether the spermatozoa are capable or not of impreg-

nating the ova after they have lost their power of spontaneous motion, I cannot offer any decided opinion; from the few trials I have made, I am led to believe that the one quality or power is distinctive of the other, and that, ceasing to move, they become inert.

In a charr weighing about half a pound I have found the number of ova to be 1230, all nearly of full size. As the volume of the mature and distended testes is about the same as that of the ripe ovaries, the number of spermatozoa belonging to them must almost baffle calculation; and if, as there is reason to believe, a single one may suffice to impregnate an ovum, the whole from one male may, it is presumed, be more than adequate to effect the impregnation of the entire eggs of many females, especially taking into account how readily these minute bodies are suspended and diffused in water.

2. Of the time required for the hatching of the Ova; and of the young Charr in their early stage.

The principal spawning season of the charr in the several lakes of the Lake District in which this fish occurs, is the beginning of winter, from about the first week in November to the first in December, when the water over the spawning-beds has become comparatively cool, reduced from about 60° Fahr. to about 50°. Whether this is the only season is somewhat doubtful; the fishermen of Windermere speak of a later one, in which it is believed by them that fish of the larger size and few in number deposit their spawn, viz., in February and March.* Be this as it may, all the observations I have recorded were made on spawn obtained during the first period mentioned.

From analogy, it might be inferred that the time required for the hatching of the charr would be a variable one, depending on the degree of temperature of the water and on other less appreciable circumstances. In 1850–51, Mr Reynolds, as he informs me, found none hatched in a shorter period than 60 days; the greater number on the 70th, and from that to the 75th day; some few as late as the 90th. The average temperature of the water in the breeding boxes was about 40 . At a higher temperature, viz., an average one of about 55°, I have witnessed the completion of the process in the short period of 41 days. In this instance the milt and the roe were mixed as soon as they were taken from the fish on the 29th of last October; a certain number of the ova were put into a glass vessel and covered with water to the depth of about an inch, which was changed twice daily, and kept in a room the temperature of which was very uniform,—

* I am disposed to think that the breeding-time of the charr in Windermere is even less limited than is stated above, having found in the latter end of February individuals with the testes nearly of their full size, and this not in large fish; and others with ovaries containing eggs varying in size from a mustard to a millet seed. These fish were all from the lake; I have never heard of one being taken or seen in the Brathay (a river flowing into the lake, to be mentioned hereafter) after December.

seldom below 54° and never above 56°. On the 10th of December two young fish left their shells, and on the following day a third. They were all three feeble, as if their development had been premature; in a few days they died. Some eggs from the same fish which had been placed in Mr REYNOLD'S breeding boxes were not hatched till the 90th day, or more than double the time.

What the other circumstances are—other than that of mere difference of temperature—which influence the acceleration or retardation of the hatching process, are deserving of being investigated experimentally. Something may, perhaps, depend on the size and quality of the egg; something on the contact of the spermatozoa, their number and activity; and other conjectures might be offered.

In illustration of the growth of the young fish, after quitting the egg, I shall briefly describe what I witnessed in the instances of three that I observed with some care from the time of their escape from the shell to the attainment nearly of their perfect form. It was on the 17th of January that they were hatched. Some days previously the embryos were very active, frequently changing their position by sudden jerks, effected by the tail and the posterior portion of the body. One I saw in the act of bursting the shell, now become very thin and tender. The rupture took place suddenly at a spot where there was a little prominence,—an evident yielding of the shell to the pressure from within,—and simultaneously the coiled-up fœtus became liberated; the effort, it may be inferred, made by the tail, by which the opening was made, sufficing to extricate it. The instant the young fish entered the water, it darted about wildly for a few seconds; then rested, lying on its side. It was most easily disturbed; on the slightest touch, even if merely applied to the water near it, it fled from the touching body, moving with wonderful rapidity, and in such an irregular, devious course as was well adapted to promote its escape from a pursuing enemy.

These fish varied in length from about six-tenths to seven-tenths of an inch; the yolk attached was about ·25 of an inch in length, and about ·15 of an inch in depth, of an oval form. They were transparent and almost colourless, allowing the circulation of the blood to be seen distinctly with the microscope, using even a low power, such as a glass of one-inch focal distance. Their eyes appeared to be perfect, the lens visible and apparently prominent, the iris coloured; and, in accordance, the vision seemed to be acute, even the approach of a moving body, without coming in contact with the water, exciting alarm, indicated by a sudden change of place. The pectoral fins were distinct and almost constantly in action; the single embryonic fin including the rounded tail, extended inferiorly to the yolk sac, and superiorly a little beyond the spot where the dorsal fin was to be.

On the 30th of January, a very slight increase in their length was observable, about ·02 of an inch. The several fins, the dorsal, the abdominal, and anal, were beginning to appear in the form of slight projections from the single fin, especially the dorsal, in which rays were noticeable. The gill-covers now were somewhat

projecting, resembling fins, and were in constant motion over the branchial arches, in which the blood corpuscles were to be seen circulating in looped vessels.

On the 4th of February, it is noticed that the fish were acquiring colour, dark colouring matter being deposited in stelliform specks; that the embryonic fin was diminishing, and that the adipose fin was beginning to appear, marked by a slight elevation.

On the 14th of the same month, they were found to have increased to about ·8 of an inch in length, and the yolk to have diminished to ·2 of an inch, and to have become narrower.

On the 22d, the water in which they were kept was frozen over: they were seen swimming actively under the ice, and restlessly, as if in search of a passage to deeper and less cold water.

On the 13th of March, the dorsal fin was almost apart, the other fins advancing, the single one receding from absorption; the tail still rounded; the abdominal integument extending over the diminishing yolk, but not yet entirely covering it.

One died on the 18th of this month; the others on the following day. In these there was an appearance of sooty matter about the gills, which probably was the cause of their death, by obstructing respiration. One of them, weighed, was found to be little more than half the original weight of the egg; merely wiped, it was equal to ·58 of a grain; thoroughly dried, at a temperature of 100°, it was reduced to ·16 of a grain. From the time of their hatching to that of their death, I am not aware that they had taken any food other than that provided for them by nature in the attached yolk, a period of sixty and sixty-one days. Probably had they been favourably situated, where they could have found suitable food in the water, their growth would have been more rapid. One taken from the breeding-boxes on the 22d of March, hatched about the same time as the preceding, viz., the 17th of January, and when, consequently, about sixty-five days old, may be adduced in proof; premising that, from the manner in which the boxes were supplied with water, and their being shaded with trees, and some aquatic plants having been introduced, brought from the bed of the Brathay—that part of the river where the charr is known to spawn—there was probably no want of the proper food of the young fish, minute insects and infusorial animalcules, traces of which, indeed, were detected in its excrements, when seen under the microscope using a high power. The young fish of the age mentioned was perfect in its form. The embryonic fin had entirely disappeared, with the exception of a slight vestige of it between the anal and the abdominal fins. All the permanent fins had become distinct, even the adipose, though it was rather more extended and less elevated than in the full-grown fish. The caudal had lost its rounded form, and had become not forked but square. No vestige remained externally of the yolk-vesicle, the abdomen being entirely closed, covered uniformly with a

silvery integument. The back and sides, of a light greenish-brown, were marked by two rows of spots of a dark hue, almost black, the inferior the largest, reminding one of the bars of the parr and the marking of the young trout. Measured, its length was found to be one inch; its width or depth, where greatest, about ·16 of an inch. It was very active, and disposed to feed, darting often with avidity at any minute body thrown into the water, but only whilst in motion; and often after taking it into its mouth, casting it out. Fed daily, chiefly with finely-grated dried beef, it was kept alive till the 21st of June, when it was increased in length only to 1·06 inch, so inconsiderable had been its growth. The water in which it had been kept, and which was changed daily, was about the temperature 50°, sometimes two or three degrees higher, seldom lower. The young fish was frequently to be seen in a restless state, as if seeking to escape. Those of the same brood, left in the breeding boxes, effected their escape about the middle of April, when, in consequence of a flood, the water overflowed. They were then from 1·25 to 1·5 inch in length.

In the cartilaginous fishes, the yolk is found in the cavity of the abdomen long after it has disappeared externally. In the torpedo I have detected it there as late as the fifth month from the time of hatching.* That the same happens in the young charr, I cannot entertain a doubt. In one instance,—that of a fish hatched six weeks, kept the whole of the time in the breeding-box, and which was nearly perfect in its form,—though no trace of the vesicle remained externally, it was visible within, seen through the transparent parietes of the abdomen, distinguishable both by its form and under the microscope by the oil globules belonging to it.

3. *Of some Agencies and Circumstances supposed likely to influence the Ova and Young Fish.*

These, so far as I have tested them by experiment, I shall briefly notice.

From the best information I have been able to obtain, the charr in the Lake District, with few exceptions, chooses for its breeding-place stony and gravelly shallows in the lakes in which it is found, and never, after the manner of the trout, ascends the small streams towards their source to deposit its spawn. The exceptions alluded to, which have come to my knowledge, are in the instances of the charr of Windermere and that of Ennerdale. The former, it is known, not only breeds in the lake, but also in the river Brathay; but it deserves to be kept in mind, that that part of the river which it selects for the purpose has a good deal the character of a lake, the water there being expanded, forming a small lake or pool, where, in parts out of the actual current, it is little more disturbed by the wind than the shallows of Windermere itself. The charr of the lake of Ennerdale—the other exception—I am assured on good authority, that of Dr

* See Researches, Physiological and Anatomical, vol. i., p. 73.

Lietch of Keswick, frequents in the spawning season a pool of a little mountain river, called, from the circumstance, the "Charr Dub," about 300 yards from the head of the lake; itself (the pool) about 120 yards in length, and about 6 or 7 yards in width, with a sandy, gravelly bottom, and large stones here and there interspersed. In this pool, it is said that the fish congregate, with great regularity as to time, about the 7th or 8th of November, and remain there usually about a fortnight, when, having performed the function for which they came, they return to the deep water of the lake.

I make this statement in consequence of some naturalists, guided by the analogy of the best-known species of the Salmonidæ, having inferred that, like them, the charr can breed only in running water, and that its being seen in large numbers in the spawning season in shallow water in lakes, was only preparatory to ascending the streams. The weight of evidence against this conclusion is such, that I think it cannot be maintained; nevertheless, it appeared to me worth while to make a few experiments for the purpose, if possible, of testing it. With this intent, portions of roe, after having been mixed with liquid milt, were put into vessels, some of earthenware, some of glass, with a limited quantity of water (not changed during the trial); some in the open air, some within doors. This was done on the 4th of November, using the roe that had been obtained on the 30th of October, the same as that from which three ova, as already mentioned, had been hatched in forty-one days. None of these trials were perfectly successful: excepting in one, no progress towards development was observable. This was in the instance of ova contained in a glass bottle of eight ounces capacity, the water about two inches deep, and kept in a room, the temperature of which was commonly about 55°. On the 26th of the same month, marks of progress were observable in one of these ova; the eyes of the embryo were apparent as black specks, and vessels carrying red blood were to be seen ramifying in the vitelline membrane. The development went no farther. Even imperfect as this result is, is it not in favour of the conclusion that running water is not essential to the hatching of the fish?

Mr Reynolds mixed together the roe of a lake trout and the fluid milt of a charr, which he placed in his breeding-boxes in November. In 70 days some of the ova were hatched, and the young fish had a hybrid character, the fish themselves having much the appearance of the charr of the same age, whilst the yolk attached, with its few large richly-coloured oil globules, was exactly similar to that of the trout. Is not, I would ask, this fact that the ova of the one species can be fertilized by the spermatic fluid of the other, in favour also of the conclusion that the breeding-places of the two are different? Were they not so, as the breeding season of the two is the same, a constant crossing would be almost unavoidable, and a confusion and loss of species would be an almost necessary consequence.

As a solution of common salt has the property not only of keeping liquid the fluid of the yolk, but also of dissolving its coagulum, it seems well adapted as a medium for the purpose of examining the fœtal structure. Using it thus, I found that an ovum in which the embryo was active on the 42d day, immersed in a solution of salt of the specific gravity 1033, kept therein about half an hour, retained its vitality; and that, excluded by an opening artificially made in the shell, the young fish remaining in the solution, continued active for another half hour. This result led me to try the effect of keeping the ova in solutions of common salt, and also the young fish, to ascertain whether the former would be hatched, and what would be the effects on the latter. One trial was made with the ova, using salt water of the specific gravity mentioned, 1033; another with water just perceptibly impregnated with salt, confined in glass bottles and kept in the room of the average temperature of about 55°. In the stronger solution, the ova remained transparent, but no marks of development appeared. In the weaker solution, on the 26th of November,—the trial was begun on the 4th,—black specks denoting eyes, in the act of forming, were observable in four ova, and vessels carrying red blood in the vitelline membrane. In this stage, further progress was arrested by death. The first experiment on a young fish was made on one that had been hatched about 22 days. Put into sea-water, diluted with spring-water so as to be of specific gravity 1020, it was found dead three hours after; it was contracted in length from ·68 to ·46 of an inch. The next was on a young charr of the same age: this, immersed in a solution of the specific gravity 10036, after 24 hours, seemed as active as before. More salt was then added so as to increase the specific gravity to 10068, but still without marked effect. After other 24 hours the specific gravity, by another addition of salt, was raised to 10098; now the fish became more restless, as if seeking to escape. After the same interval a fresh portion of salt was introduced, raising the specific gravity to 10153: the effect now was strongly marked; in about six hours the fish was found motionless, except the lower jaw, which, under the microscope, exhibited a tremulous movement, and except the heart, which still acted pretty vigorously, and which continued to act, but with decreasing force, for about 20 hours, reckoning from the time that the fish first appeared motionless and moribund.

The next trials I shall mention were made with the intent to endeavour to ascertain how long young charr might be kept alive in the same portion of water, and that a small quantity, such as might be used in conveying the fish from place to place at an early age, when, before the yolk is exhausted, it stands in no need of a supply of food from without. Two experiments were made, one with a portion of pure oxygen over the water, the other with common air. The volume of water and air in each instance was nearly equal—about four ounce measures,—the capacity of the containing bottle being about eight ounces. The bottles, after the introduction of the young fish, were closed with a glass stopper and inverted

in water; they were kept part of the time in the open air, and part of it in the room of equable temperature: each fish had been hatched about six weeks. The one in water, with oxygen, put in on the 28th of January, was very active till about the middle of February; about the 24th of that month it began to appear languid, and it was more so on the 26th, when it was taken out and transferred to a vessel fully exposed to the air, and the water in which was changed daily. Though it lived till the 18th of March it did not recover its activity. Its growth whilst under oxygen was much the same as if it had been kept in water exposed to the air and changed daily. The oxygen used was not tested for carbonic acid; by the taper-test its purity did not appear to be impaired. The trial with common air was commenced on the 7th of February; on the 13th, the young fish was found dead. As there was a small spot of stagnant blood in the vitelline membrane, its death might be owing to disease unconnected with the peculiarity of circumstances in which it was placed. On the 28th of March I repeated the experiment with a young fish which was vigorous and active. Taken out on the 4th of April, its activity seemed unimpaired; it fed greedily. This fish had been hatched about seven weeks.

The only other trials I have made have been on the effects of temperature,—an influence that this fish appears to be peculiarly sensitive of, as indicated in all its habits, and in the circumstance that it is only found in those lakes in which, in consequence of their great depth, it can find a retreat in summer and winter in water of about 40 Fahr. On the 28th of March I transferred into water, of the temperature of 83 , a young charr that had been hatched not quite seven weeks. It rushed about for a second or two, then turned on its back and rose almost inanimate to the surface. The heart and gill-covers being still in motion, it was instantly put back to the water from which it had been taken of 52 . It made one or two efforts as if reviving, swimming for a few seconds in a natural position; but in less than a minute it was dead, the heart having ceased to act: thus, compared with the effects of a solution of common salt, offering a remarkable contrast. On the 29th of the same month, a young charr of about the same age as the preceding was put into water of 75 : it immediately became very restless: its gill-covers moving rapidly. After a quarter of an hour, when the temperature of the water had fallen to 70 , it lay still at the bottom and not apparently distressed, except that the movement of the gill-covers and the action of the heart were unduly quick. In an hour and a-half, when the water was 60°, it was still at rest: some hours later, when the water was 54°, it seemed well; and, on the following day, put into fresh water, it appeared as active as before.

I have now to conclude. This I shall do without entering on the embryology of the charr,—a vast subject, which, in the instance of one of the family of the

Salmonidæ (*Coregonus Palœa*), M. Vogt has so ably and elaborately treated of in the work already referred to.

The observations I have described are fewer than I could have wished, and the results more imperfect; I can offer them only in the manner in which I trust they will be received, viz., as a contribution to the history of the charr.

I may notice some of the facts which they seem to establish, and some of the inferences which they appear to me to warrant.

1. That the time required for hatching the ova of the charr is variable, depending on the degree of temperature of the water and other influences: that 70 days may be considered about the average, and 40 and 90 about the extremes.

2. That after exclusion from the egg the young fish can live at least 60 days without taking food, deriving the material required for its support and growth from itself, and chiefly from the store that nature has supplied in its yolk.

3. That under favourable circumstances, it attains its perfect form in about from 60 to 70 days, when it becomes dependent for its subsistence chiefly on food which it has to seek and to procure from without; though even then it is probable the whole of the yolk is not expended, so that external food failing, the privation can be borne and life maintained, and that for no inconsiderable time, by means of the residual yolk contained within the abdominal cavity.

4. That running water is not essential to the hatching of the ova; and, in consequence of its breeding-place being distinct from that of the trout, it is exposed to little risk of being lost as a species by repeated crossings with the trout.

5. That salt water, even of greater saltness than sea-water, is not immediately fatal to the embryo, even when not included in its shell; moreover, that in slightly brackish water a partial development of the ovum may take place; and that the young fish can exist some days in such water, rendering it probable that the adult may be capable of existing in a tidal stream, or even in the sea, for a time, where it is stated that the Welsh charr has been caught.*

6. That in water of small bulk, such as may be used for transporting fish from place to place, with common air, the young charr may endure confinement for several days without impairment of its vigour; and that substituting oxygen, it may endure such confinement for a much longer time, at least quadruple that period.

7. That the young fish can bear, without any immediate injury that is apparent, a temperature removed only a degree or two from the freezing-point of water; and also a higher temperature, ranging from 60° to 70°, but not above 83°, which, in the single instance tried, was almost instantly fatal to it.

The application of these facts to the breeding and transporting of the charr

* See Mr Yarrell's History of British Fishes, vol. ii., p. 71. 1st Edit.

hardly requires any comment. Whilst they shew how easily it may be introduced into any lake or body of water, they are of no significancy in relation to the establishing it for a permanency in such water. What appears to be most requisite for the purpose is deep and pure water. In no body of water in the Lake District is the charr found, which is not of this character. The attempts to establish it in some not possessed of the qualities named, have repeatedly failed; and in others, in which the fish once abounded, it has become either entirely or almost extinct, since mines have been opened in their vicinity, by which the purity of the water, it may be inferred, has been impaired. Whether the quality of the food is of much importance, seems to be doubtful in relation to this its maintenance. There are circumstances that seem to warrant the conclusion, that, like the trout, its condition rather than its existence depends on the kind of food, and the quantity it can obtain. This we know, that it is taken with the same baits as the trout, and also that it exhibits varieties like the trout, though hardly so strongly marked, according to, as is believed, its manner of feeding; for instance, the charr of Hawes Water, which is known to feed a good deal on insects, is a small and slender fish in comparison with the charr of Windermere, which feeds more at the bottom, and has a less precarious supply, especially of squillæ, which abound in that lake.* These remarks are offered with hesitation. The subject is one that is not without obscurity, and in need, for the better understanding of it, of further and minute inquiry specially directed to it.

Lesketh Howe, Ambleside,
February 28, 1852.

P.S. Reflecting on the effects of sea-water on the ova of the charr and its young, shortly after quitting the egg, as described in this paper, I venture to offer the conjecture, that the action of sea-water may be similar on the impregnated egg of the salmon and its fry; and that it is on this account (looking to the final cause), rather than for the purpose of seeking water cooler and more aerated, that the salmon, impelled by instinct, quits the sea for the river, preparatory to breeding; and also, that the young remain in fresh water till they have acquired not only a certain size and strength, but also additional scales, fitting them, in their smolt stage, to endure without injury the contact of the saline medium.

* The charr of the Lake District, though occasionally taken with the artificial fly and minnow, like the trout, on the whole, I believe, may be considered a more delicate feeder, and, in consequence, of superior quality for the table; its organization is in accordance with this, viz., its smaller teeth, and smaller stomach and intestines. The charr of Upper Austria is said to have a thick stomach, approaching in its character to that of the Gillaroo trout. (See *Salmonia*, p. 55, ed. 4th.) In most instances that I have examined this organ in the charr of the Lake District, I have found it as thin, and often even thinner in its coats than that of the trout inhabiting the same water.

I have had no opportunity to try the effect of sea or salt water on the impregnated ova of the salmon. The few experiments I have been able to make on the young fish have given results favourable to the above conjecture. I shall briefly relate them.

On the 10th of April, a young fish, about an inch in length, its permanent fins fully formed, taken from a small pool in the bed of the Leven (the river that flows out of Windermere, and then unusually low) was put into a half-pint of salt water, of the specific gravity 10277. It lived about thirty-three minutes. Shortly after, a smolt, the instant it was taken was put into the same water; it was about seven inches in length, and its head was not constantly under water. It lived about an hour. From comparative experiments with fresh water, I am led to infer that in the same limited quantity of river water, it might have lived two hours; the limit being probably the exhaustion of the air. When a stronger solution of salt was used—that in the preceding experiments being nearly the same as sea-water—the effects were far more decided. Thus a fish of the same size as that first mentioned, put into a saturated solution of common salt, died in two minutes; and a parr taken on the 10th of October, measuring about four inches in length, put into a solution of common salt of the specific gravity 1047, died in a few minutes.

April 12, 1852.

XXI.—*On the Total Eclipse of the Sun, on July* 28, 1851, *observed at Göteborg; with a description of a new Position Micrometer.* By WILLIAM SWAN, F.R.S.E.

(Read 1st December 1851.)

Having long desired to witness a total eclipse of the sun, I resolved to proceed to some place in the line of the moon's shadow, for the purpose of observing that which took place on the 28th of last July.

Various reasons induced me to prefer the town of Göteborg in Sweden to any other station. It had interesting historical associations connected with eclipses; and there was something pleasing in the prospect of seeing the red prominences, which excited so much attention at the eclipse of 1842, at the very spot where they were observed, probably for the first time, in 1733;* but a more important ground of preference, was its proximity to the central line of the moon's shadow, and its being directly accessible from England.

I accordingly sailed from Hull on the 19th July, by the Courier Steamer, and reached Göteborg on the 24th. Among the passengers were Mr LASSEL, Mr ADAMS of Cambridge, Mr CARRINGTON of Durham Observatory, Mr ROBERT CHAMBERS and Mr JOHN ADIE of Edinburgh, Mr DUNKIN of Greenwich Observatory, and the Astronomer-Royal.

Mr AIRY had determined to observe the eclipse at Göteborg, and at his request a meeting was held there on the 26th July, when it was agreed by those who were present to separate as much as possible, in order to increase the chance of at least some one seeing the eclipse, in the event of the weather proving cloudy,—a precaution which its unfavourable aspect at that time rendered the more advisable. Professor CHEVALLIER of Durham and Mr JOHN ADIE, had previously determined to observe the eclipse from the roof of their hotel in town. Lieutenant PETTERSSON of the Navigation School of Göteborg, kept by the Observatory of that institution. The Astronomer-Royal selected a station to the east of the town, and Mr CHAMBERS, one about three miles to the west. I chose for my station a hill named Ramberget, situated about a mile to the north of Göteborg, and on the opposite bank of the river. This, being the highest eminence in the neighbourhood of the town, commanded an extensive view of the country on every side, and was therefore a very favourable station for witnessing the effects produced on the landscape during the eclipse. I referred its position to that of Lieutenant PETTERSSON's Observatory, by the magnetic bearings of several conspicuous objects, taken by means of a prismatic compass. (*See Table* I., page 345.) The observations, when

* See *Observatio Eclipsis Solis totalis cum mora facta Gothoburgi Sveciæ, &c., à Dom. Birgero Vassenio.*—Phil. Trans., vol. xxxviii.

protracted on a trustworthy map of the environs of Göteborg,* are very well satisfied by a point, which, from the known latitude and longitude of the Observatory, I find to be in latitude 57° 42′ 57″·3 N. and longitude $0^h\ 47^m\ 45^s{\cdot}2$ E.†

So many phenomena occur at the total phase of a solar eclipse, that I wished to avoid having my attention distracted by my being obliged to count the beats of the chronometer in taking observations for time. I therefore gladly availed myself of the assistance of Mr Edward W. Lane, Advocate, of Edinburgh, who kindly undertook to read the chronometer, and mark the times at a preconcerted signal. His co-operation proved quite invaluable; and it is with the greatest pleasure I avail myself of this opportunity of acknowledging my obligations to him.

The telescope I employed in observing the eclipse was furnished by Mr Adie. It has a very good object-glass‡ of about 2·3 inches aperture, and 31·5 inches focal length, and was mounted on a rough equatorial stand. Of the eye-pieces belonging to this instrument, I chose that of the lowest power, magnifying 28 times, as it was necessary for some of the observations I purposed to make, to have the entire disc of the sun within the field of view at once. I also then thought, and I am still of the same opinion, that any advantage gained by using a higher power would be more than counterbalanced by the time lost, during the short duration of the total phase of the eclipse, in directing the telescope from point to point of the moon's limb, instead of seeing the whole at once. In effect, the power I had chosen proved very convenient, and apparently quite sufficient for observing the interesting phenomena of the total phase; while the definition of the corona and the red prominences seemed as perfect as could be wished.

I had prepared some slips of smoked plate-glass, gradually increasing in depth of tint from one end to the other, for the purpose of observing the sun before the period of total obscuration; but Professor Chevallier kindly lent me a dark glass, by Troughton and Simms, consisting of wedges of coloured glass achromatised by a colourless prism.§ This combination of glasses made the sun appear yellow, slightly tinged with green, and I willingly adopted it in preference to the smoked glasses, as the definition of the sun was decidedly sharper when it was used instead of them. This dark glass slid in a groove in front of the eye-piece, so as to admit of being instantly removed.

From the conflicting accounts which were given regarding the red prominences

* This map, published by A. Hahr, is entitled *Topografisk Karta öfver Götheborgs Omgifning Jemte plan öfver Staden med dess nya Hambyggnad.* 1844.

† Since this paper was read, Lieutenant Pettersson has kindly verified my calculation, and assigns, as the position of my station, lat. 57° 42′ 58″·0 N., long. $0^h\ 47^m\ 45^s{\cdot}3$ E.

‡ As the value of the following observations must depend greatly on the character of the instrument with which they were made, I may mention that this telescope shews bright stars, with perfectly round, well-defined discs; and, with a power of 75, the two stars in Castor are seen completely separated.

§ To Professor Chevallier, and especially to Lieutenant Pettersson, my warmest thanks are also due, for their kind assistance and advice.

BRITISH MUSEUM
NATURAL HISTORY.

Fig 3 Fig 4 Fig 5 Fig 6

Fig 1

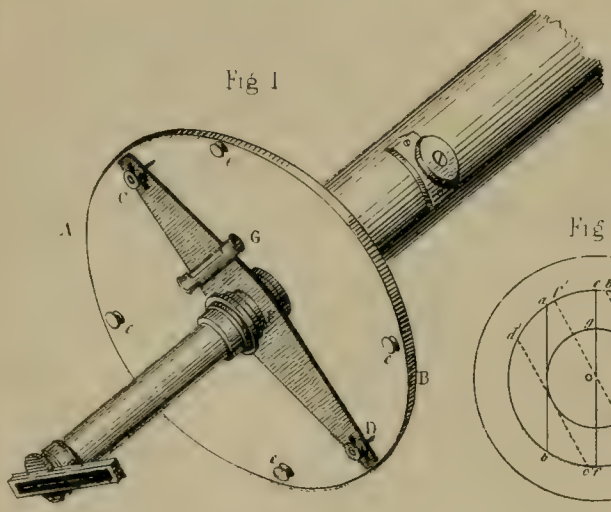


Fig 2

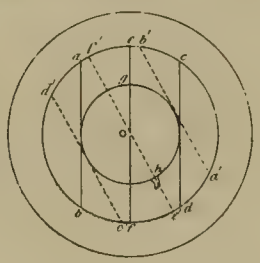


Fig 7

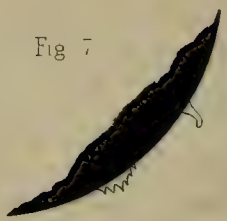
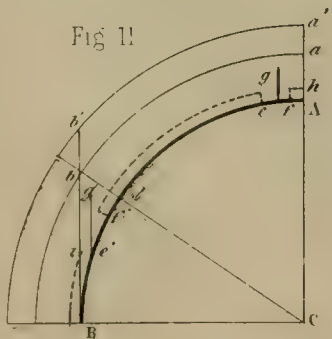


Fig 8

Fig 9

Fig 10

Fig 11



Lith.r by W. & A. Johnston, Edin.r

seen on the moon's limb at the eclipse of 1842, it seemed very desirable to have some means of noting with accuracy the positions of any objects of a similar nature that might appear at the approaching eclipse; and in a letter in the *Athenæum* of 12th July 1851, I suggested a species of position micrometer suitable for that purpose. The instrument there described, with a slight addition, was constructed for me by Mr JOHN ADIE, and its performance proved very satisfactory. It consists of a circular plate of metal, AB, fig. 1, (Plate XI.) 8 inches in diameter, attached to the sliding tube of the telescope by a split collar with a tightening screw, not seen in the figure, so as to prevent it from turning round. The face of this plate, next the eye-end of the telescope, was covered with a disc of card, attached to it by four screws, *eeee*. Inside the tube carrying the plate, another tube carrying the eye-piece slid smoothly, so as to admit of being freely turned round. To this were attached, by another split collar and clamping screw, two springy arms, FC, FD, bearing steel points, by which holes could be pricked in the card disc, and a small level, G, was fixed at right angles to one of the arms. In the eye-piece were three equidistant parallel spider-lines, *ab*, *cd*, *ef*, fig. 2; the two outer, *ab*, *cd*, being placed at an interval equal to the apparent diameter of the moon, calculated for the time of the total phase of the eclipse; so that when they were made to embrace the moon's disc, *gh*, the middle wire would pass through its centre, *o*. The instrument was adjusted for observation by making the middle wire coincide with a plumb line, seen at a distance of about 150 yards, while at the same time the bubble of the level was brought to the middle of its tube; and the arms with the level were then clamped to the tube carrying the eye-piece.

When this adjustment was completed, it is obvious that the wires in the eye-piece would point vertically whenever the bubble of the level was again brought to the middle of the tube. If now the bubble were brought to the middle of the tube, while the outer wires were made to embrace the moon's disc, the middle wire would pass through its vertex, *g*; and two holes being pricked in the card, the line joining them would represent the moon's (or, with sufficient accuracy, the sun's) vertical diameter at the moment of observation. If next, while the moon was still kept between the outer wires, the middle wire were made to bisect any object, *h*, near its limb, the wires now having the positions *a'b'*, *c'd'*, *e'f'*, and holes were again made in the card, the angle between the lines joining the respective pairs of holes would measure, *goh*, the angular distance of the object from the sun's vertex. It is easy to see how, in this manner, the positions of the red prominences seen during a total eclipse, could be rapidly registered on the card without ever removing the eye from the telescope. In order to repeat the observations, the steel points admitted of being moved in longitudinal slits in the arms, so as to describe circles of different radii on the card; and the *reading point* was distinguished from the other by being placed a little farther from the centre.

It is evident that if the telescope were mounted equatorially, the level could be dispensed with, and the objects might be referred to a parallel of declination, by causing a spot on the sun to travel along the wires; but my stand was too rude to allow this method to be adopted with safety.*

In order to ascertain the times of the different phases of the eclipse, I used a box chronometer by ADAMS of London, which was obligingly furnished by Lieutenant PETTERSSON. It was compared with his standard chronometer about $3^h\,15^m$ before the commencement of the eclipse, and again the following day after an interval of 24 hours. The error and rate of the standard chronometer had been determined by observations made with a small transit instrument at the Observatory.

For several days before the eclipse the weather was variable, with little sunshine; and it became gradually worse, until at length the morning of the 28th arose as gloomy as the most unfavourable foreboding could have anticipated. But about noon, to the great delight of every one, the sun shone brilliantly, and the sky soon became nearly cloudless towards the zenith. This state of things, however, did not last long; for shortly after the commencement of the eclipse, an extremely thin cirrous cloud began to overspread the sky. I was apprehensive that this might interfere with the observation of the eclipse; but it produced no sensible effect in impairing the definition of the sun, which was remarkably good, and unusually free from tremulous motion. All the minute spots and faculæ, which were visible before the cirrous cloud had formed, were seen until they were covered by the moon; and it was only after the total phase of the eclipse had passed that the definition was perceptibly injured. Towards the horizon, especially in the east, the sky was pretty thickly studded with detached cumulous clouds; and a strong south-west breeze continued to blow during the eclipse, except about the period of the totality, when the wind almost entirely subsided.

I determined the places of the only spots I saw near the sun's limb by means of the position micrometer. There was a patch of small spots $96^\circ\,30'$ to the west of the sun's vertex, and about $1{\cdot}5'$ from its limb; and a considerable spot, evidently round, but much foreshortened, $62'$ to the east of the vertex, and less than $1'$ from the limb of the sun. This spot was surrounded by conspicuous faculæ; and after two days, when it had advanced on the sun's disc, it proved, as it had seemed at first, to be circular.

At the commencement of the eclipse, my eye was directed to the point at which the moon's limb entered the sun's disc; but, although I distinctly saw the first impression of the moon, I did not feel perfectly sure of this until about two

* The chief inconvenience I found in using this instrument, arose from being obliged to point the telescope by the hand. A slow rack motion would have been very useful. In observing a total solar eclipse, every moment is so valuable, that too much care cannot be bestowed beforehand in having everything adapted to save time. From my own experience, I should recommend observers to have their telescope mountings as commodious and *firm* as possible.

seconds had elapsed. The time stated as the commencement of the eclipse, is, therefore, probably two seconds too late. This was $2^h\ 53^m\ 4^s{\cdot}4$ Göteborg mean time.

There were numerous mountains on the moon's limb, which gave it a sensibly serrated appearance, and it was much more sharply defined than that of the sun. The gradually decreasing brightness of the sun's disc from the centre towards the edges, which is pointed out by Mr AIRY in his account of the eclipse of 1842, was best seen when the sun was about half covered by the moon.

Repeated attempts were now made with the naked eye, with the telescope, and with a French opera-glass of 1·9 inches aperture, and 5·8 inches focal distance, to ascertain whether the moon's disc was sensibly illuminated, and whether any part of its limb was visible beyond the sun. But although in every trial, the sun's light was as little diminished by the dark glasses as the eye could bear, the face of the moon looked quite black, and no part of its limb was visible beyond the sun's disc.

During the progress of the eclipse, the cusps continued perfectly sharp, as represented in fig. 3, until the sun was reduced to an extremely narrow crescent of 90°, or less, when they began to assume a decidedly rounded appearance (fig. 4). It seemed as if the light had flowed beyond its proper boundary, so as to invade the province of darkness; the cusps becoming disfigured, much as they would have been had one attempted to draw their outline in ink upon blotting paper, where the ink flowed slightly beyond the limit traced by the pen.*

Daylight had now greatly diminished, and the air felt chilly. Towards the west, in the direction of the approaching shadow of the moon, the sky looked extremely black and frowning, and the whole landscape wore a peculiarly cold and desolate air. The light had much of the ordinary gray tint of morning, and less than I expected of the peculiar greenish hue I remember to have observed at Edinburgh, in the eclipses of May 1836 and July 1842; and as the totality approached, the sky assumed a more cloudy appearance than it had at the commencement of the eclipse, either from the actual formation of clouds, or as I could not help thinking, from something in the altered state of the light rendering the existing clouds more visible.

The sun was now nearly gone, and darkness was coming on with a degree of rapidity which was quite startling. From the accounts of previous eclipses, I was prepared to anticipate something very awful; but I certainly did not expect that this part of the phenomenon would have affected me so much. An instan-

* SHAKESPEARE makes Hecate say:—

"Upon the corner of the moon
There hangs a vaporous drop profound."

This odd fancy forms no unapt description of the rounded appearance of the cusps, which certainly looked very much as if a drop of liquid were depending from them.

taneous transition from the blaze of noon to midnight darkness would be a grand phenomenon; but I believe it would not be more appalling than the gradual, but at length fearfully rapid march of darkness, which precedes a total eclipse of the sun.

As the total phase was rapidly approaching, I took a last look of the landscape, in order to note the appearance of the shadow sweeping over the ground, which has been described by former observers. I failed, however, to see this, probably from having expected it too soon; but just before proceeding to observe the commencement of the totality, I looked up for an instant, westward of the zenith, when I am satisfied I saw the progress of the moon's shadow through the sky. The boundary between light and darkness was tolerably definite, and the slightly clouded state of the atmosphere no doubt helped to render this more visible than it would otherwise have been.

A short time before the sun disappeared, the rounding of the cusps became very striking (see fig. 5), so that their points somewhat resembled the spurious discs with which bright stars are seen with a considerable magnifying power. This resemblance seems to have been that alluded to by HALLEY in describing the eclipse of 1715, where he remarks, that, "about two minutes before the total immersion, the remaining part of the sun was reduced to an extremely fine horn, whose extremities seemed to lose their acuteness, and to become round like stars." *

The limb of the moon now became quickly joined to that of the sun by numerous thick lines† (fig. 5), which immediately began to run into each other with great rapidity, like contiguous drops of water, so that the eye could not follow their motion. They occupied nearly all the remaining crescent of the sun, and were so numerous, that I had not time to count them before their fluctuating movements rendered it impossible to do so. The spaces between the lines were at first rudely rectangular, but gradually became rounded so as to resemble a string of bright beads (fig. 6), and then finally disappeared. The disappearance of "Baily's beads" took place at $3^h\ 55^m\ 52^s{\cdot}6$, Göteborg mean time, which was observed as the commencement of the total phase.

I had gradually slid out the dark glass towards the approach of the total phase, so that when the sun disappeared, its light was but slightly obscured. No trace of the corona, however, was visible through the dark glass, and it was only when I looked at the sun with the naked eye, the moment the beads were gone, that I saw the corona already fully formed. It was a ghastly sight to be-

* Phil. Trans., vol. xxix., p. 248.

† The nearly instantaneous appearance of these lines vividly recalled a well-known passage of Coleridge's,—

> "that strange shape drove suddenly
> Betwixt us and the sun,
> And straight the sun was flecked with bars——"

hold a black sun surrounded by a pallid halo of light in a sky of sombre leaden hue. The darkness at first seemed very great, owing to its contrast with the recent sunshine; and Mr LANE found it necessary, in reading the chronometer, to use a candle which had been previously lighted. The horizon, however, towards the north, was filled with light of a magnificent yellow-orange or amber colour, which contrasted strongly with the dark purplish-gray of the sky overhead. I neglected to try whether it was possible to read by this light; but I have no doubt I could have done so, as there was no difficulty in writing down the time of commencement of the total phase;* and on looking towards Göteborg, the spires, and eminences in the neighbourhood of the town, were dimly discernible. The surface of the moon, to the naked eye, seemed slightly luminous, especially towards the edges; but this might be caused by light reflected from the thin cirrous cloud which intervened between it and the eye. The light of the corona also gave the moon a sharpness of outline, and an appearance of being raised from the sky, which made it look very near.

These observations were completed in a few seconds, and I instantly proceeded to examine the corona through the telescope, having first removed the dark glass. The beauty of the corona and its red mountains† at once made me forget the frightful appearance of the eclipsed sun as seen by the naked eye; and I never witnessed any spectacle which so powerfully fascinated both the imagination and the senses. I gazed at the wondrous sight with intense pleasure; and it was with a feeling of painful regret that at length I saw the increasing light on the moon's western limb, which warned me it was about to depart for ever.

To the naked eye the corona seemed white, slightly tinged with faint purple or lavender colour. This, however, might be merely a complementary tint, occasioned by the contrast of the strong amber-coloured light in the horizon; for when viewed through the telescope, it was silvery white. Its structure was distinctly radiated, the light appearing to stream out from behind the moon in vivid needle-like rays, as if it emanated from some source of intense ignition. Its appearance has been aptly compared by Mr BAILY to that of the sun shining through a grove of trees, and a similar apparent emanation of luminous particles may be seen in looking at the lime-ball or the electric light. HALLEY says, that at the eclipse of 1715,‡ "there were perpetual flashes or coruscations of light, which seemed for a moment to dart out from behind the moon, now here, now there, on all sides." I saw nothing resembling this except the apparent motion of the light outwards, to which I have just referred; but that motion was tolerably uniform, and did not cause a flashing appearance There was no circular motion in the corona, neither did it exhibit

* It may be proper to mention that my vision is rather highly myopic, and that I can read with less light than most persons.

† It is not meant to indicate by this term any opinion as to the *nature* of the red prominences.

‡ Phil. Trans., vol. xxix., p. 249.

anything like concentric rings. Its light was brightest next the moon's limb, and gradually shaded off into darkness at a distance of about half the moon's diameter.

The most striking feature in the corona was the appearance of brilliant beams of light which shone out in various directions. They were sharply defined, and much brighter than the rest of the corona; and, probably owing to their superior illumination, they were visible a little beyond its general outline. One of these beams (see Plate XII.), I found was situated 28° 35′ to the east of the sun's vertex. It constituted by far the brightest part of the corona, and had a sort of conoidal figure. I had not time to ascertain the positions of the other beams; but there was a remarkable one about 35° or 40° to the west of the sun's vertex, and two others which I have ventured to represent in the figure from memory. The three latter beams were quite different in form from the first. They resembled the narrow sunbeams which shine through broken clouds; or the inverted cone of light visible in the dark over a blast-furnace fed by coke. Their sides were beautifully rectilinear, apparently converging to the centre of the sun, so that their forms were those of very acute cones. In one at least, the light increased in brilliancy from the centre towards the sides, as if the cone were hollow; its edges appearing brightest owing to the luminous stratum, constituting the hollow cone, being there presented to the eye more obliquely, and therefore acting on it with a greater depth of lucid matter.

The first object that attracted my attention on looking at the corona through the telescope was a remarkable hook-shaped red prominence (represented in Plate XII., and in Plate XI., figs. 7 and 8) 110° 30′ to the west of the sun's vertex. The next moment I thought there was the trace of a red prominence in the middle of the bright beams of light to the east of the sun's vertex; but in another instant my attention was withdrawn from this by the appearance of a second prominence a little below the hook-shaped one, and on looking back I saw no farther trace of red light to the east of the sun's vertex.

As considerable doubt had been expressed whether the red prominences exist in the sun or moon, or are only optical phenomena, I was prepared to look for faint objects of variable and indistinct appearance, requiring, perhaps, considerable attention to see them at all. I was therefore agreeably surprised to find the prominences objects of perfectly definite outline, and of permanent form so long as they continued visible. The hook-shaped prominence, especially, had a remarkably smooth, sharp outline, and its rose tint became darker towards the edges, suggesting the idea of a convex surface. At the risk of offering what may be deemed a whimsical comparison, I may mention, that, at the moment, it seemed to me very like the Eddystone, or Bell Rock lighthouse transferred to the sun, with its top beginning to fuse and bend over like a half melted rod of glass. The other prominence was of less height, but of greater lateral extent; and its top was deeply serrated, so as to bear a strong resemblance to a chain of peaked

Sun's Vertex

W. Swan, Delt

Engraved by W. & A.K. Johnston, Edinr

THE TOTAL ECLIPSE OF THE SUN

July 28th 1851.

As seen towards the end of the totality by M.r W. Swan at Göteborg in Sweden, with a telescope magnifying 28 times

NATURAL HISTORY

granite mountains. Both prominences were remarkably distinct from the corona, so as almost to appear standing in front of it; and their outlines seen upon it were at least as definite as that of the illuminated edge of a detached cumulous cloud projected against the clear blue sky. But as the sharp definition of such a cloud is an illusion, depending as much on its distance, as on the density of the vapour composing it, I do not mean to draw from this comparison any inference regarding the density of the matter composing the red prominences. Notwithstanding their definite outlines, they may, like the tails of comets, be of extreme rarity, and indeed, as Sir JOHN HERSCHEL remarks,* their faint illumination clearly proves them to be "cloudy masses of the most excessive tenuity." The colour of the prominences was a full rose-tint, and the light of the corona in their neighbourhood seemed brighter than elsewhere, with the exception of the brilliant beams already mentioned.

The appearance of the prominences as they were seen shortly after the commencement of the totality is represented in figure 7. By means of the micrometer I determined their positions as well as that of the bright rays to the east of the sun's vertex, and then quitted the telescope for a little to make some other obervations. On returning to the telescope I found that the bright rays to the east of the sun's vertex appeared shorter than before, while the red prominences to the west had increased sensibly in height; and while I watched them, they continued to increase still more in size, as if rising from behind the moon's limb. I should almost say their motion was sensible; but however doubtful this may be, its cumulative effect was strikingly apparent, for before the end of the totality they had assumed the appearance presented in figure 8. All this was exactly what would have happened on the supposition that the prominences belonged to the sun; for objects on the eastern limb would gradually suffer occultation by the advancing moon, while those on the western limb would be simultaneously exposed.

While, then, the definite outlines and permanent forms of the prominences satisfied me that they were real objects, and not mere optical phenomena, their gradually increasing altitude convinced me that they belong to the sun and not to the moon. The observed angles of position of the red prominences and spots on the sun's disc, referred to the sun's vertex, and also their angles of position reckoned eastward from the sun's vertex are given in Table II., p. 346. The data for reducing their positions to the sun's vertex are the known latitude of the station, the sun's declination, and the hour angle from apparent noon, assuming the observations to be made at the middle of the totality.

The prominences were distinctly visible to the naked eye by the strong red tinge they imparted to the adjacent portions of the corona; but I could neither distinguish their outlines nor see them as separate objects.

I wished to compare the shadow cast by the corona with that formed by a

* Outlines of Astronomy, 1851, par. 395.

candle; but upon a rapid trial it was found that the corona cast no sensible shadow, its feeble light being evidently overpowered by the diffuse illumination derived from the horizon.* I also looked at the corona for an instant with a Nicol's prism, and thought its outline was slightly distorted, so as to appear somewhat four-cornered; but as there was no time to repeat this observation I regard it as extremely doubtful.

I was so much occupied during the totality with more important observations that I found no time to look for stars; but Venus was too conspicuous an object to escape detection. It appeared shining brilliantly a little to the west of the sun.

I now prepared to observe the end of the total phase, and I had not the slightest difficulty in finding the point of greatest brightness on the moon's limb where the sun actually emerged. His re-appearance was preceded by something like a gradually brightening twilight; and the red prominences had vanished, before the formation of Baily's beads announced the end of the totality. The beads were not now so numerousas at the moment of total obscuration, but their appearance was otherwise the same.

The end of the totality was observed at $3^h\ 59^m\ 8^s{\cdot}1$ Göteborg mean time, making its duration $3^m\ 15^s{\cdot}5$, and the eclipse ended at $4^h\ 57^m\ 57^s{\cdot}8$; but by that time the clouds had become so much thicker as to impair the definition of the sun's limb, which rendered it difficult to observe the end of the eclipse with accuracy.

The observations of the different phases of the eclipse, along with Lieutenant Pettersson's observations, which he has kindly placed at my disposal, will be found at p. 346.

After the totality, the appearance of the sky was greatly altered. Its warm tint before the commencement of the eclipse had given place to a cold gray; and the cumulous clouds in the horizon had changed to stratous clouds, which now overspread the whole of the sky. At about $4^h\ 55^m$ a large halo formed round the sun, and everything indicated a great change in the meteorological conditions of the atmosphere. The weather gradually became more gloomy, and there was heavy rain in the evening.

The observations of temperature contained in Table V., (p. 346), were made by means of two small thermometers by Adie. Their scales are trustworthy; and on comparison with Mr Adie's standard thermometer, were found correct to the 10th of a degree. The thermometers were hung on pieces of wood stuck in the ground, and were sheltered from the sun by a rock.

Neither Mr Lane nor myself had any opportunity of witnessing the effects of the eclipse on the lower animals; as there were no cattle or birds on the hill near our station.

* If this experiment be ever repeated, it should be performed in an apartment, or by means of a box adapted to exclude the general light of the atmosphere. The candle should be carefully preserved in order to compare its light with that of the moon.

One reason which induced us to select a station on the opposite side of the river from Göteborg, was to avoid what HALLEY in his account of the solar eclipse of 1715, quaintly terms, being "opprest by too much company." We were, therefore, not a little disconcerted at finding a large number of people resorting to the hill we had chosen. But the fears we entertained of being interrupted proved quite groundless, for with much propriety of feeling, every one kept at a respectful distance during our observations. When the eclipse was over, a venerable Swedish clergyman came up and shook hands with us; an example which was followed by a good number of his countrymen who were present. We did not understand each other's language, but it was not necessary that we should. Our mutual congratulation, although silent, was quite intelligible, and I am sure it was warmly felt on both sides.

TABLE I. *Magnetic Bearings from Ramberget, the station from which the Eclipse was observed.*

STATION.	Mag. Azimuth, reckoned from North, Eastward.
Lejonet, ...	107° 45′
Christine Kyrkan, . .	135 45
Domkyrkan, . . .	140 30
Kronan, ...	168 45
Carl Johan's Kyrkan, . .	216 15
Nya Warfvet Telegraph, .	227 53
Elfsborg, ...	257 00

As the station, *Elfsborg* is not included in my map, I could not employ its azimuth in my calculation. The position of Ramberget was determined from the remaining six azimuths along with the following data kindly furnished by Lieutenant PETTERSSON from his own observations.

The Navigation School is situated in lat. 57′ 42′ 6″·2 N.; long. $0^h\ 47^m\ 51^s$ E. The *true* azimuth of a telegraph shewn in the map, on a hill named *Stigbergsasen*, as seen from the Navigation School, is 108° 56′ 55″ NW.

The variation of the compass at Göteborg on the 1st April 1851, was found to be 17° 15′ W.

TABLE II. *Observations of Red Prominences and Spots on the Sun.*

	Göteborg M. T.	Observed angle from Sun's vertex.	Calculated angle from Sun's north point.
Group of spots, 1′·5 from Sun's limb,	$1^h\ 37^m$	96° 30′ west.	288° 47′
Single spot, 1′ from limb, . .	1 40	62 00 east.	- 87 17
Hook-shaped red prominence, .	About 3 58	110 30 west.	282 8
Serrated prominence, . .	About 3 58	132 40 west.	259 58
Bright rays in corona, . .	About 3 58	28 30 east.	61 8

TABLE III. *Phases of the Eclipse observed by MM. Swan and Lane, in Lat.* 57° 42′ 57″·3 N.; *Long.* $0^h\ 47^m\ 45^s{\cdot}2$ E.

First external contact,	2^h	53^m	$4^s{\cdot}4$	About 2^s late.
Beginning of totality,	3	55	52 ·6	
End,	3	59	8 ·1	
Last external contact,	4	57	57 ·8	Probably too late.

TABLE IV. *Phases of the Eclipse observed by Lieutenant Pettersson, in Lat.* 57° 42′ 6″·2 N. *Long.* $0^h\ 47^m\ 51^s$ E.

First external contact,	2^h	53^m	$3^s{\cdot}9$	
Beginning of totality,	3	55	58 ·2	Too late.
End,	3	59	8 ·2	
Last external contact,	4	58	2 ·6	Difficult to observe.

TABLE V. *Thermometrical Observations.*

Times.	Dry Ther.	Wet Ther.	
$2^h\ 45^m$	66°	60°	
2 53	...	...	First external contact.
3 0	64	59	
3 15	62	57·5	
3 30	61	56·6	
3 45	60	57	
3 50	57·8	55·5	
3 56	...	...	Beginning of totality.
3 59	...	...	End of totality.
4 10	57	55	
4 30	58·5	56	
4 45	60	57	
4 55	62·3	59·5	
4 58	...	...	Last external contact.
5 5	62	58·5	
5 30	62	57·5	

XXII.—*Researches on some of the Crystalline Constituents of Opium.*

By Thomas Anderson, M.D., F.R.S.E.

(Read 5th April 1852.)

Since the year 1803, when Desrone discovered the substance which afterwards received the name of Narcotine, the chemical investigation of opium has engaged the attention of many skilful and distinguished chemists, and in their hands has proved the source of a series of substances, unprecedented in their number and the variety of their properties. Up to the present time, there have been detected in it no less than eleven different substances,* one acid, and ten, either basic or indifferent, all presenting definite characters and crystalline form, besides various imperfectly characterised substances, described under the names of caoutchouc of opium, resin of opium, extractive, and the like.

With these facts before us, the chemistry of opium may appear at first sight to be almost exhausted, and that little remains to be done, except to fill up the minor details of former investigations. But when we come to inquire more minutely into its history, the meagre and even conflicting statements of different investigators, sufficiently indicate the imperfections of their researches, and the necessity of revising and greatly extending their inquiries before our knowledge can be considered as either definite or satisfactory. The most remarkable constituents of opium were detected a number of years since, at the time when attention was first directed to the existence of peculiar constituents on which the active properties of vegetables depended; and since their discovery, comparatively little has been done to confirm the original observations, which are often unsatisfactory, and serve not so much to supply definite facts, as to indicate the direction in which they are to be sought for.

Some conception of the limited extent of our information regarding opium may be formed by a few preparatory statements as to our present knowledge of its basic and indifferent constituents, amounting, as has been already mentioned, to ten in number. Of these, four have been repeatedly examined within the last few years, and their constituents may now be considered as conclusively established. These are:—

* Since this paper was written, two new substances have been added to the number of the constituents of opium; these are methylonarcotine and propylonarcotine, which have been recently described by Wertheim.

Morphine, ...	$C_{34}\ H_{19}\ NO_6$
Codeine, ...	$C_{36}\ H_{21}\ NO_6$
Paparcine, ...	$C_{40}\ H_{21}\ NO_8$
Narcotine, ...	$C_{46}\ H_{25}\ NO_{14}$

And the products of decomposition have been entered upon in some detail in the case of narcotine and codeine, but are still entirely unexamined in the other two.

Of the remaining six, porphoroxine is as yet unanalysed; opianine is only of recent discovery, and the details of its analysis not having yet been published, the formula given for it must still be considered doubtful. The other four have been submitted to analysis, most of them at the time of their discovery, but the results obtained are very imperfect, and not of a character to inspire much confidence in their accuracy. The following are the formulæ which have been most generally adopted for these substances:—

Thebaine, ...	$C_{25}\ H_{14}\ NO_3$
Pseudomorphine, ...	$C_{27}\ H_{18}\ NO_{24}$
Narceine, ...	$C_{28}\ H_{20}\ NO_{12}$
Opianine, ...	$C_{66}\ H_{36}\ NO_{23}$
Meconine, ...	$C_{10}\ H_5\ O_4$

These formulæ are very far from being satisfactory; indeed, most of them are purely empirical, and even in those instances in which the atomic weight has been determined, it has been done according to some of the older methods, on which much dependence could not at any time be placed, and which are now entirely superseded by more accurate and satisfactory methods of experiment. As far as their physical properties are concerned we have tolerably—though only tolerably—accurate information; but of their products of decomposition absolutely nothing is known except in the case of meconine, on which we have just sufficient information to shew how much interesting matter lies ready for investigation.

The following paper contains the result of a pretty extended investigation of some of the constituents of opium, to which my attention has been directed by the facilities afforded, by an extensive morphia manufactory, of obtaining products which, though commercially little better than refuse, are of much interest in a scientific point of view.

For the preparation of the bases which form the subject of my investigation, I have made use of the mother liquors of the preparation of muriate of morphine by the process of Robertson and Gregory. This method, as is well known, consists in precipitating the aqueous infusion of opium with a solution of chloride of calcium, filtering from the meconate of lime and evaporating the solution to a small bulk. On cooling, crystals of muriate of morphine are deposited, which are separated by expression, and the mother liquor again evaporated. A fresh crop of crystals is thus obtained, and the evaporation is continued as long as muriate of

morphia is deposited. As the final result of these processes there is obtained a thick fluid, perfectly black, and of the consistence of tar, which formed the raw material for my investigation.

I. *Preparation of the Bases.*

The black mother liquor just referred to is diluted with water, and filtered through cloth in order to separate a small quantity of a brown flocky matter which is deposited. To the filtered fluid ammonia is added as long as a precipitate is obtained, and the whole is strained through a cloth filter, and the precipitate subjected to strong pressure. The precipitate thus obtained is of a rather dark-brown colour, and granular, but if left in the press for any length of time, is apt to run together into a resinous mass. It must, therefore, be rapidly removed before this change has taken place, broken up with the hands in a fresh quantity of water, and again expressed; and this is repeated several times, until the fluid which runs off is no longer dark-coloured. The precipitate consists principally of narcotine, along with a considerable quantity of resin and a small quantity of thebaine; the filtrate contains narceine, and must be preserved for the preparation of that substance.

A portion of the precipitate is boiled with rectified spirit and filtered hot; on cooling, impure and very dark-coloured crystals of narcotine are deposited, which are collected on a cloth washed with a small quantity of cold alcohol and expressed. The mother liquor of these crystals is then employed for the solution of a fresh quantity of the precipitate, the crystals obtained washed and expressed as before, and the operation repeated until the whole precipitate has been treated in the same way. The impure crystals of narcotine are then reduced to powder and rubbed into a paste with a concentrated solution of caustic potash. After standing for some time a large quantity of water is added, and the narcotine is deposited in a much less coloured state, the resinous impurities being retained in solution by the potash. The solution is then poured off, the precipitate of narcotine washed with water, and finally purified by several crystallisations from boiling alcohol.

The alcoholic solution from which the first dark-coloured crystals of narcotine were deposited, on being distilled in the water-bath, leaves behind a considerable quantity of a dark amorphous mass containing much resin mixed with a little narcotine and the whole of the thebaine present in the original precipitate. This residue is treated with hot dilute acetic acid, which leaves behind a large quantity of resinous matter, and dissolves the two bases, along with a certain quantity of resin. After several trials, I found that subacetate of lead afforded the best means of obtaining the thebaine in a state of purity from this solution. When subacetate of lead is added to the acetic solution until the reaction becomes dis-

tinctly basic, the whole of the resin and narcotine are precipitated, while the thebaine remains in the solution. The fluid is filtered from the precipitate, and the excess of lead thrown down by means of sulphuric acid, the sulphate of lead separated by filtration, and ammonia added, when there is immediately obtained a more or less brown precipitate of thebaine, which is collected on a filter, washed, dried, and dissolved in boiling alcohol. The solution, which is generally very dark-coloured, becomes filled, on cooling, with flattened crystals of thebaine. The mother liquor is separated by expression, and the crystals, after boiling with animal charcoal and several crystallisations from boiling spirit, constitute pure thebaine.

The mother liquor of the original ammonia precipitate, as has been already mentioned, contains narceine, for the separation of which I have found it most convenient to proceed in the following manner. A solution of acetate of lead is added to the fluid, and the dirty brownish precipitate which appears is separated by filtration through cloth. The excess of lead is removed by means of sulphuric acid, and the fluid filtered from the sulphate of lead, after being saturated with ammonia, is set to evaporate on the sand-bath at a moderate temperature. If the operation has been properly conducted, a film appears on the surface at a certain degree of concentration, and on cooling, a quantity of a crystalline matter is deposited in the thick brown mother liquor, which increases somewhat on being allowed to stand for some days. When this substance is collected on a cloth, and washed with a small quantity of water, it is sometimes obtained perfectly colourless at once, but more generally has a brownish colour. By farther evaporation of the mother liquor an additional quantity of crystals is obtained. The crystals are then boiled with a large quantity of water, and the solution, filtered hot, becomes filled on cooling with fine silky needles of narceine, while a large quantity of sulphate of lime and other impurities remain on the filter. The crystals of narceine have generally a slight shade of colour, and retain traces of sulphate of lime, from which they are purified by solution in alcohol, boiling with animal charcoal, and again crystallising from water.

II. *Narceine.*

Narceine was discovered by Pelletier,* about the year 1832, and to his own and Couerbe's† researches we owe all our present information regarding it. Both these observers have analysed it, but with results quite incompatible with one another, and from which they have deduced entirely different formulæ. Their analyses, when recalculated with the corrected atomic weight of carbon, gave the following results:—

* Annales de Chimie et de Physique, vol. l., p. 262. † *Ibid.*, vol. lix., p. 151.

	Pelletier.	Couerbe.	
Carbon, . .	54·02	56·42	56·00
Hydrogen, .	6·52	6·66	6·62
Nitrogen, .	4·33	4·76	...
Oxygen, . .	35·13	32·16	...
	100·00	100·00	

From which Pelletier has deduced the formula $C_{32}H_{24}NO_{16}$, and Couerbe that of $C_{28}H_{20}NO_{12}$, of which the respective calculations are given below.

	Pelletier's Formula.	Couerbe's Formula.
Carbon, . .	53·63	56·37
Hydrogen, . .	6·70	6·71
Nitrogen, . .	3·91	4·69
Oxygen, . .	35·76	32·23
	100·00	100·00

Couerbe's formula agrees extremely well with his analytical results, but it, as well as Pelletier's, is entirely unsupported by determination of the atomic weight, which neither of them seem to have attempted, owing to the impression they derived from their experiments, that narceine does not possess basic properties, an opinion which I have found to be altogether incorrect.

III. *Analysis of Narceine.*

The analysis of narceine was made upon a quantity which had been purified by repeated solution both in water and alcohol, and which was absolutely white. It loses its water with great difficulty at 212°, and it is most convenient to dry it at 230°.

I. { 4·632 grains of narceine gave
10·130 ... carbonic acid, and
2·660 ... water.

II. { 4·861 grains of narceine gave
10·522 ... carbonic acid, and
2·824 ... water.

{ 5·650 grains of narceine gave
2·790 ... platinochloride of ammonium.

{ 7·236 grains of narceine gave
3·845 ... platinochloride of ammonium.

	Experiment.		Calculation.		
	I.	II.			
Carbon, . .	59·64	59·03	59·63	C_{46}	276
Hydrogen, .	6·38	6·45	6·28	H_{29}	29
Nitrogen, .	3·10	3·30	3·02	N	14
Oxygen, . .	30·88	31·22	31·09	O_{18}	144
	100·00	100·00	100·00		463

These results correspond exactly with the formula $C_{46}H_{29}NO_{18}$, as is obvious from their comparison with the calculated numbers given above. The atomic weight was determined by the analysis of its platinum salt, which is a very characteristic compound, and which gave, as the mean of three experiments, 14·56 per cent. of platinum, giving, for the atomic weight of the base, 464·8, and corresponding perfectly with 463, the calculated number.

IV. *Properties of Narceine.*

Narceine crystallises in delicate needles which mat together into a very light and bulky mass, with a brilliant silky lustre. These crystals are always extremely white; indeed, narceine is remarkable for the facility with which it is obtained colourless, and while all the other crystalline principles of opium retain colour with considerable obstinacy, it may, with ordinary care, be obtained colourless by a few crystallisations, and in some cases is deposited in that state even from the highly-coloured mother liquor of the ammoniacal precipitate. In cold water it is sparingly, but in hot readily, soluble, and the solution on cooling becomes filled with a network of bulky crystals. In alcohol it is still more soluble, and is deposited from the hot fluid in needles which are generally shorter, thicker, and less silky, than those obtained from water. It is insoluble in ether. Ammonia and dilute solutions of potash and soda dissolve it in larger proportion than water, but the addition of a large quantity of concentrated potash to the dilute solution, precipitates it, even in the heat, in the form of an oily mass, which remains fluid for some time under the solution. The potash fluid, on standing for some time, deposits unchanged narceine, in the form of shining plates, which, by recrystallisation, again acquire the acicular form. It dissolves in dilute sulphuric, nitric, and hydrochloric acids, without undergoing any change, and the solutions if sufficiently concentrated, deposit crystalline salts of narceine.

When boiled with dilute nitric acid, the solution acquires a yellow colour, which on saturation with potash becomes reddish-brown, and the odour of a volatile base is immediately evolved. Concentrated nitric acid acts violently in the cold with copious evolution of nitrous fumes; after boiling for some time it gives on dilution a whitish precipitate, soluble in ammonia, and the fluid contains oxalic acid. Strong sulphuric acid dissolves it in the cold with an intense red colour, which on the application of heat passes into a dark green. Strong hydrochloric acid dissolves it entirely, and without producing the blue colour which is described by PELLETIER as characteristic of narceine. I did obtain a blue colour on one occasion, but it was when operating on a very small scale, and when the narceine was not absolutely pure; but on repeating and varying the experiment in every possible way with the pure base, I have never again succeeded in producing it. I have been equally unsuccessful with a quantity of narceine which I obtained

direct from the establishment of Messrs Robiquet, Pelletier, and Caventou, in Paris; but I am informed by Professor Heinrich Rose of Berlin, that he possesses a specimen from the same source, which shews a feeble blue.*

The specimen of narceine which I obtained from Paris, though closely agreeing in character with that which I had myself prepared, presented some minor differences, and analysis shewed that its constitution was entirely different.

4·458 grains of Robiquet's narceine gave
10·250 ... carbonic acid, and
2·620 ... water.

3·340 grains of Robiquet's narceine gave
2·245 ... platinochloride of ammonium.

	Experiment.	Calculation.		
Carbon	62·70	62·95	C_{32}	192
Hydrogen	6·53	6·23	H_{19}	19
Nitrogen	4·22	4.58	N	14
Oxygen	26·45	26·24	O_{10}	80
	100·00	100·00		305

These results correspond exactly with the formula C_{32} H_{19} NO_{10} of which, however, I have no means of confirming the correctness. I attempted to form a platinum salt, but the fluid, in place of depositing a crystalline salt, solidified into a thin jelly, which I did not think deserving of analysis. The high price of the substance (nine francs per gramme), has deterred me from attempting a more extended examination.

V. *Salts of Narceine.*

According to Pelletier and Couerbe, narceine, though dissolved by the acids, is deposited unchanged from the solutions. In this, however, their results do not agree with mine. Though incapable of restoring the blue of reddened litmus, narceine is a feeble base, and its solutions in acids deposit crystalline salts of well-marked characters.

Hydrochlorate of Narceine.—When narceine is mixed with water, and hydrochloric acid is added, it rapidly dissolves, and on standing deposits large groups of radiated silky needles. These needles, if collected on a filter and left for some time, occasionally pass into a congeries of short, thick, irregular prisms, and similar crystals are deposited by spontaneous evaporation in dilute solutions. These crystals are readily soluble in water and alcohol, and their solution has a

* Dr Traill has since informed me that a specimen in his collection gives a fine blue with hydrochloric acid; so that the product sold in Paris as narceine, would appear to be very variable in its properties.

distinctly acid reaction. Dried at 212° the salt gave the following results to analysis:—

{ 4·692 grains of hydrochlorate of narceine gave
9·520 ... carbonic acid, and
2·710 ... water.

{ 4·477 grains hydrochlorate of narceine gave
1·274 ... chloride of silver.

	Experiment.	Calculation.		
Carbon .	55·31	55·25	C_{46}	276
Hydrogen .	6·41	6·00	H_{30}	30
Nitrogen .	...	2·80	N	14
Oxygen .	...	28·85	O_{18}	144
Chlorine .	7·04	7·10	Cl	35·5
		100·00		499·5

And the formula of the salt is consequently $C_{45}\ H_{29}\ NO_{18}\ H\ Cl$.

Sulphate of Narceine is deposited from its solution in tufts of silky needles, not differing much in appearance from the base itself. It is of rather sparing solubility in cold water, but dissolves abundantly in hot.

Nitrate of Narceine.—The nitrate is deposited in radiated groups from a hot solution of narceine in dilute nitric acid. It is sparingly soluble in the cold.

Chloride of Platinum and Narceine.—When a solution of chloride of platinum is added to hydrochlorate of narceine, the double compound is deposited sometimes in prismatic crystals of small size, and sometimes as a crystalline powder, of a dark orange colour. The salt was dried for analysis at 212° and gave the following results:—

I. { 7·635 grains of platinochloride of narceine gave
11·576 ... carbonic acid, and
3·188 ... water.

II. { 6·448 grains of platinochloride of narceine gave
9·698 ... carbonic acid, and
2·675 ... water.

9·713 grains of platinochloride gave 1·392 grains of platinum.
6·829 ... 1·008 ...
6·195 ... 0·907 ...

	Experiment.			Calculation.		
	I.	II.	III.			
Carbon,	41·08	41·01	...	41·24	C_{46}	276
Hydrogen,	4·60	4·60	...	4·48	H_{30}	30
Nitrogen,	...	...	...	2·09	N	14
Oxygen,	...	...	...	21·51	O_{18}	144
Chlorine,	...	...	...	15·94	Cl_3	106·5
Platinum,	14·33	14·76	14·64	14·74	Pt	98·7
				100·00		669·2

The formula $C_{46}H_{30}NO_{18}H\,Cl\,PtCl_2$ expresses completely the results of experiment.

VI. *Thebaine.*

Thebaine was discovered in 1832, and was examined and analysed by PELLETIER,* who gave to it the name of Paramorphine, expressive of its isomerism with morphine, which he supposed to be established by his analysis. It was afterwards examined by COUERBE† and by KANE,‡ with results differing widely from one another and from PELLETIER, and each has deduced from his analysis a different formula, none of which can be considered as agreeing in a satisfactory manner with the analytical numbers, as is obvious from the following tabular view of their analyses, recalculated according to the corrected atomic weight of carbon, and compared with the formula deduced from them and the theoretical numbers which they ought to give.

	PELLETIER.	COUERBE.		KANE.	
Carbon,	71·09	71·07	70·96	73·39	73·07
Hydrogen,	6·29	6·47	6·44	6·78	6·85
Nitrogen,	4·40	6·38	...	6·94	...
Oxygen,	17·22	16·08	...	12·89	...
	100·00	100·00		100·00	

PELLETIER's formula, . $C_{34}\ H_{18}\ NO_6$
COUERBE's, . . . $C_{25}\ H_{13\cdot5}\ NO_4$
KANE's, . . . $C_{25}\ H_{14}\ NO_3$

Calculation—

	PELLETIER'S Formula.	COUERBE'S Formula.	KANE'S Formula.
Carbon,	71·83	71·59	74·25
Hydrogen,	6·34	6·44	6·93
Nitrogen,	4·93	6·68	6·93
Oxygen,	16·90	15·29	11·89
	100·00	100·00	100·00

The atomic weight has been determined by COUERBE and KANE by ascertaining the amount of hydrochloric acid absorbed by the dry base. Their results, however, differ in a very remarkable manner, and do not admit of any conclusion or satisfactory deductions being made from them. COUERBE, who does not give any particulars as to the method in which his experiment was made, found that 100 parts of base absorb 8·35 of the acid. KANE, on the other hand, found that when the hydrochloric acid was passed into thebaine, at the temperature of 212°, it absorbed as the mean of two experiments, which, however, do not agree very well, 16·96 per cent. of the dry gas; but that when the absorption took place at

* Journal de Pharmacie, vol. xxi., p. 569.
† Annales de Chimie et de Physique, vol. lix., p. 155.
‡ Annalen der Chemie, vol. xix., p. 9.

ordinary temperatures, 33·28 per cent. was taken up. These results are very unintelligible, and certainly cannot be employed as the foundation of an atomic weight. It is, however, worthy of observation, that they are very nearly in the ratio of 1, 2, and 4, but this relation must be purely fortuitous, as I have found that thebaine is very easily decomposed by hydrochloric acid, and none of the results agree at all with the actual atomic weight, as deduced from the experiments which I am about to detail.

VII. *Analysis of Thebaine.*

The thebaine employed for analysis was prepared by the process already described, and purified by repeated crystallisation; it was burned with oxide of copper, and is very easily combustible.

I. { 5·475 grains of thebaine, dried at 212°, gave
14·675 ... carbonic acid, and
3·500 ... water.

II. { 4·990 grains of thebaine gave
13·383 ... carbonic acid, and
3·135 ... water.

III. { 5·089 grains of thebaine, of another preparation, gave
13·621 ... carbonic acid, and
3·228 ... water.

{ 5·336 grains of thebaine gave
3·735 ... platinochloride of ammonium.

{ 6·332 grains of thebaine gave
4·515 ... platinochloride of ammonium.

	I.	II.	III.
Carbon, .	73·10	73·14	73·01
Hydrogen, .	7·10	6·98	7·04
Nitrogen, .	4·39	4·47	...
Oxygen, .	15·41	15·41	...
	100·00	100·00	

These results correspond exactly with the formula,

$$C_{38}\ H_{21}\ NO_6$$

differing from that of codeine by two equivalents of carbon, as is seen from the following comparison of the experimental mean with the calculated result of that formula.

	Mean.	Calculation.		
Carbon, .	73·08	73·31	C_{38}	228
Hydrogen, .	7·04	6·75	H_{21}	21
Nitrogen, .	4·43	4·50	N	14
Oxygen, .	15·45	15·44	O_6	48
	100·00	100·00		311

Some difficulty was at first experienced in the determination of the atomic weight of thebaine by the analysis of its platinum salt, until it was ascertained that that salt, when dried at 212°, retains two equivalents of water. The mean of three determinations of platinum gave 18·70 per cent. of the metal, and the calculated result for the formula,

$$C_{38}\,H_{21}\,NO_6,\,H\,Cl,\,Pt\,Cl_2 + 2\,HO$$

is 18·44. These results were also confirmed by the analysis of the hydrochlorate, of which the details will be given in their proper place.

Properties of Thebaine.

Thebaine crystallises from its alcoholic or ethereal solution in brilliant square plates with a silvery lustre. It is insoluble in water, but very soluble in alcohol and ether, especially on boiling. It dissolves readily in acids, and forms salts which are not obtained in crystals from aqueous solutions. It is insoluble in potash and ammonia. Strong sulphuric acid reacts upon it, and produces a deep-red colour, even when it is free from nitric acid. Concentrated nitric acid acts violently in the cold, with copious evolutions of red fumes, and formation of a yellow solution, which becomes dark coloured on the addition of potash, and evolves a volatile base. In hydrochloric acid it dissolves readily, and the solution on evaporation becomes dark coloured, and leaves behind a resinous matter, which does not dissolve completely in water. Sulphuric acid, of specific gravity 1·300, dissolves it in the cold; and on gently heating a resinous or semisolid matter is thrown down, which, on boiling with water, slowly dissolves, and deposits, on cooling, a rather sparingly soluble salt, in microscopic crystals, which appears to be a product of decomposition, but of which I must defer the examination until I have obtained an additional quantity of thebaine. Chlorine and bromine rapidly decompose thebaine with the formation of resinous compounds.

VIII. *Salts of Thebaine.*

The small quantity of thebaine which I had at my disposal has prevented my extending the examination of its salts as far as I could have wished, and I have only examined such as are necessary for the determination of its atomic weight, and must reserve further details for a future paper.

Hydrochlorate of Thebaine.—In order to prepare this salt, thebaine is mixed with a small quantity of strong spirit, and an alcoholic solution of hydrochloric acid gas is gradually added until the thebaine is dissolved, an excess being carefully avoided. On standing for some time, the hydrochlorate is deposited in extremely brilliant rhomboidal crystals, often of considerable size, or as a crystalline powder if the solution be agitated. These crystals are purified by resolution in absolute alcohol. They are extremely soluble in water, and the solution, on evaporation, gives only a resinous mass. In alcohol, especially if absolute,

they are rather sparingly soluble, and in ether they do not dissolve. Dried at 212°, their analyses gave

I. { 4·108 grains of hydrochlorate of thebaine gave
9·398 ... carbonic acid, and
2·515 ... water.

II. { 5·356 grains of hydrochlorate of narceine gave
12·215 ... carbonic acid, and
3·192 ... hydrogen.

{ 3·620 grains of hydrochlorate of narceine gave
1·517 ... chloride of silver.

{ 5·355 grains of hydrochlorate of narceine gave
2·085 ... chloride of silver.

	Experiment.		Calculation.		
	I.	II.			
Carbon, .	62·39	62·19	62·38	C_{38}	228
Hydrogen, .	6·80	6·62	6·56	H_{24}	24
Nitrogen, .	...	...	3·83	N	14
Oxygen, .	...	...	17·52	O_8	64
Chlorine, .	10·36	9·63	9·71	Cl.	35·5
			100·00		365·5

The salt, dried at 212°, contains, therefore, two equivalents of water, and is represented by the formula, $C_{38}\ H_{21}\ NO_6\ H\ Cl + 2\ HO$.

Platinochloride of Thebaine.—The platinum compound is thrown down as a yellow crystalline precipitate on the addition of bichloride of platinum to the preceding compound. It is sparingly soluble in boiling water, and the solution deposits a salt which appears to be a product of decomposition.

I. { 5·555 grains of platinochloride of thebaine, dried at 212°, gave
8·735 ... carbonic acid, and
2·180 ... water.

II. { 5·418 grains of platinochloride of thebaine, dried at 212°, gave
8·593 ... carbonic acid, and
2·315 ... water.

5·037 grains of platinochloride of thebaine gave 0·927 grains of platinum.
4·998 ... ... 0·936 ...
5·793 ... ... 1·100 ...

	Experiment.			Calculation.		
	I.	II.	III.			
Carbon, .	42·88	43·25	...	42·60	C_{38}	228
Hydrogen, .	4·36	4·74	...	4·48	H_{24}	24
Nitrogen, .	...	...	...	2·61	N	14
Oxygen, .	...	...	...	11·98	O_8	64
Chlorine, .	...	...	...	19·89	Cl_3	106·5
Platinum, .	18·43	18·72	18·98	18·44	Pt	98·7
				100·00		535·2

The formula of the salt is therefore $C_{38}\ H_{21}\ NO_6\ H\ Cl\ Pt\ Cl_2 + 2\ HO$.

Deficiency of time and material have prevented the full examination of any other salts of thebaine. The sulphate was prepared by adding sulphuric acid to an ethereal solution of thebaine; it was deposited partly in crystals, partly as a resinous mass which became crystalline on standing. This was dissolved in absolute alcohol, and thrown down by ether as a white powder. A determination of sulphuric acid gave 16·53 per cent., which is not far removed from the quantity required by theory for a sesquisulphate.

Hydrochlorate of thebaine gives, with a spiritous solution of corrosive sublimate, a fine white crystalline precipitate of a double salt, and the alcoholic solution of the base itself gives a bulky precipitate insoluble in water and alcohol; neither of these substances, however, could be got of constant composition.

Terchloride of gold gives a fine reddish-orange precipitate, which, at 212°, fuses into a resinous mass.

IX. *Action of Nitric Acid on Narcotine.*

Narcotine has been already repeatedly analysed, and its constitution satisfactorily determined. I have not, therefore, attempted to repeat its analysis, or to add any confirmatory evidence of the correctness of its formula, but have directed my attention to the action of nitric acid upon it, which I had found, by previous experiments, to give a series of products varying with the circumstances of the action and the concentration of the acid. When proper precautions are taken, the whole series of products which Wöhler discovered by the action of peroxide of manganese and sulphuric acid upon narcotine, are obtained along with several new substances, which stand in very intimate relation to these compounds, and are peculiarly remarkable, both in their chemical relations and the circumstances under which they are produced.

When concentrated nitric acid is added to narcotine, a very violent action ensues, even in the cold; red fumes are copiously evolved, and a thick resinous-looking red matter is left behind. With somewhat weaker acid and a gentle heat, a similar action takes place, and a red fluid is obtained, which, by evaporation, yields an amorphous orange residue. In both cases, the action was much too violent, and the product obtained obviously the result of several complex actions. The action of nitric acid in a more dilute state was therefore tried and after several experiments, the following was found to be the most advantageous method of treatment. Six hundred grains of narcotine are mixed with two-and-a-half ounces, by measure, of nitric acid, of specific gravity 1·400, diluted with ten ounces of water, and exposed in the water-bath to an uniform temperature of 120° Fahr. The narcotine fuses into a yellowish mass, which, by continuous agitation, slowly dissolves without the evolution of red fumes. When the solution is nearly complete, a small quantity of a white deposit begins to make its appear-

ance in the solution, and gradually increases in quantity until the fluid becomes filled with bulky crystalline flocks. The quantity of this substance produced appears to depend, to a great extent, upon the rapidity of the oxidation, being sometimes extremely minute, and always bearing a very small proportion to the quantity of narcotine employed. When these flocks have ceased to increase in quantity, they are separated from the fluid by filtration through asbestos, washed with water, in which they are insoluble, and purified by solution in a considerable quantity of boiling alcohol, from which they are deposited on cooling in minute needles. To this substance I give the name of Teropiammon, for reasons which will be immediately apparent.

Teropiammon.—As obtained by the process just described, teropiammon is in the form of extremely small colourless needles. It is insoluble in water, both hot and cold, and undergoes no decomposition by boiling with that fluid. It is very sparingly soluble in cold alcohol, more so in boiling; and it is also very little soluble in ether. Concentrated sulphuric acid dissolves a small quantity in the cold, with a yellow colour, and on heating a fine crimson colour is produced. Nitric acid dissolves it readily in the cold; and on heating, red fumes are evolved, and on dilution with water, a white precipitate of teropiammon in an altered condition is obtained. It is insoluble in hydrochloric acid and in ammonia. Boiled with caustic potash, it dissolves with evolution of ammonia, and opianic acid is found in the fluid. At my first examination, I considered this substance to be identical with WÖHLER'S* opiammon, but the entire absence of xanthopenic acid in this reaction, as well as various other differences in its properties, convinced me that it was actually different,—a conclusion which has been confirmed by analysis.

I. { 5·052 grains of teropiammon, dried at 212°, gave
10·960 ... carbonic acid, and
2·262 ... water.

II. { 5·090 grains of teropiammon gave
11·020 ... carbonic acid, and
2·290 ... water.

I. { 4·358 grains of teropiammon gave
1·515 ... platinochloride of ammonium.

II. { 7·312 grains of toropiammon gave
2·405 ... platinochloride of ammonium.

	Experiment.		Calculation.		
Carbon, .	59·16	59·04	58·91	C_{60}	360
Hydrogen, .	4·97	4·99	4·74	H_{29}	29
Nitrogen, .	2·18	2·06	2·29	N	14
Oxygen, .	33·69	33·91	34·06	O_{26}	208
	100·00	100·00	100·00		611

* Annalen der Chimie und Pharmacie, vol. l., p. 6.

These numbers correspond very closely with the formula $C_{60}H_{29}NO_{26}$, as is obvious from the comparison of the calculated results of that formula given abov e That it is actually different from WÖHLER'S opiammon, of which the formula is $C_{40}H_{19}NO_{16}$, is very obvious, but it bears a very interesting relation to it. The latter substance is derived from two equivalents of opianic acid and one equivalent of ammonia, by the removal of the elements of four equivalents of water as thus represented:—

2 eq. opianic acid, . . .	$C_{40}H_{20}O_{26}$
1 eq. ammonia, ...	H_3N
	$C_{40}H_{23}NO_{20}$
4 eq. water, ...	H_4O_4
1 eq. opiammon, . . .	$C_{40}H_{19}NO_{16}$

and the new compound is derived in a precisely similar manner from three equivalents of opianic acid:—

3 eq. opianic acid, . . .	$C_{60}H_{30}O_{30}$
1 eq. ammonia, . . .	H_3N
	$C_{60}H_{33}NO_{30}$
4 eq. water, ...	H_4O_4
1 eq. teropiammon, . . .	$C_{60}H_{29}NO_{26}$

Both these substances may therefore be considered as a sort of nitriles of opianic acid, at least they bear to the opianates of ammonia a similar relation to that which the nitriles hitherto examined do to the ammonia salts from which they are obtained. That this is actually the constitution of teropiammon, is proved by the action of potash, which, when boiled with it, produces an abundant evolution of ammonia, while the fluid contains an acid, which was found by its properties, as well as by an analysis, of which the details will be given under another head, to be opianic acid. It is in consideration of this constitution, that I give to the substance the name of *teropiammon*, while I should propose that of *binopiammon* for the substance described by WÖHLER, reserving that of *opiammon* for the corresponding compound derived from one equivalent of opianic acid and ammonia, should that substance be discovered, which is by no means improbable. The production of teropiammon in a highly acid fluid must be considered as an extremely remarkable phenomenon, and one of which, so far as I know, we have no similar example. It is obviously the result of a secondary decomposition, produced by the further action of nitric acid on narcotine, which, as we shall immediately see, yields a great variety of curious and complex products; but it has appeared to me that the quantity obtained was largest when the action was most moderate, at least I have never succeeded in obtaining it more abundantly by continuing the action for a longer time, but rather the reverse.

The fluid from which teropiammon has been separated is pale yellow. When supersaturated with potash, it acquires a more or less dark colour; and on standing, and still more rapidly on agitation, deposits a quantity of pale-yellow crystalline grains. The mother liquor, which contains a large excess of potash, is separated by filtration, and the precipitate washed with water. It then presents all the characters of cotarnine, dissolves in the acids, with a red colour, gives highly-soluble salts, and is precipitated by potash and soda, but not by ammonia. Its identity was further determined by the following analysis of its platinum salt:—

{ 5·436 grains of platinochloride of cotarnine gave
{ 1·242 ... platinum.

	Experiment.	Calculation.		
Carbon, ...		35·68	C_{26}	156
Hydrogen, ...		3·20	H_{14}	14
Nitrogen, ...		3·20	N	14
Oxygen, ...		10·98	O_6	48
Chlorine, ...		24·37	Cl_3	106·5
Platinum, . .	22·84	22·57	Pt	98·7
		100·00		437·2

In this way cotarnine is obtained with extreme facility, and the process is greatly to be preferred to Wöhler's method of preparation. The sole precaution necessary is to avoid the application of too high a temperature during the action of the nitric acid, and to arrest the action as soon as the whole of the narcotine is dissolved. If the heat be too great or too long continued, the cotarnine itself undergoes decomposition, and yields products which will be described afterwards.

X. *Examination of the Potash Solution.*

In the alkaline fluid from which the cotarnine had been separated, it was natural to look for the opianic acid of Liebig and Wöhler, which, as the simultaneous product of the oxidation of narcotine, must almost of necessity be present. Its existence was accordingly soon ascertained; but it was also found that it was by no means the only or the invariable product of the action, but that different substances were obtained in different operations, even when the nitric acid was caused to act under what were supposed to be perfectly identical conditions. In some instances, opianic acid was entirely absent, and its place was taken by hemipinic acid, which was invariably obtained in greater or less quantity, even when opianic acid was present; and in other cases, substances appeared which could not be produced at will, and were only obtained when the conditions of the oxidation were very successfully fulfilled.

In order to obtain these substances, the alkaline fluid is evaporated on the sand-bath to a small bulk, and the nitre, which deposits on cooling, is separated

by filtration, and the evaporation repeated until as much as possible is separated. The remaining syrupy fluid, which contains a large quantity of carbonate of potash, is then boiled with successive quantities of rectified spirit, as long as anything is extracted, the alcohol is distilled off, and the residue mixed in the cold with an excess of hydrochloric acid. A precipitate makes its appearance, with characters differing according to the substances which happen to be present, and is sometimes crystalline, and sometimes a syrupy mass, which passes into the crystalline state on standing. This precipitate contains opianic acid, hemipinic acid, and in some instances two other substances, to one of which I give the name of Opianyl; and to the other that of Hydrate of Opianyl.

Opianyl.—This substance is only formed when the oxidation has been extremely gentle, and, though repeated trials have been made, it has been found impossible to moderate the action in such a way as to produce it at will. In order to obtain it in a pure state, the precipitate by hydrochloric acid, which has just been referred to, is dissolved in a large quantity of boiling water, and the solution allowed to cool. A crop of crystals is deposited which consists of opianyl along with some opianic acid, if the quantity of water employed have not been sufficiently large. These crystals are purified by solution in boiling water and in alcohol. In one instance opianyl was obtained along with hemipinic acid, and with only traces of opianic acid, and in that case its purification was conveniently effected by dissolving in boiling water, precipitating hemipinate of lead with a solution of neutral acetate of lead, washing the precipitate in boiling water, and evaporating to a small bulk, when opianyl was deposited in colourless crystals, which were purified by solution in boiling water.

Opianyl is thus obtained in long delicate needles, which, when pure, are perfectly colourless. They are sparingly soluble in cold water, and more soluble in boiling. When a quantity is boiled with a smaller amount of water than is required to dissolve it, the residue melts under the fluid; but it does not fuse at 212° in the water-bath, requiring, when dry, a temperature of 230° to produce its fusion, and, on cooling, it resolidifies at about 220°. In alcohol it is easily soluble. Ether takes it up readily, and, on evaporation, deposits it in brilliant groups of radiated needles. Concentrated sulphuric acid dissolves it in the cold, and forms a perfectly colourless solution, which, when heated, becomes of a beautiful and characteristic purple colour. Nitric acid, of specific gravity 1·400, dissolves it in the cold, and on dilution with water it is deposited unchanged. By boiling, red fumes are evolved, and the fluid no longer gives a precipitate on being diluted. Hydrochloric acid dissolves it in somewhat larger quantity than water. Solutions of potash, soda, and ammonia, do not dissolve it more abundantly than water. It is incapable of forming compounds with the metallic oxides, and contains no nitrogen.

I. { 5·590 grains of opianyl, dried at 212°, gave
12·605 ... carbonic acid, and
2·680 ... water.

II. { 5·895 grains of opianyl gave
13·350 ... carbonic acid, and
2·885 ... water.

III. { 5·886 grains of opianyl gave
13·307 ... carbonic acid, and
2·760 ... water.

	I.	II.	III.
Carbon, . . .	61·49	61·76	61·65
Hydrogen, . .	5·32	5·43	5·21
Oxygen, . . .	33·19	32·81	33·14
	100·00	100·00	100·00

These results correspond exactly with the formula $C_{20} H_{10} O_8$, as is obvious from the following comparison of the calculated numbers with the experimental mean :—

	Mean.	Calculation.		
Carbon, . .	61·63	61·85	C_{20}	120
Hydrogen, . .	5·32	5·15	H_{10}	10
Oxygen, . .	33·05	33·00	O_8	64
	100·00	100·00		194

Opianyl thus bears a very interesting relation to opianic and hemipinic acids, provided we assume for the former the formula as corrected by Berzelius, and, for the latter, an atomic weight twice as high as that assigned to it by Wöhler, both of which assumptions are consistent with analyses which will be detailed in the sequel. The three substances then stand as follows :—

Opianyl, ...	$C_{20} H_{10} O_8$
Opianic acid, ...	$C_{20} H_{10} O_{10}$
Hemipinic acid, ...	$C_{20} H_{10} O_{12}$

and appear as three successive degrees of oxidation of the same radical. I have not attempted to convert opianyl into opianic acid by oxidation, as the quantity at my disposal was not sufficiently large to admit of an accurate experiment, but no reasonable doubt can be entertained on that subject.

The derivation of opianyl from narcotine is abundantly simple. Two equivalents of hydrogen are oxidised by the nitric acid, and the narcotine splits up into opianyl and cotarnine, as is expressed in the following scheme :—

1 eq. of narcotine	$C_{46} H_{25} NO_{14}$
2 eq. of oxygen, ...	O_2
	$C_{46} H_{25} NO_{16}$

Yield

1 eq. of cotarnine, ...	$C_{26} H_{13} NO_6$
1 eq. of opianyl, ...	$C_{20} H_{10} O_8$
2 eq. of water, ...	$H_2 O_2$
	$C_{46} H_{25} NO_{16}$

The same scheme, with the addition of two or four equivalents of oxygen, represents also the mode in which opianic and hemipinic acids respectively are derived from narcotine, much more simply than it has been by BLYTH* in his paper on the action of bichloride of platinum on narcotine, who gives a scheme involving the evolution of carbonic acid. The appearance of this gas, which was actually observed by BLYTH during the action, has, however, always appeared to me to be the result of a secondary decomposition; and this view, I think, receives confirmation from the production of teropiammon, where nitric acid acts even in the most feeble manner on narcotine, and the formation of which must, of necessity, be attended by the evolution of carbonic acid.

If we pursue the relations of opianyl to narcotine, we shall find that these also are of a very interesting nature. By subtracting an equivalent of cotarnine from one of narcotine,

Narcotine, ...	$C_{46} H_{25} NO_{14}$
Cotarnine, ...	$C_{26} H_{13} NO_6$
	$C_{20} H_{12} O_8$

we find that the substance coupled with cotarnine to form narcotine may be considered as a *hydruret* of opianyl, or a substance bearing to opianyl a relation similar to that which alloxantin bears to alloxan, and the preparation of which in a separate form would be most interesting. The attempts which I have made to obtain it have, however, as yet proved abortive. I have tried the action of sulphuretted hydrogen upon opianyl, but no change took place, and also the fermentation of narcotine, but with equally little success. Although this hydruret of opianyl has not been obtained in a separate form, we have a corresponding compound in the sulphopianic acid of WÖHLER, which may be considered as hydruret of opianyl, in which the two equivalents of hydrogen are replaced by sulphur.

Hydruret of opianyl, . . .	$C_{20} H_{10} O_8 + H_2$
Sulphopianic acid, ...	$C_{20} H_{10} O_8 + S_2$

and in this point of view the latter substance deserves a further investigation. It bears certain analogies in properties to opianyl, and especially gives a purple colour when heated with sulphuric acid; but it appears to possess acid properties, although they are certainly very feeble.

Hydrate of Opianyl.—On one occasion, in acting upon narcotine with nitric acid, there was obtained a substance which closely resembled opianyl, but differed

* Annalen der Chimie und Pharmacie, vol. l., p. 29.

from it in fusing readily when introduced dry into the water-bath, which opianyl does not. Its fusing point is 205°. In all its other properties, however, it is quite identical with opianyl. It gives the same purple with sulphuric acid, and shews the same relations to solvents. Its analysis gave the following results:—

I. { 4·295 grains of hydrate of opianyl, dried at 212°, gave
9·265 ... carbonic acid, and
2·000 ... water.

II. { 5·955 grains of hydrate of opianyl, dried in vacuo, gave,
12·840 ... carbonic acid, and
2·905 ... water.

	Experiment.		Calculation.		
	I.	II.			
Carbon, . .	58·83	58·84	59·11	C_{20}	120
Hydrogen, . .	5·17	5·42	5·41	H_{11}	11
Oxygen, .	36·00	35·74	35·48	O_9	72
	100·00	100·00	100·00		203

These numbers correspond with the formula $C_{20}\,H_{10}\,O_8 + HO$. The quantity of this substance which I obtained was too small to admit of any detailed examination of its chemical relations.

Opianic Acid.—The fluid which has deposited opianyl yields, on evaporation, a crop of crystals of opianic acid, which are readily purified by solution in water or alcohol. Its properties are already so well known, that I have not thought it necessary to examine them further; but as the formula given by Wöhler is different from the one I have adopted, which is that of Berzelius, the following analyses, in which every care was taken to dry the substance thoroughly, may be of value as confirming the correction of the latter chemist:—

I. { 5·343 grains of opianic acid, dried at 212°, gave
11·170 ... carbonic acid, and
2·440 ... water.

II. { 4·528 grains of opianic acid gave
9·485 ... carbonic acid, and
2·013 ... water.

III. { 4·883 grains of opianic acid gave
10·200 ... carbonic acid, and
2·190 ... water.

	Experiment.			Calculation.		
	I.	II.	III.			
Carbon,	56·99	57·12	56·96	57·14	C_{20}	120
Hydrogen,	5·07	4·93	4·98	4·76	H_{10}	10
Oxygen,	37·94	37·95	38·06	38·10	O_{10}	80
	100·00	100·00	100·00	100·00		210

The opianic acid of the last of these analyses was prepared by the decomposition of teropiammon by potash.

Wöhler's formula, $C_{20}\ H_9\ O_{10}$, requires 4·30 per cent. of hydrogen, which is much too low for the experimental result.

Opianic Ether.—According to Wöhler, opianic ether cannot be obtained by the action of sulphuric or hydrochloric acids upon a mixture of opianic acid and alcohol. I have found the reverse of this to be the case, and obtained it by chance on one occasion when hydrochloric acid had been added to the alcoholic solution of the opianate of potash, which had been separated from the excess of carbonate in the preparation of the acid itself. It is obtained in the form of colourless needles which are insoluble in water, but dissolve readily in alcohol and ether. It melts under water, and also dry, at the temperature of 198° Fahr. Its analysis gave—

5·615 grains of opianic ether gave
12·325 ... carbonic acid, and
2·982 ... water.

	Experiment.	Calculation.		
Carbon, . .	59·86	60·50	C_{24}	144
Hydrogen, . .	5·90	5·88	H_{14}	14
Oxygen, . .	34·24	33·62	O_{10}	80
	100·00	100·00		238

Hemipinic Acid.—By further evaporation of the solution which has deposited opianic acid, hemipinic acid is obtained, and it may be purified by several crystallisations. It is, however, in this case frequently more or less yellow-coloured, but may be readily obtained colourless, and is also effectually separated from any traces of opianic acid which may chance to adhere to it, by precipitating the solution with acetate of lead, and decomposing the washed hemipinate of lead by a current of sulphuretted hydrogen. The characters of the acid corresponded in all respects with Wöhler's description, and the analysis gave the same results as his.

5·445 grains of hemipinic acid gave
10·603 ... carbonic acid, and
2·275 ... water.

	Experiment.	Calculation.		
Carbon, . .	53·17	53·14	C_{20}	120
Hydrogen, . .	4·64	4·42	H_{10}	10
Oxygen, . .	42·19	42·44	O_{12}	96
	100·00	100·00		226

It will be observed that the formula above given, $C_{20}\ H_{10}\ O_{12}$, is exactly double of that attributed to hemipinic acid by Wöhler. The examination of some of the salts of hemipinic acid leave no doubt that it is a bibasic acid, and that its constitution is correctly expressed by the higher formula, which its relations to opianyl and opianic acid would also lead us to consider as extremely probable.

Acid Hemipinate of Potass.—This salt was met with at first accidentally in an experiment in which, during the preparation of the acid, a sufficient quantity of hydrochloric acid was not added to the original alkaline solution; but it may be readily prepared by dividing a quantity of hemipinic acid into two equal quantities, neutralising one half with potash, then adding the other and evaporating. It is deposited in thick six-sided tables sometimes of considerable size. It is readily soluble both in cold and hot water and in alcohol, but not in ether, which throws it down from its alcoholic solution in shining plates. It is highly acid to test paper. Dried at 212° it gave the following results:

6·203 grains acid hemipinate of potass gave
10·245 ... carbonic acid, and
1·935 ... water.

5·330 grains acid hemipinate of potass gave
1·763 ... sulphate of potass.

	Experiment.	Calculation.		
Carbon, . .	45·04	45·42	C_{20}	120
Hydrogen, . .	3·46	3·40	H_9	9
Oxygen, . .	...	33·32	O_{11}	88
Potass, . .	17·88	17·86	KO	47·2
		100·00		

13·505 grains of the crystallised salt lost, at 212°, 1·950 grains = 14·43 per cent., and corresponding to 5 equivalents of water, the calculated result for which is 14·55 per cent. The formula of the crystallised salt is consequently,

$$KO\ HO\ C_{20}\ H_8\ O_{10} + 5\,HO.$$

Neutral Hemipinate of Potass is a highly soluble salt, and crystallises only with difficulty.

Neutral Hemipinate of Silver is obtained as a white precipitate, insoluble in water. Dried at 212° and burned, it gave the following results:—

6·278 grains hemipinate of silver gave
3·098 ... silver.

	Experiment.	Calculation.		
Carbon, . .	...	27·27	C_{20}	120
Hydrogen, . .	...	1·81	H_8	8
Oxygen, . .	...	21·83	O_{12}	96
Silver, . .	49·34	49·09	Ag_2	216
		100·00		440

Acid Hemipinic Ether—Hemipinovinic Acid.—When hemipinic acid is dissolved in absolute alcohol, and a current of hydrochloric acid gas passed through the solution, hemipinovinic acid is formed. It is obtained in the pure state by

evaporating to dryness in the water-bath and crystallising the residue from alcohol or water. It is thus obtained in the form of tufts of extremely light and bulky crystals, sparingly soluble in cold water, but more so in boiling. It fuses when dry at the temperature of 270° Fahr., but melts easily under boiling water into a transparent fluid. It is strongly acid to test paper. Its aqueous solution does not precipitate the salts of lead or silver, but gives with perchloride of iron a bulky, pale brownish-yellow precipitate. It dissolves readily in potash, and the solution, on boiling, evolves alcohol.

5·445 grains hemipinovinic acid, dried at 212°, gave
10·603 ... carbonic acid, and
2·275 ... water.

	Experiment.	Calculation.		
Carbon, . .	56·45	56·69	C_{24}	144
Hydrogen, . .	5·67	5·51	H_{14}	14
Oxygen, . .	37·88	37·80	O_{12}	96
	100·00	100·00		254

This corresponds exactly with the formula

$$C_4\,H_5\,O\,HO\,C_{20}\,H_8\,O_{10};$$

but the crystallised substance contains, in addition, three equivalents of water of crystallisation. 5·74 grains lost 0·570 grains at 212°, corresponding to 9·93 per cent., and the calculation for three equivalents gives 9·60. Crystallised hemipinovinic acid has, therefore, the formula

$$C_4\,H_5\,O\,HO\,C_{20}\,H_8\,O_{10} + 3\,HO.$$

Although hemipinovinic acid possesses distinctly acid properties, and is capable of combining with bases, I have failed in obtaining its compounds in a state of purity. Its baryta salt was obtained in tufts of minute needles, by digesting the solution of the acid with carbonate of baryta, or baryta water, evaporating, dissolving in water and recrystallising. But the compound could not be obtained in a state fitted for analysis, and appears to be very liable to undergo decomposition.

The existence of an acid potass salt, and an acid ether, appear to me to establish, in the most conclusive manner, the bibasic character of hemipinic acid, and to connect it most closely with opianic acid. It is, however, very remarkable, that by the simple addition of two equivalents of oxygen, the latter acid, which is unequivocally monobasic, should acquire bibasic properties. The compounds and products of decomposition of both acids are deserving of further study, but want of time has prevented my pursuing this part of the investigation further.

XI. *Action of Nitric Acid on Cotarnine.*

I have already mentioned that narcotine, when treated with strong nitric acid, undergoes a very powerful oxidation, in which products of further changes are

obtained; and as these changes are obviously not confined to the non-nitrogenous component of narcotine, but extend to the cotarnine also, I proceeded to the examination of the action of nitric acid upon that substance in a pure, or at least in a nearly pure, state.

The products of the action of nitric acid on cotarnine are extremely complex, and several different actions appear to occur simultaneously, in each of which a different decomposition is produced. When concentrated nitric acid is employed, an extremely violent and tumultuous action takes place on boiling, and the fluid contains a large quantity of oxalic acid. When, however, the acid is more dilute, the action goes on more steadily, red fumes are abundantly developed and an acid substance is produced, which remains in solution in the nitric acid. The preparation of this substance is a matter of considerable nicety, and it is particularly important that the nitric acid be not employed in too large an excess, partly on account of the risk of carrying the action too far, and partly on account of the difficulty of separating the product from a very large excess of acid. As the new product is liable to undergo a further oxidation with production of oxalic acid, it is not safe to attempt its separation by evaporating the nitric acid solution. The best method is to dissolve the cotarnine in nitric acid diluted with about twice its bulk of water, and then adding strong nitric acid to raise the mixture to the boiling-point. Red fumes are copiously evolved, and after some time a small quantity of the fluid is taken out and mixed with a considerable quantity of alcohol and ether. If on standing for a short time crystals are deposited, the whole fluid is treated in the same manner; but, if they do not appear, the digestion is continued somewhat longer, and it is tried again, and so on, until the right point is hit. The fluid, mixed with alcohol and ether, is allowed to stand for twenty-four hours, and the precipitated crystals are separated by filtration. This substance agrees in all respects with the apophyllic acid obtained by Wöhler as a product of the action of bichloride of platinum upon cotarnine, and which he obtained in too small a quantity for examination and analysis.

Apophyllic Acid.—The crude crystals of the acid deposited from the alcohol and ether are purified by solution in boiling water, and, if necessary, by digestion with animal charcoal and crystallisation. It then presents all the characters attributed to it by Wöhler, and is obtained with ease, either in hydrated or anhydrous crystals, as described by him. It dissolves in water, but not in alcohol and ether. Concentrated sulphuric acid dissolves it readily, and the solution remains colourless even when pretty strongly heated. Strong nitric acid oxidises it. When heated on the platinum knife it fuses, and, on cooling, solidifies into a crystalline mass. Its fusing point is 401°. It dissolves in large quantity in potass and soda, also in ammonia, and the latter solution, on evaporation, gives very soluble crystals of an ammonia salt. Its salts are all very soluble, and are not easily obtained in a satisfactory state for analysis.

I. { 5·690 grains of apophyllic acid, dried at 212°, gave
10·995 ... carbonic acid, and
1·990 ... water.

II. { 5·185 grains of apophyllic acid gave
10·055 ... carbonic acid, and
1·915 ... water.

{ 3·983 grains of apophyllic acid gave
4·675 ... platinochloride of ammonium.

	Experiment.		Calculation.		
	I.	II.			
Carbon,	52·70	52·88	53·04	C_{16}	96
Hydrogen,	3·88	4·12	3·86	H_7	7
Nitrogen,	7·37	...	7·73	N	14
Oxygen,	36·05	...	35·37	O_8	64
	100·00		100·00		181

The formula of the acid is, therefore, $C_{16}\,H_7\,NO_8$, and this has been confirmed by the analysis of its silver salt, to be given below. Its derivation from cotarnine cannot be distinctly understood, and is attended by the formation of several other substances which I have not examined. If we compare its formula with that of cotarnine, however, we shall see that if we add to the latter two equivalents of oxygen, and subtract from it the formula of apophyllic acid, the difference is $C_{10}\,H_6$.

Cotarnine + 2 eq. O,	$C_{26}\,H_{13}\,NO_8$
Apophyllic acid,	$C_{16}\,H_7\,NO_8$
	$C_{10}\,H_6$

I have, however, been unable to determine whether this group of atoms passes into any particular form of combination, or whether it be entirely oxidised into carbonic acid and water, which I suspect it is.

Apophyllic acid differs from anthranilic acid by the elements of two equivalents of carbonic acid.

Apophyllic acid,	$C_{16}\,H_7\,NO_8$
2 eq. carbonic acid,	$C_2\,O_4$
Anthranitic acid,	$C_{14}\,H_7\,NO_4$

According to WÖHLER, when apophyllic acid is distilled, it gives a quantity of *chinoline*. From our knowledge of its constitution, however, we should rather anticipate the production of aniline, or, at least, of an isomeric of it, particularly if distilled with excess of lime or baryta, thus :—

$$C_{16}\,H_7\,NO_8 - 4\,CO_2 = C_{12}\,H_7\,N.$$

By distillation I obtained a small quantity of an oily base with a somewhat aromatic odour ; but it did not give the reaction of aniline with chloride of lime,

and I did not obtain it in sufficiently large quantity for analysis. Chinoline, I conceive, it cannot possibly be, but whether it is aniline, or a base isomeric with it, I shall for the present leave an open question. My impression certainly is, that it is not aniline, and it is quite conceivable that the decomposition may be very different, and that during distillation only two equivalents of carbonic acid are separated, as is the case in every other instance if the production of a volatile base; and in that case we should have a substance isomeric with anthranilic acid, and possessing basic properties.

Apophyllate of Silver.—This salt can only be obtained by digesting apophyllic acid in solution upon moist carbonate of silver, filtering from the excess of carbonate, and precipitating the solution with a mixture of alcohol and ether. The salt is thrown down as a perfectly white powder, of a more or less crystalline appearance, and which colours slightly by exposure to the light. It is extremely soluble in water, sparingly soluble in alcohol, and insoluble in ether. It does not explode when heated, and undergoes a slow decomposition, leaving behind metallic silver. As precipitated from the original solution it is liable to retain excess of oxide of silver, and requires to be purified by a second solution in water and reprecipitation with alcohol and ether.

{ 6·685 grains of apophyllate of silver gave
  8·005 ... carbonic acid, and
  1·385 ... water.

{ 5·802 grains of apophyllate of silver gave
  2·882 ... silver.

{ 6·425 grains of apophyllate of silver gave
  2·395 ... silver.

	Experiment.		Calculation.		
	I.	II.			
Carbon, . .	32·65	...	33·22	C_{16}	96
Hydrogen, .	2·30	...	2·08	H_6	6
Nitrogen, .	...	...	4·85	N	14
Oxygen, .	...	...	22·33	O_8	64
Silver, . .	37·39	37·27	37·52	Ag	108·1
			100·00		288·1

The formula is therefore $AgO\ C_{16}\ H_6\ NO_7$.

Apophyllate and Nitrate of Silver.—When a solution of nitrate of silver is added to one of an alkaline apophyllate, a rather sparingly soluble crystalline salt is deposited, which has been described by WÖHLER as the apophyllate of silver. It is, however, a double compound of that salt with nitrate of silver. It explodes violently when heated, and the silver must be determined as chloride.

{ 6·238 grains of the double salt gave
  4·785 ... carbonic acid, and
  1·800 ... water.

{ 4·522 grains of the double salt gave
{ 2·984 ... silver.

	Experiment.	Calculation.		
Carbon, . .	20·92	20·95	C_{16}	96
Hydrogen, . .	3·20	1·30	H_6	6
Nitrogen, . .	...	6·11	N_2	28
Oxygen, . .	...	24·44	O_{14}	122
Silver, . .	49·70	47·20	Ag_2	216·2
		100·00		458·2

This analysis, though far from correct, gives a sufficient approximation to theory to shew that the substance actually is a double compound, and the presence of nitric acid in it can be easily demonstrated.

Apophyllate of Ammonia.—When apophyllic acid is digested with ammonia, and the solution evaporated, this salt is left in small prismatic crystals. Of this, a nitrogen determination gave a number too low for the formula of a neutral apophyllate, but approximating to it.

Apophyllate of Baryta is obtained by digesting the acid with carbonate of baryta. It is highly soluble in water, and is precipitated by strong alcohol in wart-like crystals.

Associated with apophyllic acid, another substance was obtained on one occasion in small quantity. It occurs in the form of yellow needle-shaped crystals, which have an acid reaction and are readily soluble in water. They fuse on the application of heat into a yellow fluid, which solidifies on cooling into a crystalline mass. Its analysis gave these results :—

{ 4·857 grains, dried at 212°, gave
{ 10·907 ... carbonic acid, and
{ 1·723 ... water.

{ 4·755 grains, dried at 212°, gave
{ 3·155 ... platinochloride of ammonium.

	Experiment.	Calculation.		
Carbon, . .	61·24	60·85	C_{36}	216
Hydrogen, . .	3·94	3·66	H_{13}	13
Nitrogen, . .	4·16	3·94	N	14
Oxygen, . .	30·66	31·55	O_{14}	112
	100·00	100·00		355

The formula approximating most nearly to these numbers is $C_{36}\,H_{13}\,NO_{14}$; but I have been unable, from want of material, to confirm them by additional analyses or determinations of atomic weight.

On another occasion, a substance was obtained which presented no marked differences from the last, but which contained 55·80 of carbon and 3·94 of hydrogen. There was not a sufficient quantity for a nitrogen determination. These matters will require a further investigation, and I only refer to them here for the

purpose of indicating the great complexity of the decomposition which nitric acid produces.

When the solution containing alcohol and ether, from which the apophyllic acid has been precipitated, is distilled, a syrupy residue of a more or less dark-brown colour is left, which, on the addition of caustic potass, immediately evolves the odour of a volatile base. In order to obtain this substance a considerable excess of potass was employed and the liquid distilled. A highly alkaline fluid passed into the receiver which gave abundant fumes with hydrochloric acid, and rapidly restored the blue of reddened litmus. In this fluid, ammonia and one or two other bases are always present. For the separation of the former the fluid is supersaturated with hydrochloric acid evaporated to dryness, and extracted with absolute alcohol at the boiling temperature. The filtered solution deposits on cooling traces of sal ammoniac, which are dissolved even in absolute alcohol. The alcohol, on distillation, leaves a salt in fine scales, which gives, with bichloride of platinum, a yellow precipitate soluble in boiling water, and deposited, on cooling, in fine golden-yellow scales, and sometimes needles.

{ 9·375 grains of platinum salt, dried at 212°, gave
1·492 ... carbonic acid, and
2·098 ... water.

{ 9·275 grains of platinum salt gave
3·878 ... platinum.

	Experiment.	Calculation.		
Carbon, . .	4·34	5·05	C_2	12
Hydrogen, . .	2·48	2·52	H_6	6
Nitrogen, . .	...	5·93	N	14
Chlorine, . .	...	44·89	Cl_3	106·5
Platinum, . .	41·81	41·61	Pt	98·7
		100·00		237·2

These results correspond with the formula $C_2 H_5 N HCl Pt Cl_2$, and indicate the presence of methylamine.

In another experiment a platinum salt was obtained which gave,—

{ 9·075 grains of platinum salt gave
2·615 ... carbonic acid, and
2·098 ... water.

{ 8·475 grains of platinum salt gave
3·360 ... platinum.

	Experiment.	Calculation.		
Carbon, . .	7·84	9·55	C_4	24
Hydrogen, . .	2·84	2·78	H_8	8
Nitrogen, . .	...	5·99	N	14
Chlorine, . .	...	42·39	Cl_3	106·5
Platinum, . .	39·64	39·29	Pt	98·7
		100·00		251·2

I have placed these results in juxtaposition with the calculated numbers of ethylamine for the purpose of shewing that it consisted of a mixture of the platinum salts of that base and of methylamine. The presence of the latter base was also determined, in this case, by dissolving the salt in water and twice crystallising, when the small quantity obtained gave 40·32 per cent. of platinum, indicating that in this way a separation of the two might have been effected if the quantity of material had been sufficiently large to admit of additional crystallisations.

Some observations lead me to suppose that these are not the only bases formed, but that others with much higher atomic weights are occasionally produced. A serious difficulty, however, in the way of such investigations, is found in the decompositions which all these bases undergo in contact with nitrous acid, to which they are exposed during the action of nitric acid upon the original substance. I propose, however, to pursue the subject further, and hope to find a means of avoiding this difficulty.

I shall conclude this paper with a tabular statement of the substances examined in it, and their formulæ.

Narceine,	$C_{46}\,H_{29}\,NO_{18}$
Hydrochlorate of narceine,	$C_{46}\,H_{29}\,NO_{18}\,HCl$
Platinochloride of narceine,	$C_{46}\,H_{29}\,NO_{18}\,HCl\,Pt\,Cl_2$
Robiquet's narceine,	$C_{32}\,H_{19}\,NO_{10}$ (?)
Thebaine,	$C_{38}\,H_{21}\,NO_6$
Hydrochloride of thebaine,	$C_{38}\,H_{21}\,NO_6\,HCl + 2\,HO$
Platinochloride of thebaine,	$C_{38}\,H_{21}NO_6\,HCl\,Pt\,Cl_2 + 2\,HO$
Teropiammon,	$C_{60}\,H_{29}\,NO_{26}$
Opianyl,	$C_{20}\,H_{10}\,O_8$
Hydrate of Opianyl,	$C_{20}\,H_{10}\,O_8 + HO$
Opianic acid,	$C_{20}\,H_{10}\,O_{10}$
Opianic ether,	$C_4\,H_5\,O\,C_{20}\,H_9\,O_9$
Hemipinic acid,	$C_{20}\,H_{10}\,O_{12}$
Acid hemipinate of potass,	$KO\,HO\,C_{20}\,H_8\,O_{10}$
Hemipinate of silver,	$2\,Ag\,O\,C_{20}\,H_8\,O_{10}$
Hemipinovinic acid,	$HO\,C_4\,H_5\,O\,C_{20}\,H_8\,O_{10}$
Apophyllic acid,	$C_{16}\,H_7\,NO_8$
Apophyllate of silver,	$AgO\,C_{16}\,H_6\,NO_7$
Methylamine,	$C_2\,H_5\,N$
Ethylamine,	$C_4\,H_7\,N$

XXIII.—*On a Necessary Correction to the Observed Height of the Barometer depending upon the Force of the Wind.* By Captain Henry James, R.E., F.R.S., M.R.I.A., F.G.S., &c.

(Read 15th March 1852.)

The oscillations of the barometer during gales of wind must have been noticed soon after the invention of the instrument by Torricelli 200 years ago. Every observer is familiar with the fact, that the barometric column is continually rising and falling during gales; and we frequently meet such observations as "Barometer very unsteady," in Meteorological Registers.

In Sir William Reid's work on the Law of Storms, he says, "during the hardest part of the gale (the Bermuda hurricane of 1839) several persons observed remarkable oscillations of the mercury in the tubes of the barometers;" and in a letter which I had the honour to receive from the Astronomer-Royal, Professor Airy, in reply to one from myself on this subject, he says, "I think (but am not certain) that the depression of the barometer at every gust of a gale of wind is an ordinary phenomenon, without reference to the position of the barometer with regard to the direction of the wind. Many years ago I was in the observatory of Marseilles during the blowing of the Mistral (a wind well known there), and there I saw the drop of the barometer at every gust in great perfection. I do not remember the position of the barometer."

I am not aware that the cause of this unsteadiness of the barometer has been hitherto investigated by any one, or that the amount of the depressions has been shewn to depend upon the force of the wind.

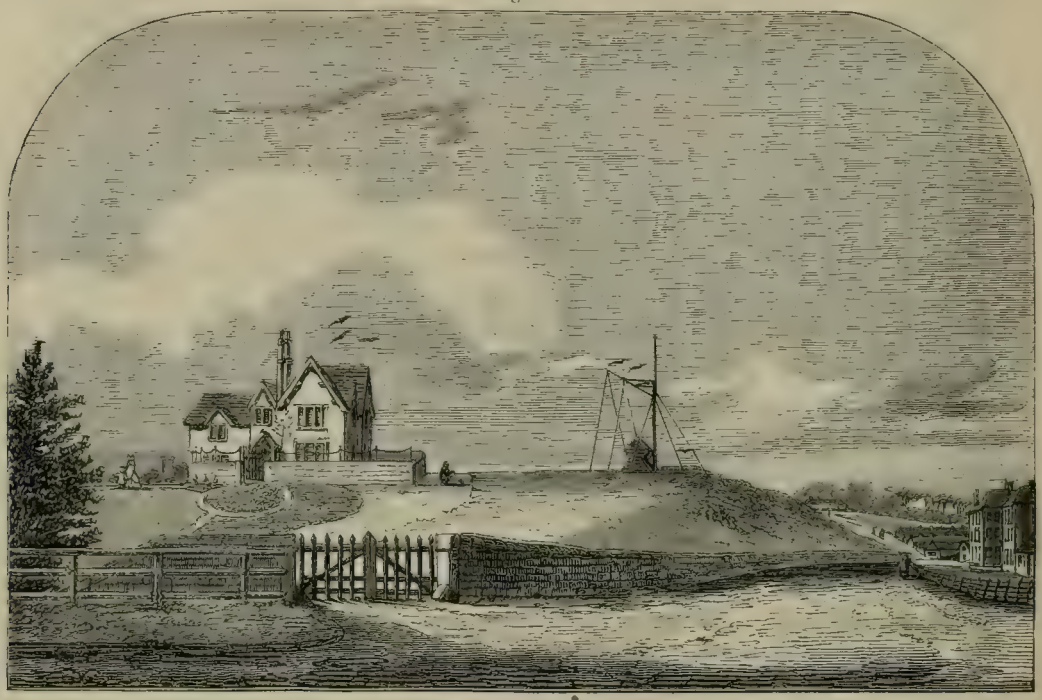
My attention was particularly drawn to this subject last December, by observing that during the heavy gale of wind which we had on the 7th and 8th of that month, the barometer was always depressed at each gust of wind, and that, as far as I could judge by the ear, by listening to the rush of the wind round my cottage at Granton, the amount of the depression was in some proportion to the force or velocity of the wind.

My cottage, which stands alone on a height overlooking the Forth, is peculiarly well situated for investigating this question; and the succession of gales from the south-west which we had during the months of January and February, afforded me the opportunity for following up this inquiry, to confirm my previous impressions, and to give approximately the depression of the barometer corresponding to the different amounts of the pressure of the wind.

The barometer used in these experiments was an aneroid, which from its being so portable, and requiring no other adjustment than to be laid horizontal, was

best suited for the purpose.* The wind-gauge was of a very simple construction, and on the same principle as the instrument used for weighing letters, the weight or pressure being indicated by the compression of a spiral spring in a tube.

Fig. 1.



Lufra Cottage, Grahton.

The wood-cut, fig. 1, represents my cottage (⌐¬~) and an open summer-house (~~) near it. The table in my room in the cottage, the seat of the summer-house, and the surface of the ground (~) close to the summer-house, are all on the same level; I could thus very readily compare the indications of the barometer in these three different situations—that is, as sheltered by the cottage, as sheltered by the back only of the open summer-house, and as laid on the ground without any shelter whatever.

During calm weather I found that the indications of the barometer were identically the same in all three positions; but that when the wind blew with any considerable force, the barometer in the two sheltered positions, that is, in the cottage and in the summer-house, were depressed as compared with the indications of the instrument on the open ground, and that in the two sheltered positions the depressions were in proportion to the force of the wind; and further,

* The correction for temperature to my aneroid between 56° and 92°, is ·0025 for every degree of increase or decrease of temperature, but the barometer is more immediately affected by a change of temperature than the enclosed thermometer.

that every gust of wind was indicated by a corresponding depression of the barometer, whilst the barometer on the open ground remained stationary during all the changes in the amount of pressures of the wind, whether arising from the increased force of the gale, or from the intermittent gusts.

It was, therefore, obvious that the cause of the depressions of the barometer was owing solely to the screened position of the instrument in the cottage and in the summer-house; and that all barometers in detached houses, or observatories in exposed situations, must be similarly affected; and that a ship's barometer, which is always hung in the cabin, and therefore also in a screened position, must be affected in a like manner.

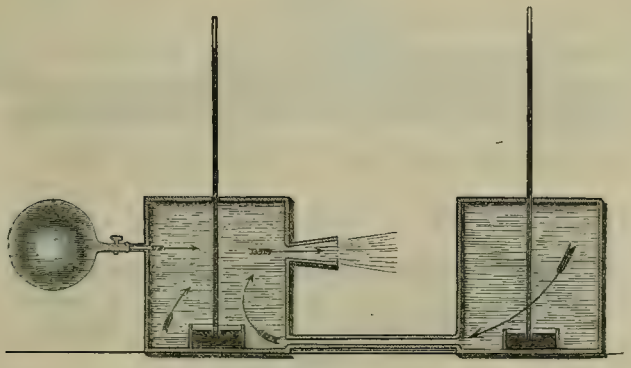
The cause of this phenomenon may be explained by the pneumatic experiments made by HAWKESBEE and LESLIE, and by the hydrodynamic experiments of BERNOUILLI and VENTURI, though the former were made to illustrate a different subject from that which is now under investigation.*

" Dr HALLEY sought to account for the depression of the barometer before a storm, to the withdrawing of the vertical pressure of the atmosphere, when borne swiftly along the surface of the globe by a horizontal motion."—*Encyc. Brit.*

The experiment of HAWKESBEE was made with the view of illustrating and supporting the above hypothesis, whilst the experiment of LESLIE was made with the view of refuting it; but they each serve admirably to explain the cause of the depressions of the barometer in a screened position during a gale of wind.

In HAWKESBEE's experiment, two barometers are enclosed in boxes which are

Fig. 2.



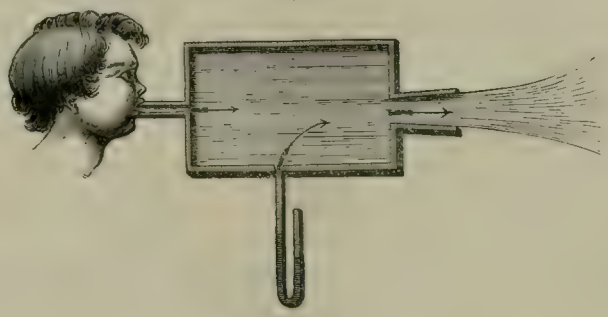
connected by a pipe, as shewn in the wood-cut. A globe of compressed air is screwed to a tube leading horizontally into the upper part of one of the boxes, whilst a larger tube is placed opposite to it, for the escape of the air. When the

* I am indebted to my friends Professor KELLAND and Professor PIAZZI SMYTH, for drawing my attention to these experiments.

cock which confines the compressed air is turned, the air rushes out by the larger tube, drawing with it part of the air which was in the two boxes, and causing a partial vacuum—as is demonstrated by the fall of the barometers in each box.

Sir JOHN LESLIE's experiment is of a more simple nature, and will be understood at once by reference to the wood-cut.

Fig. 3.

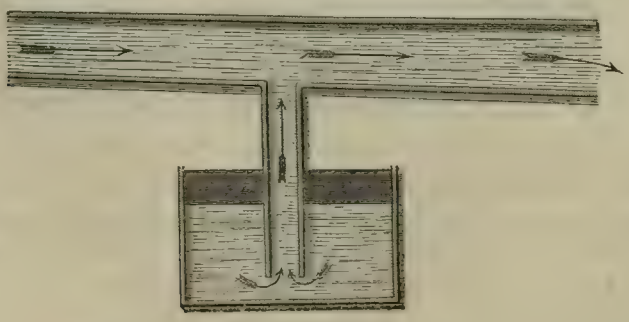


By blowing through a cylinder, to the lower sides of which a glass syphon, partially filled with water, is attached, he found that if the eduction-pipe was larger than the one through which he blew, that a partial vacuum was formed in the cylinder, as indicated by the rise of the water in the leg of the syphon attached to it; but by reversing the instrument and blowing through the larger tube, he found that the air was compressed in the cylinder, and caused a depression in that leg of the syphon.

In the experiments of BERNOUILLI and VENTURI, the rush of a stream of water through a horizontal tube made wide at the end by which the water escapes, is shewn to have the effect of drawing up water through a smaller pipe leading into it.

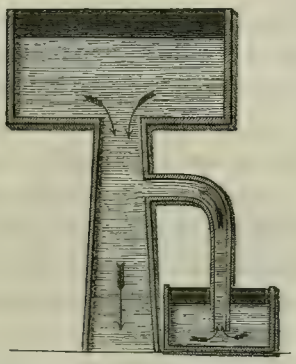
In fig. 4. it is shewn that the water is drawn up and carried away by the rush

Fig. 4.



of water through the larger tube; and, in No. 5, that the water descending vertically in the large tube, draws up the water vertically through the smaller.

Fig. 5.



Whilst these experiments illustrate each other, they also serve to illustrate the cause of the depression in the barometer during gales of wind; for the rush of a stream of wind over and around any house or ship, as shewn in figs. 6 and 7,

Fig. 6.

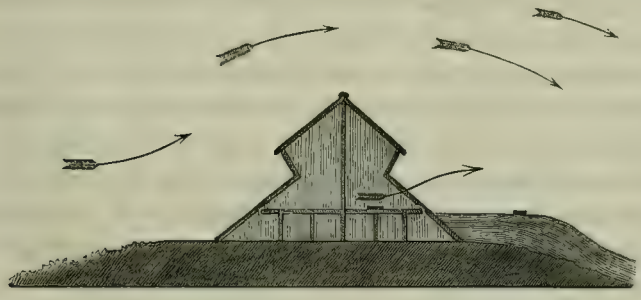
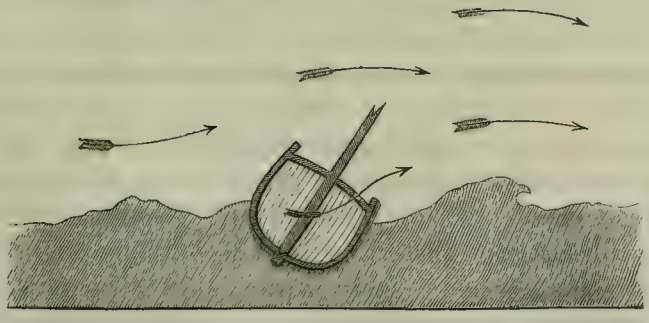


Fig. 7.



must have the same effect of drawing out the air and producing a partial vacuum

in them, as in the above experiments; and this view is further confirmed by the fact, that if a window or door exposed to the wind is opened in any room in which there is a barometer, the mercury is raised, shewing that the air is compressed in the room, as it is in LESLIE's cylinder, when we blow through the larger tube. So also the barometer is elevated by the compression of the air on the windward side of the summer-house, whilst it is depressed on the leeward in proportion to the force of the wind and the intermittent gusts; but the effect in a room, the doors and windows of which are usually closed on the windward side, is to produce a depression. We may also infer, but I know of no experiments to support the opinion, that during gales of wind the barometer would stand at a higher level on the windward side of a hill than on the leeward, the points of observation being at the same altitude. The known discrepancies between the heights deduced from the indications of the barometer during high winds and calms, are, however, most probably due to this cause.

Professor DANIELL, indeed, suggests this very question, "Whether local currents of air, and those deflections of the wind which are caused by the different directions of different valleys, may not produce various partial adjustments of density which may have an influence upon barometrical measurements;" and in the experiments which he made for determining the altitude of Hedley Heath, by observation at different stages of the height, he found an error of 7·5 feet in 157 in the height of the station in a ravine; he says, "omitting the second result (the one in the ravine) all the rest are correct, and the third is deficient exactly the quantity which is in excess in the second;" it is, therefore, obvious that the configuration of the ground was the cause of this anomaly. It is much to be regretted that Professor DANIELL had not followed up the inquiry; but he concludes his remarks by putting the following question:—"What is the effect of wind upon barometrical mensurations? If I had the means of prosecuting these inquiries in the complete manner which the nicety of the subject requires, I would not have suffered them to retain the form of crude speculation."

By a repeated series of comparisons at Granton, I obtained the following results; but I wish them to be considered as merely approximate results, to which I desire to draw the attention of meteorologists, that those who are stationed in countries subject to violent storms and hurricanes, may supply us with the amount of the depressions, corresponding to higher velocities of the wind than I have been able to supply, and that thus the law connecting the amount of depression with the velocity or pressures at different stations may be established. The effect of the wind when blowing from different quarters will also have to be studied, that the amount of the corrections necessary to be applied to the observed height of the barometer at any particular station, when the wind blows from any quarter, may be known.

Velocity in Miles per Hour.	Pressure in Pounds per Square Foot.	Depression in Inches.	
14·2	1	·010	
20·0	2	·015	
24·5	3	·020	
28·3	4	·025	
31·6	5	·030	Observed.
34·6	6	·035	
37·4	7	·040	
40·0	8	·045	
42·4	9	·050	
44·7	10	·055	
61·6	19	·100	

In this table, I have distinguished the observed depressions corresponding to the observed pressures, and have extended the table, shewing that the depression would be one-tenth of an inch for a pressure of 19 lb. per square foot, supposing the depressions follow the same law; but I do not assert that they do, and rather think it probable that they do not, with the higher velocities; but this is a matter for further investigation.

It is not, therefore, correct to speak of these depressions as oscillations in the level of the mercury, above and below a sort of mean tidal line; they are simply caused by diminished pressure, and the barometer resumes its original position without passing it, when the gale or gust of wind passes away. With a pressure of 2 lb. per square foot during the lulls in a gale, and, with gusts, giving a pressure of from 6 to 7 lb., we find the barometer is ·015 always below what it should be, and the effect of the gust is indicated by a further depression of ·035 and ·040; so that by the indications of barometer alone, we are able approximately to estimate the additional force of the gust without reference to the anemometer, and by comparing the readings of the barometer in an exposed and in a sheltered position, we may estimate the force of the wind at the time of observation. In applying the correction for the force of the wind, the depression due to the force during the lulls, should be added to the reading of the barometer when at its highest point.

Now the corrections hitherto considered absolutely necessary to the readings of the barometer, with the view of having the results in a form strictly comparable from different places of observation, are, for barometers with cisterns, without zero-points,

1*st*, For the capillary action of the tube.

2*d*, For temperature to reduce the readings to what they would be at 32°.

3*d*, For capacity, depending on the relation of the area of the surface of the mercury in the cistern to the area of that in the tube.

And, 4*th*, For altitude above the level of the sea.

Now, if we assume the observed height of the barometer to be 29·500 inches at the temperature of 40°, the size of the tube to be ·35 inches, and its capacity $\frac{1}{35}$th of the cistern, and the neutral point of the instrument at 29·750, the height

above the mean level of the sea 25 feet, the wind blowing with a force of 6 lb. per square foot, the value of the corrections will be,

Capillarity, ...	+·021
Temperature, ...	−·023
Capacity, ...	−·007
For altitude, ...	+·034
Force of wind, ...	+·035

It will thus be seen that the correction for the force of the wind, in the supposed case, would be greater than any of those hitherto considered absolutely necessary for a strict comparison, and, consequently, that it is an element which cannot properly be neglected; but I beg to repeat that I do not give the depressions corresponding to the force of the wind as absolutely determined, even for the short range I have observed; and it is possible that in towns and other sheltered places, the results would be different. My object is to draw the attention of meteorologists to the facts stated, in the hope that by more extended observations we may obtain more accurate data; and it is important that their attention should be drawn to this subject at this time, when I trust we are on the eve of seeing established a uniform system of observation and registry for the world, under the sanction of the several governments, and promulgated by the authority of a congress of the most eminent men in meteorological science, from all parts of the world.

The practical importance of the study of meteorology is daily becoming more evident by the results obtained by Mr Redfield and Colonel Sir W. Reid from the study of the law of storms, by the results obtained by the wind and current-charts of Lieutenant Maury, the astronomer at Washington, and in the true indications, as I believe, which the isothermal lines give of the most accessible route to the open Polar Sea, and the pole itself, as explained by Mr Petermann, not to speak of the accurate data which meteorology gives us for understanding the peculiarities of the climates of the different parts of the world, the causes favourable or otherwise to the health of man, or the necessary conditions for the successful cultivation of the different products of the earth, or the high interest which must ever attach to the purely scientific branch of this inquiry; but we can never arrive at a full understanding of these important subjects without that combined and uniform system which has been proposed.

I cannot terminate these remarks better than by quoting the words of Professor Daniell in "an urgent recommendation to meteorologists to use standard instruments, to observe them with care, and to make all necessary corrections for accidental differences; and, above all, *to keep their tables on the same scheme.* Much curious information is dependent upon such an extensive plan of comparative observation; and, without it, the observer does little more than accumulate an overwhelming mass of crude and incorrect materials."

XXIV. — *Defence of the Doctrine of Vital Affinity.* By William Pulteney Alison, M.D., &c. &c., Professor of the Practice of Medicine in the University of Edinburgh.

(Read 15th March 1852.)

Having expressed a decided opinion that there are, in all living bodies, chemical as well as mechanical phenomena, which, in the present state of our knowledge, ought to be designated as Vital, and referred to the operation of laws, distinct from those that regulate the chemical changes of inanimate matter, and observing that this opinion is controverted, and that the view of the chemical phenomena of life which I have maintained, is rejected as unphilosophical and delusive by two authors of high scientific reputation—Baron Humboldt and Dr Daubeny,—and that the judgment of other authors of acknowledged character on this subject is not clearly expressed, and seems to me to involve it in unnecessary obscurity, I am led to hope that some farther explanations may be of some use in establishing the first principles of a Science which, as it appears to me, has suffered, in several instances, not so much from want of facts, as from hypothetical and erroneous inferences, drawn from facts that are already known.

When I first undertook, above thirty years ago, to deliver lectures on Physiology, I ventured to express an opinion, that "a discovery would be made, connecting the *ingesta* into the animal body with the nourishment of the different textures, and with the nature of the different *excretions*, equally important as illustrating the obscure *chemical phenomena* of the living body, and the intention of the different secretions, as the discovery of the circulation of the blood was, in illustrating the *movements* going on in its interior, and the use of the organs concerned in effecting them." It did not occur to me, nor do I know that any one had then conjectured, that these chemical phenomena, like the movements of the animal fluids, partook of the nature and formed part of *a circulation*, but of one of such extent and complexity, that the atmosphere, the soil, and the vegetable kingdom, furnish the other great links in the circuit, and that all the elements of the ancients, fire, air, earth, and water, are literally and essentially concerned as agents in maintaining it.

It appears, however, from the following passage in one of the earlier writings of Sir Humphry Davy, that he was aware of, and had duly reflected on, the most

material facts on which this important discovery is founded:—"Nature has catenated together organic beings, and made them mutually dependent on each other for their existence, and all dependent on light. A privation of light would be immediately destructive to organic existence; vegetation would cease; the supply of oxygen gas would be quickly cut off from animals; the lower strata of the atmosphere would become composed of carbonic acid; and perception and volition would exist no longer."*

The following description of the circulation, in which all the matter destined by Nature to the maintenance of organised creation on the earth's surface is continually engaged, is merely an amplification of the expressions of DUMAS: and although part of the statements contained in it are liable to objection, its general import is such as amply to fulfil the expectation of a great discovery which I had expressed.

Vegetables, under the influence of light and of a certain temperature, are continually abstracting from the atmosphere, directly or indirectly, a part of its constituents, in the form of water, carbonic acid, a little nitric acid, and ammonia. The radicals of this inorganic matter (*matiere brute*) are gradually organised in vegetables, which are a true reducing apparatus, while a part of its oxygen is set free; and, after being formed into organic principles, those radicals are yielded directly or indirectly to animals. This matter is applied, without farther change, to the maintenance of the functions of animal life; particularly it furnishes the conditions, and becomes the instrument, of mental acts; after which, as if exhausted by the effort which it has made, it falls again under the influence of oxygen, in the animal body, which is a true apparatus of combustion; and either before or after the death of the animal structure, returns as inorganic matter, under the name of manure, to the great reservoir from which it came. In this eternal circuit, life is the chief agent, and by these changes it makes itself known; but the matter that is thus employed undergoes only a change of place. I apprehend we must add, that the *properties* as well as the *position* of this matter are continually *altered* and *resumed*, and that it is the modification which the properties of this matter undergo, in the course of this circulation, which constitutes the precise object of all physical inquiries, both in vegetable and animal physiology, so far as the organic functions of animals are concerned.

The final cause of all these changes is already obvious. We know that it has pleased the Author of our being to connect with a world previously existing, and consisting of matter already long endowed with all its physical properties, an infinite number and variety, and eternal succession, of sensitive creatures, and ultimately a race of beings "formed after his own image." The acts of sensation and thought which characterise these, he has placed in immediate connection

* Works, vol. i., p. 106.

with that peculiar structure, endowed with still more peculiar properties, to which we give the name of a living Nervous System; and he has established laws, in the execution of which the vegetable as well as the animal kingdom bears its part, according to which infinite varieties and endless successions of nervous systems shall be engendered and supported from a limited quantity of the matter originally contained in the atmosphere surrounding our globe,—shall be nourished, lodged, protected, and enabled to satisfy the wants and to obey the will of their immaterial inhabitants; but all this innovation on the laws regulating the matter previously existing on the earth's surface is only transient. The same portions of matter which are thus employed, whether they pass through vegetable structures only, or minister to the support both of vegetables and animals, are restored unchanged to the reservoir whence they came,—in the latter case more rapidly and frequently, and during the life of the structures thus maintained,—and are ready to run the same course again when again placed in presence of living beings. Like the figures of snow into which the imagination of SOUTHEY figured the magician OKBA, breathing the breath of life every morning, that they might people the surrounding wilderness, and charm the solitude of his daughter LEILA, they all receive vitality only for their day, and

> "Ever when night closes,
> They melt away again;"

and such of them as have served as the habitations of mental acts or feelings then "restore the spirit to Him who gave it." The provisions for the temporary maintenance, for the protection and comfort, for the sentient and mental enjoyments, and the eternal reproduction, of this infinite number and variety of sensitive beings, out of a limited quantity of certain chemical elements contained in the earth's atmosphere,—and for the progressive development of the human mind, as the destined lord of this Creation,—are the great Laws of Life, the investigation of which is the object of this science. The power of perceiving their adaptation to their object, and of appreciating the grandeur of the design, is one of the highest privileges of our nature; and without pretending to be qualified to assign the respective merit of the different physiologists, geologists, and chemists, who have illustrated the different parts of this general view of life,—of CUVIER, DALTON, PROUT, LIEBIG, BRONGNIART, PREVOST, DUMAS, and BOUSINGAULT, and their numerous friends and followers,—we may all congratulate ourselves on having lived in the age when so much of the designs of Infinite Wisdom for the regulation of this world has been made manifest to mankind.

But when we inquire a little more minutely into the nature of the changes constituting this great vital circulation, I think it must appear obvious, that the most essential of all are and must be strictly *chemical;* and it seems to me that the grandeur of the design is not clearly perceived, unless we fix atten-

tion on the alteration *of all* the qualities of matter which are here implied, and shew that the Power which has introduced living beings upon earth has had at its command, and has actually modified, *all the laws of nature*. The water, carbonic acid, and ammonia, which form the chief and essential constituents of the ingesta of vegetables, are there thrown into combinations, differing from any which they form, or which can be formed from the elements composing them, in any other circumstances. This is fairly admitted by Dr DAUBENY, who says,—"We are still far from imitating Nature in those processes by which she continues to bring about the wonderful products of organic life, and must admit that, judging from what is yet known, there would seem to be *a power residing in living matter, distinct*, at least in its effects, *from ordinary chemical and physical forces*.*

Now, before going farther, let us observe how essential to everything living, and how peculiar in its effects (from which alone it is known to us), is this power residing in living matter, and distinct from ordinary chemical forces, but which Dr DAUBENY must regard as producing chemical effects, because he himself ascribes to it the formation of "the wonderful products of organic life."

Let us remember, that the very first requisite to the commencement of this great vital circulation, the decomposition of the carbonic acid of the atmosphere, fixing the carbon which is to serve as the basis of all organised structures, and setting free the oxygen, producing therefore a change which is unquestionably both peculiar and chemical, "is done by a power," as stated by LIEBIG, "surpassing that of the strongest galvanic battery, to which the strongest chemical action cannot be compared." Next, let us observe, that the compounds formed in living bodies under the influence of this acknowledged power, of which the first indications are so striking, possess peculiarities (which I formerly noticed) quite sufficient to distinguish them from all compounds formed by chemical affinities, under any other circumstances in nature. They have a uniform complexity of constitution, even in the minutest particles, not seen in inorganic solids; they assume perfectly definite forms, varying, not according to their chemical constitution, but according to their living progenitors, or the particles of living matter with which they come in contact. These forms, as long as they belong to living structures, never become crystalline; although the same elements, after escaping from the immediate contact and influence of living structures, even within the excretory ducts by which they are to be thrown out of the body, fall into compounds which take the crystalline arrangement. Above all, these organic compounds, thus influenced by *place*, are equally liable to an influence of *time*. They are all of transient duration, and particularly in the case of animals, we know that at the same time, at the same points, and in presence of the same agents, in which the

* On the Atomic Theory, p. 370.

matter originally introduced from vegetables is applied to the nutritive assimilation and formation of the living textures, other portions of the same elements, previously used in the same process, are continually yielding to the influence, previously resisted, of the oxygen of the air, and are forming another set of compounds, by the process of destructive assimilation, which are ready to take the form of crystals; which either already possess, or rapidly tend to, the composition of the inorganic matter whence all these compounds originate;—which are poisonous if retained in the body, and for which, therefore, outlets are provided in the organs of excretion, so as to justify the striking expression of Cuvier, that all living animal matter, although the depository of force which will compel other matter to follow the same course as itself, will soon occupy its own place no longer. This is a series of chemical changes quite distinct from anything seen in any other circumstances of Nature. And farther, we know, in regard to the power exciting these, that it is obedient to certain laws of animal life, to which nothing analogous is seen in other chemical operations; nutrition and secretion being liable to sudden and important change, as living movements are, by living changes in nervous matter, *e. g.*, by those which attend certain acts of mind; and farther, every such action being liable to diminution or exhaustion by the degree in which it is itself exercised.

It will be observed, that all these are peculiarities in the *chemical* nature and constitution, even of the minutest particles of all living structures; and that without reference to them nothing living can be characterised. These phenomena are just as distinctly peculiar to living bodies, and characteristic of their living state, as the contraction of muscles, whether produced by irritations of their own fibres or of the nerves entering them; and they are much more general in all classes of living beings. If anything in the economy of living beings demands explanation, or is deserving of being made the object of scientific research, it must be these, their most essential characteristics. If they are not of such importance as to demand special investigation,—if it is only the movements of the particles concerned either in the development of vegetables, or the varied functions of animals, that we ought to regard as peculiar to living bodies,—then Physiology, so far as these properties of living bodies are concerned, has no claim to the title of a separate science, it is only a branch of Chemistry. But it is just as probable, *a priori*, that the laws of chemistry should undergo a modification in living bodies, as that the laws of motion should be made subordinate in certain parts of living animals, to the vital property of Irritability of muscles, as explained by Haller; and the very peculiar changes which are observed in tracing the course of the elements of water, carbonic acid, and ammonia, which are absorbed by vegetables, until they pass out in the form of water, carbonic acid, and ammonia (or in compounds immediately resolvable into them), in the excretions of

animals, and resume their office as manures, afford a manifest presumption that such modification takes place.

The chief consideration which seems to have prevented Dr DAUBENY from acknowledging that the power which he himself supposes to exist in living beings, and to regulate chemical changes there, is deserving of a separate name and a separate inquiry, is thus stated:—"If it is asserted that this power is to be directly ascribed to the vital principle itself, we pause for further information."

Here it seems to me manifest, that there is a misapprehension as to the correct meaning of words, and one which may be traced in many other speculations in the elementary departments of physiology,—investing the term Vital Principle with a meaning much more mysterious and formidable than is needful. According to the only idea which I can form of what is properly termed the vital principle, Dr DAUBENY has already admitted, in the words above quoted from him, that, so far as we can yet see, we *must* regard the vital principle as concerned in forming the "wonderful products of organic life;" because he says, that these result from a power residing in living matter, producing physical effects, yet distinct in its effects from ordinary chemical and physical forces.

The only correct way of defining what we call Vitality, or the vital principle, as I have always maintained, and as I think the best authorities now admit, is this:—First, we describe what we call living beings. They are those, as CUVIER states, which originate by a process of generation, which we can describe,—are maintained by a process of growth and nutrition, which we can describe,—and terminate by death and decomposition, which we can describe. Then, having thus discriminated those bodies which we call living, we say that, in so far as we can satisfy ourselves, that any part of the phenomena which they present are inexplicable by, and inconsistent with, the laws regulating the changes of any other matter, we call them effects of the vital principle, or vitality; and that is our definition of those terms. Those who object to the use of the substantive noun Vitality, or the Vital Principle, as a general expression for such phenomena, constantly use the adjective Vital, or Living, which conveys the very same meaning, and can be defined, as I apprehend, in no other way. The real *efficient* cause of these, as of all other phenomena in nature, is the Divine Will, and is inscrutable; but we know, that in all departments of Nature, this all-powerful cause acts according to laws which we can understand, and the discovery and application of which is the object of all science. When we see that any phenomena in nature take place according to the same law as others more familiar, we are said to explain them, or to assign their physical cause; but until that is clearly ascertained, we obey the dictates of science in declining to arrange them along with those depending on any law otherwise known to us, and endeavouring to apply the method of induction to themselves,—and to any such isolated phenomena as may

seem analogous to them,—so as to ascertain laws peculiar to this set of phenomena.

Dr Daubeny refers also to a passage in the writings of Dr Bostock, in which he speaks of reference to the operation of the vital principle, or to any vital affinities, "as one of those delusive attempts to substitute words for ideas, which have so much tended to retard physiological science;" or, as it may be more simply expressed, as only a reference to an occult cause, or a confession of ignorance on the subject. On this I would observe, that if, by merely using the term Vital Affinity, we were to suppose that we offered a *sufficient explanation* of any phenomena, I would agree with Dr Bostock. But I use the term only as defining the department of human knowledge to which these phenomena are to be referred, and in which the explanation of them (*i. e.*, the law according to which they take place), is to be found; and thus using it, I maintain that there is nothing delusive or unscientific in thus limiting and fixing the object of our inquiries. The investigation of the law or laws by which vital affinities are distinguished from the affinities of inorganic matter, is a subsequent inquiry, in which we may add, that some progress has been made. It is something, for example, to say that vital affinities shew themselves in living beings in two distinct ways; *first*, by the formation of new compounds, found nowhere else in nature; *secondly*, by the selection and attraction of these compounds, at different points, out of a very complex fluid, so as to form organised structures; and to point out the circumstances in which these powers act. It is something to say, with Dr Prout (if that principle is to be held as established), that in the formation of new compounds in living bodies, the elements employed by nature are not subjected to any new affinities, but only hindered from obeying certain of those which actuate them in other circumstances; while others are allowed to act. It is something to say, that the compounds thus formed perish many times during the life of the structure in which they are contained,—the more rapidly as their vital properties have been more energetically exercised; and by perishing furnish the poisonous matter which continually circulates in every living animal, and for the expulsion of which the organs of excretion are provided. It is something to say that Carbon, fixed from the atmosphere by plants, is the substratum of all the organic compounds of which living beings are composed; and that Oxygen, taken in by the lungs or gills of animals, is the great agent in forming the excretions by which they are constantly worn down. And I think we define and limit all these inquiries satisfactorily, when we say, that we seek to ascertain the laws, according to which ordinary chemical affinities are modified in living bodies; or according to which that power acts, which, by Dr Daubeny's own admission, resides in living bodies, and produces chemical effects, "but is distinct from ordinary chemical forces."

Dr Daubeny goes on to say, that Nature has at her command an apparatus

of a more refined and subtle description than any which we can command, and may therefore accomplish effects by purely chemical and physical agency, which may for ever lie beyond the reach of the coarser manipulations of art; and here he refers to HUMBOLDT, who says, that, as we do not understand all the conditions under which ordinary chemical and physical forces act in living beings, we are not entitled to *assert* that they *may not* produce all the chemical changes that we see in them: to what conditions he here alludes does not appear, but he gives this as a reason for renouncing, or, at least, expressing doubts as to the theory of vital affinity, which he had formerly espoused, and illustrated by an allegory under the name of the Rhodian Genius.

Dr DAUBENY says more precisely, that "the peculiar *structure of parts*, arising out of the *Movements* induced by a vital principle, *may be found* competent to bring about these phenomena in question, and that it is incumbent on us to investigate to the full the extent to which such physical causes can be supposed to operate, before pronouncing whether there may not, after all, be some *residual phenomenon*, inexplicable by the common principles of science, and which we must, therefore, refer to vital affinity." But even in regard to movements, from his expressions at p. 379, he does not seem to admit that any others are to be ascribed to the vital principle, than those which result from Contractility.

In thus admitting that the *movements* which take place in living animals, at least those which can be referred to contraction of solids, arise out of the vital principle (which, I apprehend, means only that they are an ultimate fact,—so far as yet known exemplified in no other department of nature); and in ascribing to the peculiarity of those movements the peculiar structure of living parts, and through the intervention of that structure the peculiar chemical changes of living beings, Dr DAUBENY has stated what I believe to be the general idea of those physiologists who reject the doctrine of vital affinity. They think that having allowed that *movements*, and particularly contractions of living solids, are truly vital phenomena, they have furnished a *possible* explanation of all chemical changes which seem peculiar to life, and that they are entitled to throw on us the burden of *disproving this theory*, before they can be called on to admit any such principle as vital affinity modifying chemical laws in the living body.

To this I reply, *first*, that this theory in explanation of the chemical phenomena of life is distinctly *inadequate*. I do not think it can be more distinctly stated, or more plausibly supported, than it was by the late Dr MURRAY, in treating of *Secretion*, who, at the same time, however, distinctly admitted that it was "*hypothesis* supported by little direct proof. The cause of production of the new combinations which constitute secretion," he says, "*may be* the simple approximation of the elements which constitute the blood. That fluid is propelled byt he vis a tergo into canals of the most astonishing minuteness, the diameters of which are

still farther diminished from their alternate contraction from the stimulus of the blood. There can be no doubt that in compounds the force of attraction subsisting among their constituent particles, is modified by the distance at which these are placed; and in compounds especially, which consist of four or more principles, the slightest alteration in their relative situation is sufficient to change entirely their existing attraction, and induce new combinations. The blood is a compound of this kind; its ultimate principles, too, are capable of entering into an innumerable variety of combinations with each other; we may conceive, therefore, that when subjected to the contraction of the extremely minute vessels through which it is forced to circulate, the relative position of its elements will be changed, and new combinations formed. And if we suppose a fluid thus passing through tubes of different diameters, and undergoing successive decompositions, we may easily conceive that very different products may be formed from the same original compound. This affords a very simple view of the nature of Secretion. No complicated apparatus is requisite; all that is necessary being the propulsion of the blood through extremely minute vessels capable of contraction. And it is easy to account for the variations to which secretion is liable, as the contraction of the vessels must vary from variations in the state of their irritability and of the stimuli acting on them." [*Murray's System of Chemistry*, vol. iv., p. 518.] In regard to the Nutrition of solids, Dr MURRAY says merely that they appear to *attract immediately from the blood* the materials which it contains ready formed, as there is probably "no solid in the animal body, of which the immediate principles do not exist in the blood." [*Ibid.*, p. 516.] But I need hardly say that subsequent researches have not only completely demonstrated the insufficiency of this explanation, but have shewn that the cause of the difference of products formed apparently from the same blood must be essentially different from that here assigned; and I would say farther, have shewn that the peculiarity of the compounds formed in living bodies cannot be reasonably ascribed to any modification of those *movements* of fluids, which Dr DAUBENY regards as the only results of the vital principle. To shew this, I need not go into the question of the mode of action of arteries on the blood, or the portion of the changes essential to secretion, which takes place in *cells*, exterior to vessels, and, of course, cannot be ascribed merely to the pressure to which the blood passing along the vessels may have been subjected; which had certainly been misapprehended by MURRAY, as by most other physiologists of that day. It is sufficient to quote a brief statement from CUVIER, which seems to me quite conclusive as to the question, whether difference of secreted fluids in the animal economy can be ascribed to difference in the structure of, and therefore of the movement of the blood through, the organs in which they appear. "The same organ," he says, *i.e.*, the organ secreting the same fluid from the blood, "presents in different classes of animals, sometimes in the same class, perfectly distinct structures. This is true of the

salivary glands, of the testes, even of the liver, of which the organisation is the most uniform, and likewise of the kidneys." "It would be interesting also," he adds, "to compare the secreting organs with their secreted fluids, and observe whether the organs that have a similar structure afford similar products. But experience will sanction no such theory. Nothing, for example, can be more various than the matter furnished by 'crypts' in different animals, from a simple mucus to the most odoriferous compounds." "The simplest secreting organs," he observes elsewhere, "are in insects, where they are merely tubes which float in the general nourishing fluid, which is in contact with their outer surface, while their inner surface contains the secreted fluid. Secretion there can be only a kind of *filtration;* but how different from that which can take place where there is no life, through 'an inorganic solid!'" (*Leçons sur l'Anat. Comp.* Lect. xxx., Art. 1.)

But farther, not only is the complex vascular structure and the varying pressure from contracting solids, which was regarded by MURRAY as the main cause of the formation of new compounds out of the blood, shewn by the examination of other animals, to be quite unnecessary for that purpose; but we now know, that where these conditions exist, that formation is *never* effected—the most compound fluids of the animal economy, which *appear* in the different glands, being really not formed there, but in the course of circulation, and appearing in the blood or in other parts of the body when the organs where they usually appear have been extirpated, or rendered useless by disease; that is, when the cause to which their origin is here ascribed has been absolutely withdrawn.

The mere selection and attraction out of the blood at different places, of different compounds already existing and circulating in it, is certainly the chief, and, according to many, and particularly according to Dr DAUBENY himself, the sole office, performed by any parts *of animals* by which any new organic products are exhibited; and the office of forming those organic compounds, the origin of which is the great chemical change effected by living beings, is performed by no organ capable of exerting a varying power of contraction and pressure, but simply by *the cells of vegetables*, where the fluid introduced from without is usually not conveyed in vessels at all, and is clearly not subjected to any such pressure from contracting solids, as is exerted on the blood in most animals; nor to any such peculiar cause of movement as can be ascribed to the living property of contractility, the only one which Dr DAUBENY admits to be strictly vital.

I have formerly stated, and notwithstanding the opposition of Dr DAUBENY and others, still think, that the judgment of various authors on the respective offices of vegetables and animals as to vital affinities,—the supposition that *no* organic compound can be formed in animals, and that their office is *merely* the selection and appropriation of the compounds formed in vegetables, and afterwards the destructive assimilation by which these are restored, through the excretions, to the inorganic world, is too hasty. It appears from the experiments of

Liebig himself, that the infusory animals decompose the carbonic acid of the air, and exhale oxygen in like manner as vegetables; and the evidence of the formation of oily out of saccharine or amylaceous matters in many animals appears to be unequivocal. The two distinct powers, therefore, of *forming* and of *fixing*, or appropriating the organic compounds, are not so accurately divided between the vegetable and animal world as has been thought. But the more that any physiologist is convinced, as Dr Daubeny is, that the formation of organic compounds is peculiar to vegetables, certainly *the less reason* can he have for supposing that this great change can be due to any mechanical movements, on the principle of contraction and impulse, arising out of the vital principle; the provisions for such movements being so striking a part of the economy of animals, and never having been proved to exist at all in vegetables.

But, *secondly*, In maintaining the scientific correctness of the doctrine of vital affinity, as I have defined it, I think it quite unnecessary to go into these details. I maintain that the objections made to this doctrine, both by Daubeny and Humboldt, are *logically incorrect*, because, in dealing with a set of facts so extraordinary, so important and characteristic as the chemical changes of living beings have been shewn to be, they hold it to be incumbent on us to prove the *negative* proposition, that these *may not* ultimately be referred to those laws which regulate the chemical changes in dead matter, which may be acting under conditions not yet known, and of which they say nothing. The rule of sound logic is,—"*affirmantibus incumbit probatio.*" It is admitted on all hands, that the phenomena of life in general are so peculiar and important as to be properly ranked together as a separate science; and we have shewn that of these phenomena, the most essential and characteristic are certain chemical changes, which are admitted to be so distinct from any that can be observed anywhere else in nature as to "indicate the existence of a power distinct from any simply chemical or physical forces." It is clearly incumbent on those who maintain that, nevertheless, these ordinary chemical forces, acting under conditions not yet understood, may be found adequate to this explanation, to give evidence in the way of observation and experiment of this proposition, otherwise their doctrine is only a hypothesis. If the subject is not thought worthy of scientific inquiry at all, then Physiology is not a separate science. If it is regarded as a separate science, of equal interest and importance as any other, then it is the duty of physiologists, acting on the strict method of induction,—because ascending from facts to principles, instead of descending from principles to facts,—to examine these individual phenomena themselves, arrange and classify them as they present themselves in the different classes of living beings, and consider how far laws deduced from the observation of dead matter can go in the explanation of them; but wherever we find that there is a difficulty in that explanation,—in-

stead of straining the principles of other sciences formerly ascertained, to make them include phenomena admitted to be distinct from any of those to which they have formerly been applied,—it becomes our duty to attempt the investigation and determination of laws peculiar to *this* department of nature. If these laws shall ultimately resolve themselves into any previously known and more general laws of nature, science will be simplified, and a great advance made; but it is assuredly mistaking the right order of inquiry to assert that, *because such simplification may ultimately be effected*, therefore we are now to decline giving these phenomena an appropriate name, and endeavouring to reduce them to general laws by an induction limited to this department of nature itself.

This is the principle which has been successfully followed in other departments of science. Speculations have been hazarded as to the cause of the principle of Gravitation itself. I recollect that the late Mr Playfair used to say a few words in favour of one of these, the theory of Ultra Mundane Particles continually moving in all directions through all space, although not making themselves known to the human senses; which, if admitted, would resolve the principle of gravitation into that of motion communicated by impulse. But no one will maintain that it was incumbent on Newton to prove, that this theory would *not* explain the phenomena, before asserting the principle of gravitation, and determining, by observation and experiment, the laws according to which that principle acts, or by which the phenomena coming under that head are regulated. It is, indeed, observed in many departments of science, that one great difficulty in the early inquiries is, to keep the inquirers from deviating into lines of research which they may think analogous to their own, and applying prematurely principles which have been established by an induction of very different facts. This is the error which Dr Reid made an object of special remark when speaking of the "enumeration of the original powers and laws of our mental constitution." "Success in an inquiry of this kind it is not in human nature to command; but perhaps it is possible, by caution and humility, to avoid error or delusion. The labyrinth may be too intricate to be traced through all its windings; but if we stop when we can trace it no farther, and secure the ground we have gained, there is no harm done; a quicker eye may in time trace it further."—(*Hamilton's edition of Reid*, p. 40.) In physiology itself, it is a similarly just and comprehensive observation of Mr Lawrence, "that although organised bodies are subjected in many respects to physical laws, yet, as regards their own peculiar phenomena, the reference to gravity, to attraction, to chemical affinity, to electricity or galvanism, can only serve to perpetuate false notions in physiology, and to draw us away from the proper point of view in which the nature of living phenomena, and the properties of living beings, ought to be considered."—(*Two Introductory Lectures*, p. 161.) It was the same idea, not, perhaps, so accurately conceived, but more graphically announced, which prompted Dr William Hunter's remark, in com-

mencing the subject of Digestion in his anatomical lectures. "Some tell you that we have here a fermenting vat, and some tell you we have a stewpan, but I tell you *we have a stomach.*" And when we remember how little has been done to elucidate the function of digestion by likening the changes in the stomach either to fermentation or to chemical solution (although both are principles which appear to act to a certain extent), and how much comparative anatomists and physiologists have done, by extending their inquiries into other classes of animals, and studying in all, the changes which commence in the stomach and terminate in the different organs of excretion—to establish laws peculiar to physiology, under which so many forms of structure, and so many vital operations may be arranged,—we can hardly fail to admit that this distinction was wisely drawn. Indeed, the whole science of Morphology, or of the *analogies of the structures* formed by living action—as it is certainly a branch of knowledge strictly *sui generis*—may be said to furnish an illustration of the advantage of keeping the investigation of the laws of living action entirely separate from all other scientific inquiries.

But the authority to which I would wish particularly to refer, as sanctioning and authorising the view of the chemical phenomena of the living body which I here advocate, is that of HALLER, whose great achievement in physiology was simply that of establishing the strictly vital nature, and laying down the most important laws, of the living property of Contractility; the only property concerned in organic life which is expressly admitted by Dr DAUBENY to be truly vital, but to the assertion of which the mechanical physiologists of that age were opposed, on grounds, as it appears to me, exactly analogous to those on which the doctrine of vital affinity is now opposed, because *it had not been proved* how far the mechanical properties of matter were, or were not, adequate to explain the movements of living bodies.

"As all physiology," says HALLER, "involves a history of motions by which the animal machine is agitated, and as all motions have their own laws, we can perceive why, about the end of last century, the principles of hydraulics, hydrostatics, and mechanics, were transferred to physiology. There is a difficulty in this matter, however, and if we reckon up all the good, and all the evil, which has been done to physiology, by the cultivation of these sciences, some may think that we might gladly renounce all the good, for the sake of escaping the evil. There are certainly many things in the animal economy very different from the effects of ordinary mechanical laws; great movements excited by slight causes; the flow of fluids hardly diminished by causes which, according to established mechanical laws, ought to arrest them entirely; motions excited by unperceived causes; vigorous movements produced by the contraction of weak fibres, &c.; from which I do not infer, that simply physical laws are to be repudiated in physiology; but this I maintain, that they are *never to be transferred to the explana-*

tion of phenomena of living bodies, unless their application is confirmed by experiment."—(*Phys. Prin.*, p. 6.)

It might have been perfectly fairly argued at that time, that physiologists did not understand all the conditions, under which the laws of mechanics and of hydraulics (admitted to have a certain influence) act in a living body, and that until it was ascertained that these *would not suffice* for the explanation,—that there was some *residual phenomenon* of life not capable of being so explained,—the exposition of any laws of motion peculiar to living bodies was premature. But HALLER did not think it incumbent on him to prove this *negative* proposition, before announcing the laws of muscular irritability as distinguished from any merely physical cause of motion; and I believe we shall all now admit, that if he had thought this incumbent on him, the greatest impulse which the science of physiology received during the last century, would have been long, and perhaps indefinitely, postponed.

Fortified by these authorities, as well as by some formerly quoted, I again assert, that the only truly scientific view to be taken of this department of Physiology is, that its object is to ascertain, by the method of induction, to use again the expressions of Professor WHEWELL, "when, and in what manner and degree, *chemical* as well as mechanical agencies are modified, overruled, or counteracted in living bodies, by agencies which must be *hyper-chemical* as well as hyper-mechanical;" and I farther maintain, that the term Vital Affinity is as accurate a term as can be employed as a general expression for these agencies; that, like all other general principles in nature, we may expect it to act according to general laws, and that several of these laws, to which I have referred in this and former papers, are already ascertained, at least, in so far as to shew that the subject is one of legitimate inquiry.

I am aware that it may be still said that this dispute is only a verbal one, and can have no practical or even strictly scientific application. But in answer to this I would observe, that so long as we adhere to the supposition, that there is nothing truly vital or peculiar to living bodies in their economy (as regards their organic functions), except motion, and that motion derived from contraction of solids and impulse, the notions that we can form of the nature of these functions in health, and of the deviations from that state in disease, *must necessarily be erroneous*, because we shall always be looking in the wrong direction for the cause of these phenomena; and that at this precise point the most plausible medical theories of the last, and even of the present age, have gone astray. This, I think, is sufficiently illustrated by the example already given, of the ingenuity of Dr MURRAY wasted in the invention and defence of the hypothesis which ascribed the secretions of animals to varying impulse on their fluids from contracting solids; and I shall only add a single illustration of the same kind drawn from the science of Pathology, and from the most fundamental of all inquiries in it.

the theory of Inflammation. It being sufficiently obvious, that inflammation is strictly a vital process, and one in which the flow of blood through the affected part is materially changed, it was naturally supposed that the vital powers by which that movement is affected in the natural state, must be those which undergo modification in this diseased state; when, therefore, it was believed that the only truly vital power concerned in the organic functions of the living body is one form or other of contractility, the only explanations of the phenomena of inflammation that were attempted turned on the possible modifications of the contractile *powers of vessels*, as influenced by their contents or through their nerves. But I believe it is now pretty generally admitted, that all this was nearly lost labour; and if physiologists had earlier seen that the most fundamental and characteristic of all strictly vital actions,—those by which nutrition and secretion are effected, and which have always more or less of a chemical character, take place, not in vessels, but in cells, independently of any contractions of the organs containing the fluids —that they are most obvious in those living beings which have neither heart, arteries, nor veins;—and that, as occurring in the higher animals, they are carried on partly in the interior of the fluids contained in the vessels, and partly in the matter that has exuded from the vessels and lies exterior to them,—they would sooner have perceived, that all the changes of action *of the organs* of circulation, heart, arteries, or capillaries, in the case of inflammation, are to be regarded as *effects* of the truly essential, fundamental, and strictly vital changes, which take place *in the fluids* of an inflamed part, and in the *relation between the fluids and solids* there; *i. e.*, in matter which is apparently at rest, and much of which, being outside the vessels of the part, has escaped from all influence of the vital contractions either of heart or vessels.

I do not say that we have a satisfactory explanation of inflammation merely by taking this view of it,—regarding it as fundamentally a perversion of nutrition or secretion, and the circulation as only secondarily affected; but I maintain that in this way we can understand, and so far explain, by reference to more general facts, known in the history of the sound as well as the diseased body, many facts as to it, which we never understand at all so long as we think only of *altered action of vessels*,—but which are easily arranged along with others previously known, when we regard them only as indications of changes in vital actions that are constantly going on *in living fluids*, both those contained in vessels, and those recently delivered from them, into the cellular structure *of living parts*. Thus, we can perceive how inflammation should *spread*, as it does, not along the course of vessels, but *from a point as from a centre*,—not only along *continuous* surfaces, but to *contiguous* surfaces lying beside them, but supplied from other vessels, the larger branches of which frequently undergo little or no change in the process; thus we can perceive how the amount of effusions and exudations from the blood in inflamed parts should bear no fixed proportion to any action of the heart, or of

any contractile organ by which it is propelled into those parts,—the most copious effusions sometimes taking place when the impulse of the blood, passing along the larger arteries, is distinctly feebler than natural during the whole disease; thus we can understand how the blood passing through an inflamed part should undergo a change in its own constituents, and how the fluid, which escapes from the vessels there, should possess a peculiar composition, and be peculiarly fitted for certain vital actions, and thereby for repairing some of the injuries resulting from the inflammation itself. Thus, also, we can understand and admit a principle which has been confidently disputed, but which I have long thought, and now find to be maintained, as fairly established, viz., that matter exuding as a result of simple inflammation, may afterwards degenerate, according to the state of the constitution, into various forms of heterologous deposit. (*See e.g. Copland and Quain, in Medico-Chirurgical Transactions*, vol. xxxiii., p. 144.) Still more, if we regard it, as I think we may, as an established fact, that the vital properties of living fluids, as well as solids, are of temporary duration only, and are subject to the general law, of increased action being followed by diminished action, or accelerated loss of vitality, we can understand how the most important consequences of inflammation, both beneficial and injurious, should be produced,—how the matter that was concerned in it being peculiarly excited, and, therefore, quickly rendered *effete*, should be peculiarly liable to Absorption, which we know to be the agent by which its injurious effects are chiefly effaced,—how the increased absorption should, under certain circumstances, extending to the adjoining sound parts, effect that destruction of texture which we call Ulceration; and how, in other circumstances, either of peculiar violence of the inflammation, or depressed vitality of the organ inflamed, this form of diseased action should, by the established laws of vitality, lead to premature death of the diseased part, *i. e.*, either to partial Sloughing or more extensive Gangrene. All these are facts of the highest practical importance, of which we have explanations so far satisfactory, on the strict principles of induction, when we look to the changes that take place in inflamed parts in those living actions which I have referred to the heads of Vital Attractions and Repulsions, and Vital Affinities; but I will venture to say, that we never shall have any explanation of them consistent with the supposition, that the contractions of living solids are the only changes in organic life which are truly vital, *i. e.*, dependent on laws essentially distinct from those that regulate the changes of inorganic matter.

XXV.—*On Meconic Acid, and some of its Derivatives.* By Mr Henry How, Assistant to Dr Anderson. Communicated by Dr T. Anderson.

(Read 5th January 1852.)

In a paper on Comenic Acid, read before this Society in April of last year, and since honoured with a place in its Transactions, I mentioned my being engaged in an investigation on Meconic Acid; the details of the experiments referred to form the matter of the present communication.

My object in undertaking this subject was to ascertain if products corresponding to those described as derived from comenic acid were formed under similar circumstances in the case of meconic acid. I also thought it probable, that as the former is itself a derivative of the latter, the changes undergone by meconic acid in some reactions, would be found to result in substances apparently the immediate derivatives of comenic acid. This remark refers to the action of heat on meconate of ammonia; and it will be seen that the expectation was realised. A similar result was found in other instances, where it had not been anticipated.

The experiments I am about to detail were performed in the laboratory of Dr T. Anderson.

The process employed for the purification of meconic acid was that given by Gregory in his "Outlines," excepting that ammonia was substituted for potass as the solvent of the crude acid. As in the case of comenic acid the volatile alkali was preferred, because, although in both cases a great deal of acid remains in the highly coloured mother liquors, from which it can only be recovered in a pure state at the expense of much time and labour, it was found that if ammonia was used, the whole of the mother liquors could be employed under circumstances where their impure state did not affect the results of the experiment. A considerable saving was thus effected. This point is of some importance, because the numerous solutions requisite for the purification of meconic acid occasion so much loss, that seldom much more than a fourth part of the weight of the crude acid started from, is obtained as the result of a careful preparation.

The process consists in dissolving crude meconic acid in hot water by aid of caustic ammonia. The crude acid is obtained from meconate of lime by treating it three successive times with twenty parts boiling water and three parts strong muriatic acid. The mixture of the acid so obtained, and about twice its weight of water, is kept hot in a water-bath and constantly agitated, till, by the addition of caustic ammonia, solution is complete; the salt formed is extremely soluble in hot water, and the fluid cools to a solid mass. The black mother liquor is squeezed

out by strong pressure, and the cake of salt redissolved twice or thrice in as small a quantity of boiling water as is found sufficient, the mother liquors being always pressed from the crystallised salt. By proceeding in this manner a perfectly white salt is obtained, from whose solution in hot water an excess of strong hydrochloric acid throws down the meconic acid in colourless brilliant scales; these require but a little washing with cold water, and one resolution in the smallest possible quantity of hot water, to be obtained on cooling of the fluid absolutely pure. This is another advantage in the use of ammonia, for the potass salt requires, at the least, three treatments with acid to abstract the alkaline base entirely.

Bibasic Meconate of Ammonia.—The ammonia salt obtained in the above given process, crystallises from tolerably dilute fluids left at rest, in groups of radiated fine silky needles: they have an acid reaction. In the following analysis the nitrogen was determined by adding hydrochloric acid to a solution of the salt, evaporating the filtrate with some bichloride of platinum, collecting the residue on a filter, and washing with alcohol and ether; the per-centage of nitrogen was calculated from the platinum remaining on ignition of the undissolved ammonia salt. This method was preferred in one or two other cases of ammonia salts, as more convenient than a combustion with soda lime, and less liable to loss; for it is not easy always to mix these salts with soda lime so quickly as to avoid the escape of ammonia.

{ 5·170 grains, dried at 212°, gave
{ 6·732 ... carbonic acid, and
{ 2·201 ... water.

{ 5·285 grains, dried at 212°, gave
{ 4·505 ... metallic platinum.

		Calculation.		
Carbon, . .	35·51	35·89	C_{14}	84
Hydrogen, . .	4·73	4·27	H_{10}	10
Oxygen, . .	...	47·88	O_{14}	112
Nitrogen, . .	12·09	11·96	N_2	28
	100·00	100·00		234

The hydrogen is rather above the calculated result, but the substance, when dried at 212°, is extremely hygroscopic: the numbers lead to the formula

$$HO, 2\,NH_4O, C_{14}\,HO_{11}$$

as representing the constitution of bimeconate of ammonia in the dry state; the crystals appear to contain varying amounts of water of crystallisation, as numbers were obtained in drying different specimens indicating a loss of between six and sixteen per cent. of water. An aqueous solution of this salt may be boiled without any change; but when kept for a considerable time boiling with an excess of ammonia, it becomes altered.

Action of Heat on Meconate of Ammonia.

Comenamic Acid.—Some of the highly-coloured mother liquors of the purifying process, were retained at or near the boiling temperature for some hours, ammonia being kept present in excess. The addition of hydrochloric acid to the cooled fluid caused copious evolution of carbonic acid, and when added in proper quantity, a considerable precipitate. By repeated crystallisations from boiling water, and the use of pure animal charcoal, the precipitated substance was obtained in colourless shining scales; the following is its analysis before being rendered absolutely pure:—

{ 4·335 grains, dried at 212°, gave
7·287 ... carbonic acid, and
1·370 ... water.

{ 6·295 grains, burnt with soda lime, gave
8·700 ... ammonio-chloride of platinum.

		Calculation.		
Carbon, . .	45·84	46·45	C_{12}	72
Hydrogen, . .	3·51	3·22	H_5	5
Oxygen, . .	...	41·30	O_8	64
Nitrogen, . .	8·67	9·03	N	14
	100·00	100·00		155

The results of which are sufficient to shew this body to have the composition of comenamic acid; the characters and reactions of the acid left me no doubt as to its identity with that derived from comenate of ammonia under similar circumstances. It may be considered as formed from the bibasic meconate of ammonia, in the presence of an excess of ammonia, by the elimination of two eq. carbonic acid, two of water, and one of ammonia, as in the equation

$$HO, 2\,NH_4O, C_{14}\,HO_{11} = C_{12}\,H_5\,NO_8 + NH_3 + 2\,HO + 2\,CO_2.$$

This offers a convenient source of comenamic acid, as very impure meconic acid may be employed.

Action of Chlorine on Bibasic Meconate of Ammonia.

A current of chlorine gas passed through some of the coloured mother liquor of the above salt deprived it of colour considerably, and caused a speedy deposit of hard granular crystals adhering to the sides of the vessel. This was collected, and recrystallised from boiling water; it was found to be not very soluble, and the fluid on cooling deposited the substance in hard crystals, which, on being magnified, were seen to consist of thick needles radiating from a centre. It contained no chlorine, and proved to be an ammonia salt of meconic acid containing one equivalent of alkaline base. I am not aware that this salt has been obtained before, I therefore subjoin an analysis of it.

{ 4·708 grains, dried at 212°, gave
{ 6·659 ... carbonic acid, and
{ 1·505 ... water.

{ 4·052 grains, dried at 212°, gave, by H Cl, &c.,
{ 1·780 ... platinum.

		Calculation.		
Carbon, . .	38·57	38·70	C_{14}	84
Hydrogen, .	3·55	3·22	H_7	7
Oxygen, . .	...	51·63	O_{14}	112
Nitrogen, .	6·21	6·45	N	14
	100·00	100·00		217

which gives as the formula of the monobasic meconate of ammonia, as dried at 212°,

$$2\,HO,\ NH_4O,\ C_{14}\ HO_{11}$$

The crystallised salt contains two equivalents of water;

{ 9·535 grains lost, at 212°,
{ 0·735 ... water.

which is equal to 7·70 per cent.; 7·65 is the number corresponding to the formula

$$2\,HO,\ NH_4O,\ C_{14}\ HO_{11} + 2\ aq.$$

The original mother liquor of this salt deposited a further quantity of the same on being concentrated; and by continued evaporation a few crystals of a different appearance were obtained; when these were separated, and recrystallised from boiling water, they presented themselves in the form of long square prismatic needles. In their appearance, and a few reactions, they shewed the characters of chlorocomenic acid. A determination of the chlorine is, I think, sufficient to prove that the crystals really consist of this acid,

{ 3·315 grains, dried at 212° gave, after burning with lime,
{ 2·505 ... chloride of silver;

the per-centage of chlorine calculated from this experiment is 18·69, which agrees very closely with 18·63, the number corresponding to the formula of chlorocomenic acid in the dry state.

$$2\,HO,\ C_{12}\left\{\begin{matrix} H \\ Cl \end{matrix}\right\} O_8$$

Oxalic acid is found in the last mother liquors of this process.

Action of Bromine on Meconic Acid.

Bromocomenic Acid; Carbonic Acid.—I had no doubt of finding the action of bromine closely similar to that of chlorine on meconate of ammonia; but it occurred to me it would be more readily learned from employing the acid itself, whether it gave a substitution product, or whether its molecule, under these circumstances, split up at once into carbonic acid and a substitution acid of comenic

acid. Accordingly, bromine water was poured upon powdered meconic acid; lively effervescence took place, which was found to result from the evolution of carbonic acid, and complete solution subsequently ensued. The fluid, when left at rest a considerable time, deposited a few long prismatic crystals of great beauty, a much more copious product was obtained by gentle evaporation. Recrystallisation from hot water gave groups of brilliant square prismatic crystals, of which,

6·787 grains, dried at 212°, gave, when burnt with lime,
5·480 ... bromide of silver.

This experiment gives a per-centage of 34·36 bromine: 34·04 is that corresponding to the formula of dry bromocomenic acid,

$$2\,HO,\,C_{12}\left\{\begin{matrix}H\\Br\end{matrix}\right\}O_8$$

The nature of the reaction is seen in the equation

$$C_{14}\,H_4\,O_{14}+2\,Br=C_{12}\left\{\begin{matrix}H_3\\Br\end{matrix}\right\}O_{10}+HBr+2\,CO_2$$

Crystals of oxalic acid were obtained by evaporating the original mother liquors to a small bulk.

Ethers of Meconic Acid.

When absolute alcohol is poured upon meconic acid, and the mixture is agitated, partial solution takes place, accompanied by a considerable fall in temperature, amounting to about 10° or 12° Fahr.: application of a gentle heat causes complete solution. A stream of hydrochloric acid gas passed through the fluid is attended by the usual result observed in these cases, the formation of an ether compound; but in this instance more than one of such substances are produced, and the relative proportion of the individual products depends on the amount of acid gas and the strength of the alcohol employed; I say the strength of the alcohol, because rectified spirit serves to produce etherification, and I have employed it, but have found it disadvantageous, because whenever I did so, I observed the formation of an uncrystalline compound which very much impeded the purification of the other substances. The large amount of water of crystallisation of meconic acid, amounting to fully 25 per cent., tends to dilute the alcohol, and I have sometimes dried the acid at 212° Fahr. before using it, and found this a good plan when working with rectified spirit.

The results I have observed may be stated in a few words as preface to the description of the individual products; when a current of dry hydrochloric acid gas is passed through an alcoholic solution of meconic acid till it fumes strongly, and the fluid is set aside to cool, there appears, after a shorter or longer time, according to the circumstances above referred to, a deposit in feathery crystals; the fluid filtered from this, where absolute alcohol has been used, gives no further deposit; but, in the case of rectified spirit, another less crystalline substance ap-

pears after some little time. On evaporating the liquid which has ceased to give deposits to complete dryness, the chief constituent of the residue is found to be a substance fusing under boiling water; it is more or less accompanied by the other bodies according to the said conditions.

Ethylomeconic Acid.

The first deposit I have usually found to be so nearly a pure and uniform substance, that one recrystallisation from hot water, after a little washing, was sufficient to render it completely so; it then appeared as highly crystalline in brilliant short needles. The following is its analysis:—

I. { 5·500 grains, air-dry, gave
9·558 ... carbonic acid, and
1·860 ... water.

II. { 5·110 grains, dried in vacuo, gave
8·830 ... carbonic acid, and
1·685 ... water.

	I.	II.	Calculation.		
Carbon,	47·39	47·12	47·36	C_{18}	108
Hydrogen,	3·75	3·66	3·50	H_8	8
Oxygen,	...	...	49·14	O_{14}	112
	100·00	100·00	100·00		228

from which it is obvious that we have here an acid ether, analogous to phosphovinic acid, in which one atom of water of a tribasic acid is replaced by an equivalent of ether; its rational formula is, therefore,

$$2\,HO,\ C_4H_5O\ C_{14}\ HO_{11},$$

according to which it is a bibasic acid: this I shall presently shew to be the case. I propose to call this the ethylomeconic acid, in preference to meconovinic acid, both as more expressive of one of its constituents, and to facilitate its comparison with another ether, to be described shortly, which I should hardly know how to name otherwise than by calling biethylomeconic acid, containing, as it does, two equivalents of ether.

Ethylomeconic acid, when pure, crystallises from boiling water in brilliant small crystals, which, when magnified, are seen to be square prismatic needles. It is very readily soluble in this menstruum, also in ether and common alcohol when warmed, less soluble in absolute alcohol. It separates from concentrated solutions in these three fluids in groups of stellate crystals, and when they are left to spontaneous evaporation, in long needles. It is anhydrous, its crystals lose no weight either in vacuo or at 212° Fahr. It fuses at about 316°–318° Fahr. to a transparent yellowish liquid, with the formation of a sublimate in very brilliant rhombic crystals.

Its aqueous solution reacts strongly acid, and readily coagulates the white of eggs. It imparts to persalts of iron a deep red colour. It decomposes carbonates with effervescence.

It is bibasic, forming two series of salts, the acid ones are readily crystallisable; its salts are very stable, the acid being recoverable from them by decomposition with stronger acids.

Acid Ethylomeconate of Baryta.—When carbonate of baryta is added in successive small quantities to water covering solid ethylomeconic acid, lively effervescence ensues and the acid quickly disappears; there is formed at the same time a small amount of an insoluble yellow salt. If the fluid be filtered immediately on the cessation of the effervescence, and the vessel be placed under the receiver of an air-pump and a vacuum made, a considerable deposit of carbonate of lime, which had been held in solution by the carbonic acid now liberated, takes place. By a second filtration a clear yellowish fluid is obtained, which yields, on evaporation in vacuo or at a gentle heat, very well-defined brilliant rhombic crystals of a yellow colour. A specimen prepared in this way gave the following results:—

5·053 grains, dried at 212°, gave
6·708 ... carbonic acid, and
1·198 ... water.

5·455 grains, dried at 212°, gave on ignition with $HO\,SO_3$,
2·175 ... sulphate of baryta.

		Calculation.		
Carbon,	36·20	36·53	C_{18}	108
Hydrogen,	2·63	2·36	H_7	7
Oxygen,	...	35·19	O_{13}	104
Baryta,	26·17	25·92	BaO	76·64
	100·00	100·00		295·64

which lead to the formula, for the dried ethylomeconate of baryta, of

$$BaO,\ HO,\ C_4H_5O\ C_{14}\ HO_{11}.$$

The crystals contain water which they lose on drying, but I missed ascertaining the quantity.

Acid Ethylomeconate of Silver.—I obtained this salt by adding an aqueous solution of the former to nitrate of silver; a precipitate was immediately formed, which, upon resolution in boiling water, after washing, crystallised out on cooling of the fluid in groups of fine small stellate crystals, brilliant and white. This salt is remarkably stable, remaining perfectly unchanged in appearance when exposed a long time to the diffused daylight of summer; it gave the following results on analysis:—

5·310 grains, dried at 212°, gave
6·215 ... carbonic acid, and
1·053 ... water.

{ 4·595 grains, dried at 212°, gave, on ignition,
{ 1·468 ... silver.

		Calculation.		
Carbon, . .	31·92	32·22	C_{18}	108
Hydrogen, . .	2·20	2·08	H_7	7
Oxygen, . .	...	33·45	O_{14}	112
Silver, . .	31·94	32·25	Ag	108·1
	100·00	100·00		335·1

which lead to the formula

$$AgO, HO, C_4H_5O\ C_{14}\ HO_{11}$$

for the dried salt; the crystals contain two equivalents of water,

{ 10· 40 grains lost, at 212°,
{ 0·545 ... water.

This number gives for per-centage 5·24; 5·08 is that corresponding to

$$AgO, HO, C_{18}\ H_6\ O_{12} + 2\ aq.$$

An aqueous solution of acid ethylomeconate of baryta gives with acetate of lead a yellowish white, with sulphate of copper a pale green, and with perchloride of iron a red-brown precipitate; this last is readily soluble in an excess of the iron salt, forming with it a dark red fluid.

Neutral Salts of Ethylomeconic Acid.—I have not been successful in procuring these salts absolutely pure, although I have tried many. On one occasion I obtained, by saturating ethylomeconic acid as nearly as possible with carbonate of baryta at a temperature of 212°, and subsequent filtering of the fluid, a salt which deposited on cooling in small short yellow needles; of this

{ 3·442 grains, dried at 212°, gave
{ 2·197 ... sulphate of baryta.

The per-centage of baryta calculated from this is 41·89: 42·19 is the number corresponding to the formula

$$2\ BaO, C_4H_5O\ C_{14}\ HO_{11}.$$

Although this result is satisfactory, I could not succeed upon repetition of the experiment in obtaining an analytical number sufficiently close to confirm it. Those I obtained by heating ethylomeconic acid with excess of carbonate of baryta, varied from 42 to 44·5 per cent. baryta, which lead to the conclusion that the acid forms basic combinations in addition to acid and neutral salts. The other alkaline earths shewed similar deportment with the acid, and when it is heated with an excess of carbonate of silver, it remains almost entirely undissolved,—in some basic combination.

When ethylomeconic acid is heated with an excess of caustic potass or soda, meconates of these bases are produced. An excess of caustic ammonia decomposes it very readily.

Meconamidic Acid.

When ethylomeconic acid is dissolved in warm water or alcohol, and an excess of strong aqueous or alcoholic solution of ammonia is added, the fluid assumes a deep yellow colour, and becomes very soon filled with a yellow semi-gelatinous-looking substance, which after being washed with dilute spirit, dries up in the air to an amorphous mass, which powders with some difficulty to a very fine yellow powder. This substance, when dissolved in hot water, smells of ammonia, and the solution gives, with dilute fixed alkalies, abundant evidence of its containing this body as a base. I was at first of the opinion that it was the neutral salt of an amide acid corresponding to ethylomeconic acid, and formed from it in the manner characteristic of the action of ammonia in these cases, in which one atom takes the place of an equivalent of alcohol. Upon submitting it to analysis, however, I found this not to be the case; and it appears to me to be the result of a complicated decomposition, which is, so far as I am aware, without analogy. Upon adding to its solution in hot water some hydrochloric acid, a white precipitate is obtained, which I presume to be the acid of the compound. I will first give its analysis, and the formula I deduce from it, to render more clear the only constitution I can assign to these two bodies. The following analyses were performed on specimens of different preparations, the acid was recrystallised from boiling water, it then appeared as a white crystalline crust or rind:

I.
- 5·972 grains, dried at 212°, gave
- 8·700 ... carbonic acid, and
- 1·775 ... water.
- 5·623 ... dried at 212°, gave, when burnt with soda lime,
- 7·020 ... platinum salt of ammonia.

II.
- 5·884 grains, dried at 212°, gave
- 8·555 ... carbonic acid, and
- 1·760 ... water.
- 5·655 ... dried at 212°, gave, with soda lime,
- 7·250 ... platinum salt.

III.
- 5·205 grains, dried at 212°, gave
- 7·540 ... carbonic acid, and
- 1·530 ... water.

IV.
- 7·925 grains, dried at 212°, gave, with soda lime,
- 9·728 ... platinum salt.

	I.	II.	III.	IV.	Mean.	Calculation.		
Carbon, .	39·73	39·65	39·50	...	39·62	39·84	C_{84}	504
Hydrogen, .	3·30	3·32	3·26	...	3·29	3·08	H_{39}	39
Oxygen, .	...	...	...	...	...	49·34	O_{78}	624
Nitrogen, .	7·84	8·05	...	7·70	7·86	7·74	N_7	98
	100·00	100·00	100·00	100·00	100·00	100·00		1265

The first glance at the above formula reveals a very complex atom, yet in the following scheme its derivation seems at least possible. Seven atoms of ammonia react on six of the acid ether,

6 atoms ethylomeconic acid,	.	$C_{18}\ H_{8}\ O_{14}$	$=C_{108}\ H_{48}\ O_{84}$
+7 ... ammonia,	. . . . .		$H_{21}\ N_{7}$
			$C_{108}\ H_{69}\ O_{84}\ N_{7}$
−6 ... alcohol,	. . . . .		$C_{24}\ H_{36}\ O_{12}$
			$C_{84}\ H_{33}\ O_{72}\ N_{7}$
+6 ... water,	. . . . .		$H_{6}\ O_{6}$
			$C_{84}\ H_{39}\ O_{78}\ N_{7}$

If we consider the above as its derivation, and the six atoms of water as water of crystallisation retained at 212°, the acid will be

$$C_{84}\ H_{33}\ N_{7}\ O_{72}\ ;$$

upon examining this, it is found to contain the elements of six atoms of normal amidomeconic, corresponding to ethylomeconic acid, plus an equivalent of ammonia,

$$C_{84}\ H_{33}\ N_{7}\ O_{72} = 6(2\ HO, NH_{2}\ C_{14}\ HO_{10}) + NH_{3}.$$

A comparison of the numerical per-centages required by the formula of the normal amidomeconic acid, with those really obtained, is here given to shew how widely they differ,

	Mean of Experiment.	Calculation of Amidomeconic Acid.		
Carbon, . .	39·62	42·21	C_{14}	84
Hydrogen, . .	3·29	2·51	H_{5}	5
Oxygen, . .	...	48·25	O_{12}	96
Nitrogen, . .	7·86	7·03	N	14
	100·00	100·00		199

Yet that the acid in question is really an amidogen compound resulting from meconic acid, is to be inferred from the fact, that when it is heated with solution of potass, ammonia is evolved in quantity, and the fluid gives, with hydrochloric acid, a crystalline precipitate to be recognised as bimeconate of potass, which, upon subsequent treatment in the same manner, furnishes the characteristic scales of meconic acid. This is the deportment of an acid amide.

The appropriation of the atom of ammonia among the six atoms of amidomeconic acid, if, indeed, this be the constitution of the compound acid produced, seems to have much diminished the basicity of the complex atom, or else the yellow salt is not a neutral one: the amidomeconic acid being bibasic, six of its atoms should, in forming a neutral salt, take up twelve equivalents of ammonia, but a considerably less amount is found in the yellow ammonia salt: this is

shewn in the following analyses; they were performed on specimens prepared at different times.

I. { 6·277 grains, dried a day at 212°, gave
8·395 ... carbonic acid, and
2·60 ... water.

II. { 6·150 grains, dried in vacuo, gave
8·205 ... carbonic acid, and
2·750 ... water.
5·751 ... dried in vacuo, gave, burnt with soda lime,
14·650 ... platinum salt of ammonia.

III. { 4·912 grains, dried in vacuo, gave, burnt with soda lime,
12·580 ... platinum salt.

IV. { 4·925 grains, dried in vacuo, burnt with soda lime, gave*
12·445 ... platinum salt.

	I.	II.	III.	IV.	Calculation.		
Carbon, .	36·47	36·38	...	...	36·23	C_{84}	504
Hydrogen, .	4·60	4·96	...	...	4·52	H_{63}	63
Oxygen, .	...	...	...	...	43·01	O_{75}	600
Nitrogen, .	...	15·99	16·08	15·86	16·24	N_{16}	224
	100·00	100·00	100·00	100·00	100·00		1391

The formula expressive of the constitution of this substance as an ammonia salt of the above acid, is,

$$9\,NH_4O,\,C_{84}\,H_{24}\,N_7\,O_{63} + 3\,aq.$$

And the acid itself, considered with regard to its amount of basic water as indicated in the salt, is represented thus,

$$9\,HO,\,C_{84}\,H_{24}\,N_7\,O_{63} + 6\,aq.$$

I attempted to determine directly the amount of nitrogen existing in the yellow salt as ammonia, but, upon reflection, I despaired of success, because the only method at my disposal being to decompose by hydrochloric acid, and evaporate the solution filtered from the precipitated amidic acid with bichloride of platinum, I saw that if this acid behaved as amidogen acids are known to do in concentrated acid fluids, namely to regenerate the parent acid and ammonia, I should inevitably obtain an excess. Nevertheless I made the experiment, and the lowest result I obtained was 10·4 per cent. nitrogen: now, 9·08 corresponds to 9 atoms of nitrogen. I also attempted to form other salts by precipitation of solutions by that of the ammonia salt, but the results were unsatisfactory and inconstant. The silver salt, a yellow gelatinous precipitate, dried up to a black mass; and the baryta compound, a yellow amorphous precipitate, insoluble in boiling water, gave varying numbers on analysis.

* I am indebted for this analysis to my friend Mr Rowney. He performed it on the substance mixed with sugar.

I have nothing to add descriptive of the acid to what little has been mentioned. It is a white powder as precipitated by acids from the yellow compound, crystallising from concentrated solution in hot water in a crystalline crust.

The yellow salt has a peculiar appearance. It does not present the least crystalline structure even under the microscope, but consists of round translucent granules; when deposited slowly from dilute fluids these have the appearance of small yellow vesicles or air-bubbles. It is readily soluble in hot water with a decided smell of ammonia; it is very sparingly soluble in hot, insoluble in cold, alcohol. It gradually loses ammonia when heated in the dry state at 212° Fahr.; at a higher temperature it blackens and fuses.

I have adopted the name of Meconamidic Acid for the acid of this salt, as simply expressive of its constituents, without any reference to the molecular arrangement of its elements.

Coupled Acid Ether of Meconic Acid.

The substance I have described as occurring in the process of making the ethers of meconic acid, when rectified spirit is employed, is deposited generally after the first product of ethylomeconic acid is filtered off. I have sometimes observed it also falling from the mother liquor, from which the first deposit had been crystallised, and also in the course of purification of the residue left on evaporation of the original acid mother liquor. Its constant occurrence induced me to examine if it were a substance of determinate composition; I accordingly redissolved some of it in hot water, it which it is extremely soluble, twice or thrice, and obtained, on cooling of the liquid, a white amorphous powder. I select the analyses of two specimens treated in this manner:—

I. { 4·660 grains, dried at 212°, gave
7·655 ... carbonic acid, and
1·311 ... water.

II. { 5·335 grains, dried at 212°, gave
8·712 ... carbonic acid, and
1·315 ... water.

	I.	II.	Calculation.		
Carbon, . .	44·80	44·53	44·85	C_{32}	192
Hydrogen, .	3·12	2·73	2·80	H_{12}	12
Oxygen, .	...	...	52·35	O_{28}	224
			100·00		428

I am inclined to think the approximation of the above numbers to the per-centages corresponding to the formula given, in a substance purified from different preparations, is too close to be accidental, and that the body in question is a determinate compound. The formula given contains the elements of one atom of meconic and one of ethylomeconic acid,

$$C_{32}\,H_{12}\,O_{28} = 3\,HO,\,C_{14}\,HO_{11} + 2\,HO,\,C_4H_5O\,C_{14}\,HO_{11}.$$

A substance of such constitution may be easily imagined to occur, when an insufficient quantity of acid gas had been employed to remove all the water from the meconic acid, or its power had been diminished in this respect by the ready formed water existing in the fluids.

That the substance is something more than an accidental mixture, is to be inferred from the action of ammonia. When its warm aqueous solution is supersaturated by strong ammonia, the fluid becomes yellow, but none of the yellow amidic salt is deposited, as might be expected in a mixture containing ethylomeconic acid. If, however, to a concentrated aqueous ammoniacal solution strong alcohol be added, a deposit in small radiated yellow silky tufts appears; and when such an aqueous solution is evaporated to dryness at 212°, a crystalline residue remains, part of which is extremely sparingly soluble in boiling water; the more soluble portion gives, with hydrochloric acid, a crystalline precipitate in the form of needles. I have not followed out the changes which these few experiments seem to indicate, for my material was small in quantity, and I had no means of readily preparing it tolerably pure at will.

I have called this substance Meconoethylomeconic Acid, as the name expresses most distinctly the constitution deduced from analysis, and represented by the formula given. I was anxious to have substantiated its constitution as such by a determination of its saturating capacity, but was unable to effect my purpose, owing to the impossibility I experienced of obtaining its salts. When it is treated with bases, the salts produced decompose into meconates with greater facility than those of ethylomeconic acid.

Meconic Ether containing two Equivalents of Ether.

Biethylomeconic Acid.—This substance is found in considerable quantity in the acid mother liquors from which the bodies before described have deposited, especially when absolute alcohol has been employed; its proportionate amount appearing to depend on that of the hydrochloric acid gas employed. It remains, on evaporation of the liquid, till acid ceases to be evolved at 212° Fahr., as a thick oil or viscid mass, becoming a solid crystalline mass on cooling. It may be rendered pure by two or three crystallisations, these serving to remove any of the former-mentioned bodies, of which small quantities are generally present in the residue left on evaporation. It is thus obtained in colourless flattened prisms: the analysis is as follows:—

I. { 4·745 grains, dried in vacuo, gave
8·932 ... carbonic acid, and
2·055 ... water.

II. { 4·865 grains, dried in vacuo, gave
9·160 ... carbonic acid, and
2·120 ... water.

	I.	II.	Calculation.		
Carbon, . .	51·33	51·35	51·56	C_{22}	132
Hydrogen, .	4·81	4·84	4·68	H_{12}	12
Oxygen, .	...	...	43·76	O_{14}	112
	100·00	100·00	100·00		256

These numbers lead to the formula,

$$HO, 2\,C_4H_5O, C_{14}\,HO_{11}.$$

Having thus far succeeded in replacing one and two of the atoms of basic water of meconic acid by corresponding equivalents of ether, I was in hopes of being able to go still further and obtain a neutral compound. For this purpose I distilled some meconic acid with absolute alcohol and strong sulphuric acid. By application of a gentle heat, tranquil ebullition was commenced and sustained. The distillate consisted of alcohol and ether, and the contents of the retort gradually acquired a syrupy consistence; at this period they were poured into a comparatively large quantity of cold water; in a short time a crystalline precipitate of a delicate rose-pink colour was formed, which gradually increased in quantity. On recrystallisation from water it was obtained in colourless flattened prisms, which gave, on analysis, the following numbers:—

4·860 grains, dried at 212°, gave
9·128 ... carbonic acid, and
2·135 ... water.

which, when calculated for per-centages, are equal to

Carbon, . . .	51·22
Hydrogen, . . .	4·88

and shew the substance to be identical with that obtained in the former process. This method obviously furnishes a ready source of the pure ether. I may mention, that I have not been able to produce it this way by employing rectified spirit in place of absolute alcohol.

Biethylomeconic acid, in its pure state, as crystallised from water, occurs in the form of long, flattened, colourless, prisms; it fuses under boiling water before dissolving. It is very soluble in alcohol. In the dry state it fuses at about 230° Fahr. to a yellowish transparent liquid.

Its aqueous solution readily coagulates the white of eggs, has an acid reaction, and decomposes carbonates with effervescence. It imparts to persalts of iron a red colour.

As the above formula indicates, it is a monobasic acid; I add the analysis of two salts which shew this fact.

When subjected to the action of ammonia in the cold, biethylomeconic acid does not undergo decomposition; the substances simply enter into combination.

Biethylomeconate of Ammonia.—Some of the ether was dissolved in strong, nearly absolute, alcohol, and dry ammoniacal gas was passed into the fluid; the whole soon became a nearly solid yellow mass. When this was freed by pressure from the ammoniacal alcohol, it was found to crystallise from hot spirit in tufts of radiated silky yellow needles. From its analysis,

5·140 grains, dried in vacuo, gave
9·055 ... carbonic acid, and
2·610 ... water.
5·825 ... dried in vacuo, gave, on burning with soda lime,
5·065 ... platinum salt of ammonia.

		Calculation.		
Carbon, . .	48·04	48·35	C_{22}	132
Hydrogen, . .	5·64	5·49	H_{15}	15
Oxygen, . .	...	41·04	O_{14}	112
Nitrogen, . .	5·46	5·12	N	14
	100·00	100·00		273

Its constitution is evidently represented by the formula,

$$NH_4O, 2\,C_4H_5O, C_{14}\,HO_{11};$$

it crystallises without water.

Biethylomeconate of ammonia is readily soluble in cold water to a yellow fluid; acids precipitate from this the unchanged ether. Its aqueous solution gives the following reactions:—with nitrate of silver a yellow gelatinous precipitate insoluble in boiling water, and apparently unaltered by the elevation of temperature; with sulphate of copper, a green gelatinous precipitate; with acetate of lead, a heavy yellowish white, and, with sulphate of magnesia, a crystalline precipitate; with the chlorides of barium, strontium, and calcium, it produces pale yellow semi-gelatinous precipitates, insoluble in boiling water, but readily soluble in excess of the earthy salts; a determination of the base in the baryta salt was made,

5·533 grains, dried at 212°, gave
1·985 ... sulphate of baryta.

		Calculation.		
Carbon, . .	...	40·78	C_{22}	132
Hydrogen, . .	...	3·39	H_{11}	11
Oxygen, . .	...	32·15	O_{13}	104
Baryta, . .	23·54	23·68	BaO	76·64
	100·00	100·00		323·64

which leads to the formula for biethylomeconate of baryta, of

$$BaO, 2\,C_4H_5O, C_{14}\,HO_{11}.$$

I believe, from an experiment made on a small scale, that biethylomeconic acid, when *heated* with ammonia, undergoes a change; the result is probably an

acid amide; want of material, however, has prevented me as yet from arriving at a satisfactory conclusion on this point.

I subjoin a list of the substances described in this paper.

Salts and Compounds of Meconic Acid.

Bibasic meconate of ammonia,	dried at 212°,	$HO, 2\,NH_4O, C_{14}HO_{11}$
Monobasic	...	$2\,HO, NH_4O, C_{14}\,HO_{11}$
... ...	crystallised,	$2\,HO, NH_4O, C_{14}\,HO_{11} + 2$ aq.
Ethylomeconic acid, ...	...	$2\,HO, C_4H_5O\,C_{14}\,HO_{11}$
Acid ethylomeconate of baryta,	dried at 212°,	$BaO, HO, C_4H_5O\,C_{14}\,HO_{11}$
... ... silver,	...	$AgO, HO, C_4H_5O\,C_{14}\,HO_{11}$
... ...	crystallised,	$AgO, HO, C_4H_5O\,C_{14}\,HO_{11} + 2$ aq.
Neutral ... baryta,	dried at 212°,	$2\,BaO, C_4H_5O\,C_{14}\,HO_{11}$
Meconamidic acid, . .	...	$9\,HO, C_{84}\,H_{24}\,N_7\,O_{63} + 6$ aq.
Meconamidate of ammonia,	dried in vacuo,	$9\,NH_4O, C_{84}\,H_{24}\,N_7\,O_{63} + 3$ aq.
Meconoethylomeconic acid,	dried at 212°,	$3\,HO, C_{14}\,HO_{11} + 2\,HO, C_4H_5O\,C_{14}\,HO_{11}$
Biethylomeconic acid, .	...	$HO, 2\,C_4H_5O, C_{14}\,HO_{11}$
Biethylomeconate of ammonia,	crystallised,	$NH_4O, 2\,C_4H_5O, C_{14}\,HO_{11}$
... baryta,	dried at 212°,	$BaO, 2\,C_4H_5O, C_{14}\,HO_{11}$

Products of Decomposition.

Comenamic acid, ...	$HO, C_{12}\,H_4\,NO_7$
Chlorocomenic acid, ...	$2\,HO, C_{12}\left\{\begin{matrix}H\\Cl\end{matrix}\right\}O_8$
Bromocomenic acid, ...	$2\,HO, C_{12}\left\{\begin{matrix}H\\Br\end{matrix}\right\}O_8$

XXVI.—*Notice of an Antique Marble Bust.* By Andrew Coventry, Esq.

(Read February 16, 1852.)

Having had the good fortune last autumn to get an antique marble bust of extreme beauty, the question naturally arose, of whom it might be the portrait, if, indeed, it was a portrait at all, and not an ideal head. I had proceeded some way in this inquiry, when it was suggested to me one day that it might interest the Society to know something of it, and that, though a little foreign no doubt to its usual topics, the change would be agreeable, and that ancient art was not without its charms. So urged I yielded,—perhaps too easily; but of this you will judge when I have done.

Unfortunately the history of the bust, before it became mine, is altogether unknown to me, further than that it belonged to a gentleman in Westmoreland, who, there is reason to think, picked it up whilst travelling in Italy. And I am sorry that owing to his absence once more abroad, wandering about with uncertain health, and often changing his residence, I have been unable as yet to learn anything of its early history. Before going further, I may mention that the bust would have been here to-night for exhibition if I had found it possible to remove it from my house with any safety. It would have been attended, however, with considerable risk, as there are several joinings, particularly in the back of the shoulders, and it is altogether a little crazy. But in its place I have brought some photographs executed by my friend Captain Scott, R.N., and one or two very delicate photographs, with collodion upon glass, by Mr Tunny of Newington. These really leave no room for disappointment or regret. In truth, they shew the features more perfectly than an exhibition of the bust itself, in the full blaze of gas light, without shadows or relief, could possibly have done. A single light, no doubt, from a torch, or day-light entering by a side-window and casting shadows, shews it to most advantage; and I only hope this may induce any gentleman present, who feels so far interested, to call at my house, when it will give me the greatest pleasure to shew it him.

The bust is that of a young woman of serene and pensive beauty. The head is of Parian marble, but the drapery of Carrara, and seemingly of a later age. Probably at an early period some accident befel it, for the Carrara marble extends from the drapery upwards a little way to a crack in the neck, and the nose and the knot of the hair have been slightly injured and restored with Carrara marble. It was not unusual, as we know,* for the head to be wrought separately from the

* Burton's Rome, 2. 203.

rest of a statue; such was the case with the Niobe and her children. Frequently, also, the original head was displaced for another to save expense.* "Pliny tells us, that in his time it was a common custom to change the heads of illustrious persons and fit on new ones; and Chrysostom reproaches the Rhodians with their economy in dedicating the same statues to different persons, defacing the original inscriptions." But in the present instance it is more probable that the circumstance of the bust being in two pieces must have been owing to a fall, as the junction is clumsily executed, and advantage has not been taken of the drapery to conceal it. Still the drapery cannot be referred to any recent period. It is too simple, and has suffered too much from the action of the weather to be modern. I think the bust must have lain for ages with the face down, and the shoulders, which have chiefly suffered, exposed; and, when it came into my possession, the folds of the drapery were full of what seemed garden mould.

It is difficult to resist the impression that we have here a specimen of high Greek art. There is the wonderful repose which baffles modern skill, the fine short upper lip, the flat pupil of the eye, and the delicate line of junction of the lips admirably given.

My belief, too, is that it is a portrait. It has an air of individuality about it; and it has none of the emblems of mythology, such as the diadem or the ivy chaplet. Further, there is a dimple on the chin, which would appear to be decisive. For Winckelmann† informs us, that there exist only three fine statues of an ideal character (the Venus de Medici, a bronze Apollo, and a Bathyllus at Samos) with a dimpled chin, it not being a feature which the Greeks admired. I may mention that the ears are pierced, as was not unusual. The ears of the Venus de Medici are also pierced.

Of whom, then, have we here the portrait? At first sight this would seem a hopeless inquiry; and if the Greeks had been in the habit, as we are, of decorating their mansions with the images of their friends, it certainly would be hopeless now, among the ruins and remains of so many families, to trace the likeness of a bust. But it was not so in Greece. There sculpture had high and public aims. There were, as Heeren‡ tells us, no private galleries and no private collections. Sometimes, indeed, an Athenian, out of piety or patriotism, commissioned a statue; but, in all cases, it was to adorn a temple or a portico, or some place of public resort: and we read§ of a person who had spent between £600 and £700 in certain votive statues, whose heir was reproached with having let them lie in the sculptor's hands unconsecrated. In this way it came that persons only of some public mark were honoured with statues; and we now have not so boundless and discouraging a field as it might have been.

* Burton's Rome, 2. 307; Pliny, 35. 2.
† Winckelmann on Greek Art, p. 220.
‡ Heeren's Greece, pp. 284–9.
§ Müller's Ancient Art, p. 65.

At Rome I believe with Heeren that it was much the same during the Republic, and private galleries were unknown. After the taking of Corinth,* however, a passion seems to have sprung up in Italy for possessing works of art, the generals and governors of provinces vieing with each other in having them. Verres plundered in Sicily and Achaia; yet, with one exception (if it be one), it was statues which had graced some temple, or had been the pride of a city, that he was charged with having carried off.† And with his rapacity Cicero‡ contrasts the conduct of Marcellus and Mummius, who, with the whole spoils of Syracuse and Corinth at their command, had appropriated not a picture or statue, but given all to their country. But Verres soon had many followers; and by the time of Juvenal§ we find that ancestral busts, but still of men who had filled some curule office, were objects of ambition with the degenerate nobles having the *jus imaginum*, the more opulent devoting a room in their houses to their reception, or using them to ornament their gardens.‖ Yet the possession of works of art long survived as a matter of municipal pride in cities, casting private galleries, we may believe, into the shade. And thus it happens, that long after the Roman arms had swept the land, we find a town in France purchasing a statue of Mercury from a Greek artist at no less a sum than £320,000 (forty millions of sisterces), as Sir James Stephen¶ relates. And the same spirit lingers in Rome and Florence to the present day.

The conclusion to which this little digression leads us is, that among the Romans as among the Greeks, statues of private persons were unknown; and such statues as did exist were rarely private property till near the age of Augustus, which is the period, as it will appear, that interests us.

To return to the bust;—its resemblance to the young Augustus was remarked to me very soon by several friends. I discovered, however, on comparing it with casts of his daughter, that it was not the profligate Julia; and much in the same way I satisfied myself that it was not Livia, of whom there is a beautiful portrait in the Dactyliotheca Smithiana.** But in my search I came upon a certain amount of evidence for its being his sister Octavia, the grandniece of Julius Cæsar, whose affecting history is too well known to require more than a passing allusion here. She was, as many may remember, the mother of the young Marcellus,—Virgil's friend too,—married young to the faithless Antony, yet did it "never taint her love,"—and who, through her whole life, toiled for her brother and her country, without one thought of self, till, as Shakspeare†† tells us, "each heart in Rome did love and pity her." In all the three English dramas

* Smith's Dictionary of Greek and Roman Antiquities, p. 908; and Müller, pp. 124–5.
† Cicero in Verrem, II. I., 19 and 23; Heeren, p. 288.
‡ Ibid. II. I., 21. § Juvenal, Satire VIII., 1–19.
‖ Smith's Dictionary, *voce* "Pinacotheca;" and Adam's Antiquities, p. 460.
¶ Lectures on French History, I., 21. ** Vol. i., 62.
†† *Antony and Cleopatra*, Act III., Scene 3.

founded on Antony and Cleopatra (Shakspeare's "Antony and Cleopatra," Dryden's "All for Love," and "The False One" of Beaumont and Fletcher), we find Octavia brought little upon the stage, as if so much worth and beauty must have robbed Cleopatra of dramatic interest.

But to proceed. The *first* thing that struck me in reference to the bust was the very great resemblance, as I have said, which it has to the young Augustus. It is really most remarkable. The same gentle rise of the nose,—the same breadth of forehead, in contrast with a tapering chin—the same small mouth,—and the same low setting of the ears,—these are points of which any one may satisfy himself by inspecting the antique casts in the adjoining room. Suetonius,* to whom we owe the full description of Augustus's appearance, specially dwells upon the delicacy of his features and the singularly tranquil and serene look he always had; and Mr Merivale (2. 465) following him, speaks of "the graceful beauty of his mouth, and chin of almost feminine delicacy." Now, curiously enough, these are the most obvious peculiarities of this bust. The precise features of Octavia herself are nowhere given that I can find; and I have searched Dion Cassius, Seneca, Aurelius Victor, and Plutarch, besides Suetonius, being curious to trace to some authentic source, if it were possible, the round face and the low brow which Shakspeare has given to his Octavia.

2d, Of Octavia there was, some years ago, a bust at Rome in the Capitol, as I am informed. Two friends of mine who had often seen it and admired it, upon visiting the bust in my possession, immediately recognised the resemblance between the two. What has become of it I cannot say; but it would appear that it must have changed either its local habitation or its name, there being no bust of Octavia there now. So I have been given to understand by a young friend at present in Italy (Mr James Swinton); but, of course, it is a matter to be further inquired into.

3d, In the "Signorum Veterum Icones" of Gerard Reynst, p. 26, there is an engraving which professes to be of Octavia; and it certainly is not of Octavia Augusta, the unfortunate wife of Nero, of whom a portrait follows at page 36. Now, it has the hair parted in the middle, as in the bust in my possession,—the same short upper lip,—the same dimpled chin,—and, I should say, the same low brow. The engraving, indeed, gives the idea of a fuller and rather a coarser face, perhaps the fault of the draftsman, but the general likeness is considerable.

4th, In the Dactyliotheca (I., 67), there is a portrait which possesses a peculiar kind of interest, not that it represents Octavia, but Antonia Augusta, her second daughter by Marc Antony; and in it I think one may see a great resemblance to the bust. This was her favourite daughter, the one that inherited her virtues and

* Suetonius, *voce* "Octavius;" and Arnold's Roman Commonwealth, II., 406.

her misfortunes; and, what is more pertinent, her looks, as I find mentioned in the Life of Octavia, which is generally ascribed to the Abbé St Real.*

With regard to coins and medallions, I have not found them of much use. Through the kindness of friends in the British Museum, I have had casts from the unique gold coin there, and from some copper coins of Thessalonica. I have also consulted an engraving of the Vienna medallion in the "Numismata Austriaca," but all with little benefit. Without going into details, I may mention that I have not spared myself a weary pilgrimage through Spanheim, Rashe, Golzius, Æneas Vicus, King, Pelerin, Mionnet, Ackerman, Smith, and Eckhel,—with this result, that the greatest uncertainty attaches to the coins of Octavia. In the copper coins of Thessalonica, for instance, the female head is generally thought to be one of Liberty, and not of Octavia. Again, the only coin which bears the name "Octavia" on it, is considered by many (Mr Burgon of the British Museum among others) to be false, the true one giving Livia; and as to the coins (Cistophori) with Antony's head beside a female head, there is great reason to suppose that it is not Octavia's, but Cleopatra's. Indeed, I have been shewn by my friend Mr William Scott, an engraving of one with the name "Cleopatra" actually occurring on it. For our purpose, it is enough, perhaps, that not two of the coins agree in their representation of Octavia, if it be Octavia that they give.

5th, Will it be thought fanciful if I add, as some corroboration, though trifling, that the bust is in perfect harmony with all we know of the history and character of Octavia. I think we may trace in it that wonderful beauty which we know was not eclipsed by her rival Cleopatra—that gentleness which made her so forgiving of her unworthy lord,—that serenity which was unruffled amidst countless wrongs,—that affection which tied her† to the last to his house and kindred,—and that pensive look, the "*frons læta parum*," even in youth, which foreshadowed in her case a broken heart.‡ I am not sure that all these things could be said of any other individual of those times. As it seems to me, the bust has, for example, too much feeling for Livia, the hard step-mother, as Tacitus§ calls her, and too much purity for either of the Faustinas; and so of many others, if we cared to follow out this view.

6th, and *lastly*. In looking over the Florentine gallery the other day, I was struck by an observation which I could scarcely avoid making, of the simple way in which it was usual for ladies to dress their hair in the time of Augustus, much as in our bust. I might refer to the heads of Livia and Antonia Augusta in that collection, as instances. But very soon the taste for that simplicity declined, and then we have Agrippina, Messalina, Nero's Octavia, Plautina, Poppæa, and a host of others, all revelling in most fantastic locks, some of them artificial, or

* Œuvres de S. Real, III., 295. † Merivale's Roman Empire, 3. 283–4.
‡ Seneca, "Ad Marciam."
§ Tacitus, Annals, I., 10. "Gravis in rempublicam mater, gravior domui Cesarum noverca."

with a fillet of hair bound round the head. If this observation be correct (and I have since found it in Muller),* then it furnishes us with one presumption more for the bust being that of Octavia; since, if it must belong to her age, it is no stretch to say that it may more fairly be given to her than to any other, when we take into account its perfect accordance with her character, and its resemblance to her brother, Augustus.

Such are the various grounds on which I should be disposed to rest. That they amount to proofs I do not pretend, for well I know the difficulty in all such matters of getting more than presumptions. Uncertainty hangs over too many of the finest remains of antiquity, making the Clite of one person the Isis of another, and raising a question, whether the beautiful Ariadne in our adjoining room is not, after all, a Bacchus, as Visconti and the latest editor of Winckelmann† maintain. Enough, then, if I may be thought to have adduced reasonable grounds of belief, and all that could be hoped for at the end of nineteen centuries, with no contemporary record of the features, and scarce a relic left to guide us.

The bust may have been made at Rome by some of the numerous Greek artists who flocked there, encouraged by Cicero and Atticus. Octavia‡ was more than once at Athens, the idol and the charm of it, but this was as a married woman,—and the bust must have been made before her marriage, if we may safely judge by the hair tied behind in a knot, and not as matrons were in the habit of wearing it. There is no reason for supposing that the drapery may not be of high antiquity. The Carrara, or, as they were then termed, the Luna marble quarries, were open before her day, in the time of Julius Cæsar. §

Since preparing this notice, I received by to-day's post the following very interesting communication from Mr Burgon of the British Museum, which was sent me by Sir David Dundas.

"Mus. Brit., *Feb.* 14, 1852.

"Dear Sir David,

"I beg to return my best thanks to your friend for his very kind compliance with my suggestion, in sending me two new photographs. I hope they may be thought to have been productive of some fruit. I have done my best in coming to a conclusion, and have made up my mind to suggest that the bust represents *Antonia*, the daughter of M. Antonius and Octavia. She was the wife of Drusus, and the mother of Germanicus and of Claudius, who struck coins in her honour. She was a personage of high celebrity and a very likely person to have a fine bust, having had the honour of numismatic deification at least.

* Müller's "Ancient Art and its Remains," pp. 169–70.
† Winckelmann, p. 96.
‡ Merivale's Roman Empire, III., 309.
§ Burton's Rome, I., 22; and II., 303.

"In adopting this opinion I have been led by the best of all guides, *an inscribed coin*, of the second brass series, not very uncommon.

"My colleague, Mr Oldfield, agrees with me in thinking, that the coins and the photographs are as similar as could possibly be expected.

"I have the honour, &c.,

"T. BURGON."

How interesting this revelation at the eleventh hour, and how curiously it dovetails into what I had written! It may be remembered that I had mentioned the likeness of *Octavia* to Augustus,—the treatment of the hair peculiar to that age,—and the accordance of her character with the bust; but all these remarks are quite as applicable to her daughter. They were both of them singularly beautiful, singularly amiable, and singularly unfortunate, as I had remarked; and what I regretted as wanting for Octavia, the evidence of coins, has been found for Antonia Augusta by the industry of Mr Burgon and his colleague. On their authority we may safely say, that the scale preponderates for ANTONIA AUGUSTA; and so the question may be considered as set at rest, and the riddle of the bust solved.

XXVII.—*On the Centrifugal Theory of Elasticity, and its Connection with the Theory of Heat.* By William John Macquorn Rankine, C.E., F.R.S.E., F.R.S.S.A., &c.

(Read December 15, 1851.)

Section First.—*Relations between Heat and Expansive Pressure.*

(1.) In February 1850, I laid before the Royal Society of Edinburgh a paper, in which the laws of the pressure and expansion of gases and vapours were deduced from the supposition, that that part of the elasticity of bodies which depends upon heat, arises from the centrifugal force of the revolutions of the particles of elastic atmospheres surrounding nuclei, or atomic centres. A summary of the results of this supposition, which I called the Hypothesis of Molecular Vortices, was printed in the Transactions of this Society, volume xx., as an introduction to a series of papers on the Mechanical Action of Heat; and the original paper has since appeared in detail in the Philosophical Magazine.

In that paper, the *bounding surfaces of atoms* were defined to be imaginary surfaces, situated between and enveloping the atomic nuclei, and symmetrically placed with respect to them, and having this property—that at these surfaces the attractive and repulsive actions of the atomic nuclei and atmospheres upon each particle of atomic atmosphere, balance each other. The pressure of the atomic atmospheres at those imaginary boundaries is the part of the total expansive pressure of the body which varies with heat; the effect of the centrifugal force of molecular vortices being to increase it.

In the subsequent investigation it was assumed, that owing to the symmetrical action of the particles of gases in all directions, and the small amount of those attractive and repulsive forces which interfere with the elasticity of their atmospheres, no appreciable error would arise from treating the boundary of the atmosphere of a single atom, in calculation, as if it were spherical; an assumption which very much simplified the analysis.

An effect, however, of this assumption was, to make it doubtful whether the conclusions deduced from the hypothesis were applicable to any substances except those nearly in the state of perfect gas. I have, therefore, in the present paper, investigated the subject anew, without making any assumption as to the arrangement of the atomic centres, or the form of the boundaries of their atmospheres. The equations deduced from the hypothesis, between expansive pressure and heat, are therefore applicable to all substances in all conditions; and it will be seen that they are identical with those in the original paper; shewing that the assump-

tion, that the atomic atmospheres might be treated in calculation as if spherical, did not give rise to any error.

By the aid of certain transformations in those equations, I have been enabled, in investigating the principles of the mutual transformation of heat and expansive power, to deduce JOULE'S *law* of the equivalence of heat and mechanical power directly from them, instead of taking it (as I did in my previous papers) as a consequence of the principle of vis-viva. CARNOT'S *law* is also deduced directly from the hypothesis, as in one of the previous papers.

(2.) *Classification of Elastic Pressures.*—The pressures considered in the present paper are those only which depend on the volume occupied by a given weight of the substance; not those which resist change of figure in solids and viscous liquids. Certain mathematical relations exist between those two classes of pressures; but they do not affect the present investigation.

To illustrate this symbolically, let V represent the volume occupied by unity of weight of the substance, so that $\frac{1}{V}$ is the mean density; Q, the quantity of heat in unity of weight, that is to say, the vis-viva of the molecular revolutions, which, according to the hypothesis, give rise to the expansive pressure depending on heat; and let P denote the total expansive pressure. Then,

$$P = F(V, Q) + f(V) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

In this equation, $F(V, Q)$ is the pressure of the atomic atmospheres at the surfaces called their boundaries, which varies with the centrifugal force of the molecular vortices as well as with the mean density; and $f(V)$ is a portion of pressure due to the mutual attractions and repulsions of distinct atoms, and varying with the number of atoms in a given volume only. If the above equation be differentiated with respect to the hyberbolic logarithm of the density, we obtain the coefficient of elasticity of volume

$$\frac{1}{\mathfrak{d}} = -\frac{d\,P}{\frac{d\,V}{V}} = -\frac{d}{\frac{d\,V}{V}}F(V, Q) - \frac{d}{\frac{d\,V}{V}}f(V) \quad . \quad . \quad . \quad (1\,A.)$$

where $\mathfrak{d}$ denotes the cubic compressibility.

The latter portion of this coefficient, $-\frac{d}{\frac{d\,V}{V}}f(V)$, consists of two parts, one of which is capable of being resolved into forces, acting along the lines joining the atomic centres, and gives rise to rigidity, or elasticity of figure, as well to elasticity of volume, while the other, which is not capable of being so resolved, gives rise to elasticity of volume only. The ratio of each of those parts to their sum must be a function of the heat, the former part being greater, and the latter less, as the atomic atmosphere is more concentrated round the nucleus; that is to say,

as the heat is less; but their sum, so far as elasticity of volume is concerned, is a function of the density only.

That is to say, as in equation (12) of my paper on the laws of the elasticity of solids (*Cambridge and Dublin Mathematical Journal*, February 1851), let the total coefficient of elasticity of volume be denoted thus

$$\frac{1}{\mathfrak{d}} = \mathrm{J} + \phi\,(\mathrm{C}_1, \mathrm{C}_2, \mathrm{C}_3) \quad . \quad . \quad . \quad . \quad . \quad (1\,\mathrm{B.})$$

C_1, C_2, C_3, being the coefficients of rigidity round the three axes of elasticity, and J a coefficient of fluid elasticity; then

$$\left.\begin{aligned} \mathrm{J} &= -\frac{d}{\frac{d\,\mathrm{V}}{\mathrm{V}}}\mathrm{F}\,(\mathrm{V}, \mathrm{Q}) - \psi\,(\mathrm{V}, \mathrm{Q}) \cdot \frac{d}{\frac{d\,\mathrm{V}}{\mathrm{V}}} f\,(\mathrm{V}) \quad . \quad . \\ \phi\,(\mathrm{C}_1, \mathrm{C}_2, \mathrm{C}_3) &= -\left(1 - \psi\,(\mathrm{V}, \mathrm{Q})\right) \cdot \frac{d}{\frac{d\,\mathrm{V}}{\mathrm{V}}} f\,(\mathrm{V}) \quad . \quad . \end{aligned}\right\} \quad (1\,\mathrm{C.})$$

For the present, we have to take into consideration that portion only of the expansive pressure which depends on density and heat jointly, and is the means of mutually converting heat and expansive power; that is to say, the pressure at the boundaries of the atomic atmospheres; which I shall denote by

$$p = \mathrm{F}\,(\mathrm{V}, \mathrm{Q})$$

Pressures, throughout this paper, are supposed to be measured by units of weight upon unity of area; densities, by the weight of unity of volume.

(3.) *Determination of the External Pressure of an Atomic Atmosphere.*—Let a body be composed of equal and similar atomic nuclei, arranged in any symmetrical manner, and enveloped by an atmosphere, the parts of which are subject to attractive and repulsive forces, exercised by each other, and by the nuclei. Let it further be supposed, that this atmosphere, at each point, has an elastic pressure proportional to the density at that point, multiplied by a specific coefficient depending on the nature of the substance, which I shall denote by h. (This coefficient was denoted by b in previous papers).

Let ϱ and p' denote the density and pressure of the atomic atmosphere at any point; then

$$p' = h\,\varrho$$

$$\text{Let} -g\frac{d\,\Phi}{d\,x}, -g\frac{d\,\Phi}{d\,y}, -g\frac{d\,\Phi}{d\,z}$$

be the accelerative forces operating on a particle of atomic atmosphere, in virtue of the molecular attractions and repulsions, which I have made explicitly negative, attractions being supposed to predominate. The property of the surfaces called the boundaries of the atoms is this

$$\left(\frac{d\,\Phi}{d\,x}\right)_1 = 0, \quad \left(\frac{d\,\Phi}{d\,y}\right)_1 = 0, \quad \left(\frac{d\,\Phi}{d\,z}\right)_1 = 0,$$

The suffix $_1$ being used to distinguish the value of quantities at those surfaces. Hence Φ_1 is a maximum or minimum. Those surfaces are symmetrical in form round each nucleus, and equidistant between pairs of adjacent nuclei. Their equation is

$$\Phi - \Phi_1 = 0.$$

Let M denote the total weight of an atom; μ that of its atmospheric part, and $M - \mu$ that of its nucleus; then

M V is the volume of the atom,—

$\frac{\mu}{M V}$ the mean density of the atmospheric part, measured by weight, the nucleus being supposed to be of insensible magnitude;—

and we have the following equations

$$\left.\begin{aligned} M V &= \iiint_{(1)} dx\,dy\,dz \quad . \quad . \quad . \quad . \quad . \\ \mu &= \frac{\mu}{M V} \iiint_{(1)} dx\,dy\,dz = \iiint_{(1)} \rho\, dx\,dy\,dz \; . \end{aligned}\right\} \qquad (2.)$$

The suffix $(_1)$ denoting that the integration is to be extended to all points within the surface

$$(\Phi - \Phi_1 = 0).$$

According to the hypothesis now under consideration, *Heat* consists in a revolving motion of the particles of the atomic atmosphere, communicated to them by the nuclei. Let v be the common mean velocity possessed by the nucleus of an atom and the atmospheric particles, when the distribution of this motion has been equalised. I use the term *mean* velocity to denote, that the velocity of each particle may undergo small periodic changes, which it is unnecessary to consider in this investigation.

Then the quantity of heat in unity of weight is

$$Q = \frac{v^2}{2g}$$

being equal to the mechanical power of unity of weight falling through the height $\frac{v^2}{2g}$. The quantity of heat in one atom is of course M Q, and in the atmospheric part of an atom, μ Q.

I shall leave the form of the paths described by the atmospheric particles indeterminate, except that they must be closed curves of permanent figure, and included within the surface $(\Phi - \Phi_1 = 0)$. Let the nucleus be taken as the origin of co-ordinates, and let α, β, γ, be the direction-cosines of the motion of the particles at any point (x, y, z). Then the equations of a permanent condition of motion at that point, are

$$\left.\begin{aligned} -\frac{1}{\varrho}\cdot\frac{dp'}{dx}-\frac{d\Phi}{dx}-2Q\left(\alpha\frac{d}{dx}+\beta\frac{d}{dy}+\gamma\frac{d}{dz}\right)\alpha=0 \quad . \\ -\frac{1}{\varrho}\cdot\frac{dp'}{dy}-\frac{d\Phi}{dy}-2Q\left(\alpha\frac{d}{dx}+\beta\frac{d}{dy}+\gamma\frac{d}{dz}\right)\beta=0 \quad . \\ -\frac{1}{\varrho}\cdot\frac{dp'}{dz}-\frac{d\Phi}{d}-2Q\left(\alpha\frac{d}{dx}+\beta\frac{d}{dy}+\gamma\frac{d}{dz}\right)\gamma=0 \quad . \end{aligned}\right\} \quad (3.)$$

Let r be the radius of curvature of the path of the particles through (x, y, z); and $\alpha'\ \beta'\ \gamma'$, its direction-cosines; then the above equations obviously become

$$\left.\begin{aligned} -\frac{1}{\varrho}\frac{dp'}{dx}-\frac{d\Phi}{dx}-2Q\frac{\alpha'}{r}=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \\ -\frac{1}{\varrho}\frac{dp'}{dy}-\frac{d\Phi}{dy}-2Q\frac{\beta'}{r}=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \\ -\frac{1}{\varrho}\frac{dp'}{dz}-\frac{d\Phi}{dz}-2Q\frac{\gamma'}{r}=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \end{aligned}\right\} \quad (3\,A.)$$

If these equations are integrable,

$$\frac{\alpha'}{r}dx+\frac{\beta'}{r}dy+\frac{\gamma'}{r}dz$$

must be an exact differential. Let $-\phi$ be its primitive function; the negative sign being used, because α', β', γ' must be generally negative. Then the integral of the equations (3) is

$$\log_e \varrho=\frac{1}{h}\int\frac{dp'}{\varrho}=\frac{1}{h}(2Q\phi-\Phi)+\text{constant};$$

or taking ϱ_1 to denote the pressure at the bounding surface of the atom :—

$$\varrho=\varrho_1 e^{\frac{2Q}{h}(\phi-\phi_1)-\frac{1}{h}(\Phi-\Phi_1)} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4).$$

Our present object is to determine the superficial-atomic density, ϱ_1, and thence the pressure $p=h\varrho_1$, in terms of the mean density $\frac{1}{V}$ and heat Q. For this purpose we must introduce the above value of ϱ into equation (2), giving

$$\mu=\varrho_1\cdot\iiint_{(1)} e^{\frac{2Q}{h}(\phi-\phi_1)-\frac{1}{h}(\Phi-\Phi_1)}dx\,dy\,dz$$

whence

$$p=h\varrho_1=h\mu\div\iiint_{(1)} e^{\frac{2Q}{h}(\phi-\phi_1)-\frac{1}{h}(\Phi-\Phi_1)}\,dx\,dy\,dz \quad . \quad . \quad (5.)$$

Let the volume of the atom be conceived to be divided into layers, in each of which ϕ has a constant value. Then we may make the following transformations.

$$\left.\begin{aligned} &\iiint dx\,dy\,dz = k\,\mathrm{M}\,\mathrm{V}\int e^{k(\phi-\phi_1)}\,\frac{\psi}{\psi_1}\,d\phi \quad . \quad . \quad . \quad . \\ &\iiint e^{-\frac{1}{k}(\Phi-\Phi_1)}\,dx\,dy\,dz = k\,\mathrm{M}\,\mathrm{V}\int e^{k(\phi-\phi_1)}\,\frac{\omega}{\omega^1}\,d\phi \quad . \end{aligned}\right\} \quad (6.)$$

k being a specific constant, and ψ and ω functions of ϕ, and of the nature and density of the substance.

The lower limit of integration of ϕ must be made $-\infty$, that it may include orbits of indefinitely small magnitude described round the atomic centre.

The nature of the function ψ is limited by the following condition,

$$1 = k\int_{-\infty}^{\phi_1} e^{k(\phi-\phi_1)}\,\frac{\psi}{\psi_1}\,d\phi \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7.)$$

$$\text{Let } \frac{2\,\mathrm{Q}}{h\,k} + 1 = \theta$$

Then these transformations give the following result for the pressure at the bounding surface of an atom:—

$$\left.\begin{aligned} p = h\,\varrho_1 &= \frac{h\,\mu}{\mathrm{M}\,\mathrm{V}} \div \int_{-\infty}^{k\,\phi_1} e^{\theta\,k(\phi-\phi_1)}\,\frac{\omega}{\omega_1}\,k\,d\phi \quad . \quad . \quad . \\ &= \frac{h\,\mu}{\mathrm{M}\,\mathrm{V}} \cdot \frac{\theta\,\omega_1}{\omega_1 - \frac{\omega'_1}{\theta} + \frac{\omega''_1}{\theta^2} - \frac{\omega'''_1}{\theta^3} + \text{\&c.}} \quad . \quad . \quad . \end{aligned}\right\} \quad (8.)$$

ω'_1, &c. being the successive differential coefficients of ω with respect to $k\,\phi$, when $\phi = \phi_1$.

(4.) The following transformation will be found useful in the sequel.

Let λ be the indefinite value of $\log_e \mathrm{V}$, and λ_1 its actual value in the case under consideration. Let G be the same function of λ which ω is of $k\,\phi$, and let G′, G″, &c. be its successive differential coefficients with respect to λ.

Let

$$\mathrm{H}_1 = \int_{-\infty}^{\lambda_1} e^{\theta(\lambda-\lambda_1)}\,\mathrm{G}\,d\lambda = \frac{\mathrm{G}_1}{\theta} - \frac{\mathrm{G}'_1}{\theta^2} + \frac{\mathrm{G}''_1}{\theta^3} - \text{\&c.}$$

Then

$$p = \frac{h\,\mu\,\mathrm{G}_1}{\mathrm{M}\,\mathrm{V}\,\mathrm{H}_1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9.)$$

The function H has the following properties, which will be afterwards referred to:—

$$\left.\begin{aligned} &\frac{d\,\mathrm{H}_1}{d\,\lambda_1} + \theta\,\mathrm{H}_1 - \mathrm{G}_1 = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \\ &\int_{-\infty}^{\lambda_1} \mathrm{H}\,d\lambda = -\frac{d\,\mathrm{H}_1}{d\,\theta} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \end{aligned}\right\} \quad (10.)$$

(5.) *Case of a Perfect Gas.*—As a substance is rarefied, it gradually approaches a condition in which the pressure, under like circumstances as to heat, varies proportionally to the density. This is because the effect of the molecular attractions and repulsions on the pressure diminishes with the density, so that Φ, ω, and G approximate to constant quantities. In the limiting or perfectly gaseous condition, therefore,

$$H_1 = \frac{G_1}{\theta}$$

and

$$p = \frac{h\mu\theta}{M V} = \frac{h\mu}{M V}\left(\frac{2Q}{hk} + 1\right) \quad . \quad . \quad . \quad . \quad . \quad (11.)$$

(6.) *Equilibrium of Heat: Nature of Temperature and Real Specific Heat.*—When the atmospheres of atoms of two different substances are in contact at their common bounding surface, it is necessary to a permanent condition, that the pressure in passing that surface should vary continuously.

Let (a) and (b) be taken as characteristics, to distinguish the specific quantities peculiar to the two media respectively. Let dm denote the volume of an indefinitely thin layer, close to the bounding surface. Then the following equations must be fulfilled, to ensure a permanent condition:—

$$p(a) = p(b)\ ;\ \frac{dp'}{dm}(a) = \frac{dp'}{dm}(b) \text{ when } p' = p \quad . \quad . \quad . \quad . \quad (12.)$$

By making the proper substitutions in equation (4), it appears, that

$$p' = p\, e^{k\theta(\phi - \phi_1)}\, \frac{\psi_1}{\omega_1} \cdot \frac{\omega}{\psi}$$

Hence

$$\frac{dp'}{dm}(p' = p) = p\left(\theta \frac{d(k\phi)}{dm} + \frac{d \cdot \frac{\omega}{\psi}}{dm}\right)$$

Now p is the same for both media: $\frac{\psi_1}{\omega_1} \cdot \frac{\omega}{\psi} = e^{-\frac{1}{h}(\Phi - \Phi_1)}$ is either a maximum or a minimum, so that its differential is null; and dm is a continuous function of $k\phi$, so that $\frac{d(k\phi)}{dm}(a) = \frac{d(k\phi)}{dm}(b)$. There remains only the function of heat

$$\theta = \frac{2Q}{hk} + 1.$$

Therefore the condition of a permanent state of molecular motion, that is to say, the condition of *equilibrium of heat*, is that this function shall be the same for the two substances; or that

$$\frac{2Q_a}{h_a k_a} = \frac{2Q_b}{h_b k_b} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13.)$$

Hence, *temperature* depends on the above function only; for the definition of temperature is, that bodies at the same temperature are in a permanent condition as to heat, so far as their mutual action is concerned.

The ratio of the *real specific heat* of (a) to that of (b) is obviously

$$h_a k_a : h_b k_b \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14.)$$

(7.) *Measure of Temperature and Specific Heat.*—The function θ is proportional to the pressure of a perfect gas at a constant density. That pressure, therefore, is the most convenient *measure* of temperature.

Let τ denote *absolute* temperature, as measured by the pressure of a perfect gas at constant density, and reckoned from a certain *absolute zero*, 274°·6 Centigrade, or 494°·28 Fahrenheit below the temperature of melting ice. Let κ be a constant which depends on the length of a degree on the thermometric scale, and is the same for all substances in nature.

Then

$$\left.\begin{aligned} \tau = \kappa\,\theta = \frac{2\,\kappa\,Q}{h\,k} + \kappa \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \\ Q = (\tau - \kappa)\,\frac{h\,k}{2\,\kappa} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \end{aligned}\right\} \quad (15.)$$

and the real specific heat of the substance, that is to say, the depth of fall, under the influence of gravity, which is equivalent to a rise of one degree of temperature in the body, is represented by

$$k = \frac{h\,k}{2\,\kappa} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16.)$$

The pressure of a perfect gas is represented in terms of temperature by

$$p = \frac{h\,\mu\,\tau}{M\,V\,\kappa} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17.)$$

It may also be expressed thus: let τ_0 denote the absolute temperature of melting ice in degrees of the scale employed, and V_0 the volume of unity of weight of the substance in the theoretical state of perfect gas, at the temperature of melting ice and pressure unity:—then

$$p = \frac{V_0}{V}\,\frac{\tau}{\tau_0} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18.)$$

On comparing this with equation (17) we see that

$$\left.\begin{aligned} \frac{h\,\mu}{\kappa\,M} = \frac{V_0}{\tau_0}\,; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \\ \kappa = \frac{h\,\mu\,\tau_0}{M\,V_0}\,;\ \frac{h\,\mu}{M\,V_0} = \frac{\kappa}{\tau_0} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \end{aligned}\right\} \quad (19.)$$

Now h is the specific elasticity of the atomic atmosphere of the substance; $\frac{\mu}{M\,V_0}$ is the mean specific gravity of that atmosphere, when the body is in the theoretical state of perfect gas; and κ and τ_0 are the same for all substances in nature. There-

force, *for every substance in nature, the mean specific gravity of the atomic atmosphere in the theoretical state of perfect gas is inversely proportional to the specific elasticity of that atmosphere.*

Real specific heat may also be thus expressed:—

$$\mathfrak{k} = \frac{V_0}{\tau_0} \cdot \frac{k\,M}{2\,\mu} \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20.)$$

in which $\frac{V_0}{\tau_0}$ corresponds to $\frac{1}{C\,n\,M}$ in my former papers, and $\frac{k\,M}{2\,\mu}$ to $\frac{3\,k\,M}{2\,\mu}$ or $\frac{1}{N}$.

The latter factor appears to depend on the chemical constitution of the substance, being the same for all simple gases.

(8.) *Total Pressure of Substances in general, expressed in terms of temperature.*

In equation (9) let $\frac{\tau}{\kappa}$ be put for θ: then

$$P = p + f(V) = f(V) + \frac{h\,\mu}{\kappa\,M}\frac{\tau}{V}G_1 \div \left\{ G_1 - \frac{\kappa\,G_1'}{\tau} + \frac{\kappa^2\,G_1''}{\tau^2} - \&c. \right\}$$

$$= f(V) + \frac{V_0}{\tau_0}\frac{\tau}{V}\left\{ 1 - \frac{A_1}{\tau} - \frac{A_2}{\tau^2} - \frac{A^3}{\tau^3} - \&c. \right\} \qquad . \quad . \quad (21.)$$

where

$$A_1 = \frac{-\kappa\,G'}{G_1}\,;\quad A_2 = -\frac{\kappa^2}{G_1^{\,2}}(G_1'^2 - G_1'')$$

$$A_3 = -\frac{\kappa^3}{G_1^{\,3}}(G_1'^3 - 2\,G_1'\,G_1'' + G_1''')\,;\ \&c.$$

This formula is identical with that which I employed in my former paper, to represent the pressure of an imperfect gas, and which I found to agree with M. Regnault's experiments, when the coefficients A and the function f (V) had been calculated empirically.

Section Second.—*Relations between Heat and Expansive Power.*

(9.) *Variations of Sensible and Latent Heat: Fundamental Equation of the Theory.*—If the forms, positions, and magnitudes of the paths described by the revolving particles of the atomic atmospheres be changed, whether by a variation of mean density, or by a variation of temperature, an increase or diminution of the vis-viva of their motion, that is to say, of the heat of the body, will take place in virtue of that change of the paths of motion; an increase when they are contracted, and a diminution when they are dilated.

Let δ . Q represent, when positive, the indefinitely small quantity of heat which must be communicated to unity of weight of a substance, and when negative, that which must be abstracted from it, in order to produce the indefinitely small variation of temperature $\delta\,\tau$ simultaneously with the indefinitely small variation

of volume δ V. Let δ . Q be divided into two parts

$$\delta Q + \delta Q' = \delta . Q$$

of which δ Q, being directly employed in varying the *velocity* of the particles, is the variation of the *actual* or *sensible* heat possessed by the body; while δ Q', being employed in varying their *orbits*, represents the amount of the mutual transformation of heat with expansive power and molecular action, or the variation of what is called the *latent* heat; that is to say, of a molecular condition constituting a source of power, out of which heat may be developed. (δ Q' in this paper corresponds to $-\delta$ Q' in my former papers.)

The variation of sensible heat has evidently this value

$$\delta Q = k \delta \tau \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22.)$$

Let δx, δy, δz, be the displacements of the orbit of the particles of atomic atmosphere at the point $(x, y, z.)$ A molecule $\varrho\, dx\, dy\, dz$ is acted upon by the accelerative forces (see equation 3 A.)

$$-2Q\frac{d\phi}{dx}\,;\ -2Q\frac{d\phi}{dy}\,;\ -2Q\frac{d\phi}{dz}\,;$$

parallel to the three axes respectively.

The sum of the actions of those forces on the molecule $\varrho\, dx\, dy\, dz$ during the change of temperature and volume, is

$$-2Q\left(\frac{d\phi}{dx}\delta x + \frac{d\phi}{dy}\delta y + \frac{d\phi}{dz}\delta z\right)\varrho\, dx\, dy\, dz$$
$$= -2Q\,\delta\phi\,\varrho\, dx\, dy\, dz$$

The sum of such actions upon all the particles in unity of weight is equal in amount and opposite in sign to the variation of latent heat: that is to say,

$$\delta Q' = \frac{2Q}{M}\iiint_{(1)} \varrho\,\delta\phi\, dx\, dy\, dz \quad . \quad . \quad . \quad . \quad . \quad (23.)$$

To determine the value of the variation $\delta\phi$, let it be divided into two parts, thus:—

$$\delta\phi = \delta\phi_1 + \delta\Delta\phi$$

where $\Delta\phi = \phi - \phi_1$

First, With respect to $\delta\phi_1$, it is obvious that because, according to equations (6, 7)

$$M V = k M V \int_{-\infty}^{\phi^1} e^{k\Delta\varphi}\frac{\psi}{\psi_1}\, d\phi$$

we must have

$$\delta V = k V\,\delta\phi_1 \text{ and } \delta\phi_1 = \frac{\delta V}{kV}$$

Hence the first part of the integral (23) is

$$\frac{2Q}{M}\delta\phi_1\iiint_{(1)}\varrho\, dx\, dy\, dz = \frac{2\mu Q}{MkV}\cdot\delta V$$

$$= \frac{h\mu}{\kappa M}(\tau - \kappa)\frac{\delta V}{V} \quad . \quad . \quad . \quad . \quad . \quad . \quad (23\,A.)$$

To determine the second part of the integral we have the condition, that the quantity of atomic atmosphere enclosed within each surface at which $\Delta\phi$ has some given value, is invariable; that is to say

$$\left(\delta\Delta\phi\frac{d}{d\Delta\phi} + \delta V\frac{d}{dV} + \delta\tau\frac{d}{d\tau}\right)\left(k\rho_1 M V\int_{-\infty}^{\phi} e^{k\theta\Delta\phi}\frac{\omega}{\omega_1}d\phi\right) = 0$$

Hence

$$\delta\Delta\phi = \frac{-\left(\delta V\frac{d}{dV} + \delta\tau\frac{d}{d\tau}\right)\left(k\rho_1 M V\int_{-\infty}^{\phi} e^{k\theta\Delta\phi}\frac{\omega}{\omega_1}d\phi\right)}{k\rho_1 M V e^{k\theta\Delta\phi}\frac{\omega}{\omega_1}}$$

The value of the second part of the integral (23) is now found to be:—

$$\frac{2Q}{M}\iiint_{(1)}\rho\,\delta\Delta\phi\,dx\,dy\,dz = \frac{2Q}{M}k\rho_1 M V\int_{-\infty}^{\phi_1} e^{k\theta\Delta\phi}\frac{\omega}{\omega_1}\delta\Delta\phi\,d\phi$$

$$= -\frac{2Q}{kM}\left(\delta V\frac{d}{dV} + \delta\tau\frac{d}{d\tau}\right)\left\{\rho_1 M V\int_{-\infty}^{k\phi_1}\int_{-\infty}^{k\phi} e^{k\theta\Delta\phi}\frac{\omega}{\omega_1}k^2\,d\phi^2\right\}$$

In the double integral, let $\lambda = \log_e V$ be put for $k\phi$, G for ω, and H for the single integral, as in equation (9.) Then the double integral becomes

$$\frac{1}{G_1}\int_{-\infty}^{\lambda_1} H\,d\lambda = -\frac{1}{G_1}\frac{dH_1}{d\theta} \text{ by Eq. (10)}$$

$$= -\frac{\kappa}{G_1}\frac{dH_1}{d\tau}$$

Also because $\rho_1 M V = \frac{\mu G_1}{H_1}$ by eq. (9), and $\frac{2Q}{kM} = \frac{h}{\kappa}(\tau - \kappa)$, the second part of the integral (23) is found to be

$$\frac{h\mu}{M}(\tau - \kappa)\left(\delta\tau\frac{d}{d\tau} + \delta V\frac{d}{dV}\right)\frac{dH_1}{H_1\,d\tau} \quad . \quad . \quad . \quad . \quad (23\,B.)$$

Hence, adding together (23 A.) and (23 B.) we find for the total variation of latent heat

$$\delta Q = \frac{h\mu}{M}(\tau - \kappa)\left\{\delta\tau\cdot\frac{d^2\log_e H_1}{d\tau^2} + \delta V\cdot\left(\frac{1}{\kappa V} + \frac{d^2\log_e H_1}{d\tau\,dV}\right)\right\} \quad . \quad (24.)$$

To express this in terms of quantities which may be known directly by experiment, we have by equations 10 and 9:—

$$\frac{dH_1}{H_1\,d\lambda} + \theta - \frac{G_1}{H_1} = 0, \text{ that is to say,}$$

$$\frac{d\log_e H_1}{dV} = \frac{G_1}{H_1 V} - \frac{\tau}{\kappa V} = \frac{M}{h\mu}p - \frac{\tau}{\kappa V}$$

and, therefore,

$$\mathrm{Log.}_e \mathrm{H}_1 = \frac{\mathrm{M}}{h\mu}\int p\, d\mathrm{V} - \frac{\tau}{\kappa}\log_e \mathrm{V} + f(\tau) + \text{constant.}$$

$f(\tau)$ is easily found to be $= -\log_e \tau$ for a perfect gas, and being independent of the density, is the same for all substances in all conditions; Hence we find (the integrals being so taken that for a perfect gas they shall = 0)

$$* \quad \frac{d\log_e \mathrm{H}_1}{d\tau} = \int\left(\frac{\mathrm{M}}{h\mu}\frac{dp}{d\tau} - \frac{1}{\kappa \mathrm{V}}\right) d\mathrm{V} - \frac{1}{\tau}$$

$$\frac{d^2 \log_e \mathrm{H}_1}{d\tau^2} = \frac{\mathrm{M}}{h\mu}\int \frac{d^2 p}{d\tau^2}\, d\mathrm{V} + \frac{1}{\tau^2}$$

$$\frac{d^2 \log_e \mathrm{H}_1}{d\tau\, d\mathrm{V}} = \frac{\mathrm{M}}{h\mu}\frac{dp}{d\tau} - \frac{1}{\kappa \mathrm{V}}$$

and, therefore,

$$\delta Q = (\tau - \kappa)\left\{\delta\tau \cdot \left(\frac{h\mu}{\mathrm{M}\tau^2} + \int \frac{d^2 p}{d\tau^2} d\mathrm{V}\right) + \delta \mathrm{V} \cdot \frac{dp}{d\tau}\right\} \quad . \quad . \quad (25.)$$

is the variation of latent heat, expressed in terms of the pressure, volume, and temperature; to which if the variation of sensible heat, $\delta Q = k\, \delta\tau$, be added, the complete variation of heat, $\delta Q + \delta Q = \delta . Q$, in unity of weight of the substance, corresponding to the variations $\delta \mathrm{V}$ and $\delta\tau$ of volume and temperature, will be ascertained.

It is obvious that equation (25), with its consequences, is applicable to any mixture of atoms of different substances in equilibrio of pressure and temperature; for in that case τ, $\frac{dp}{d\tau}$, and $\frac{d^2 p}{d\tau^2}$ are the same for each substance. We have only to substitute for $\frac{h\mu}{\mathrm{M}}$ the following expression:—

$$n_1 \frac{h_1 \mu_1}{\mathrm{M}_1} + n_2 \frac{h_2 \mu_2}{\mathrm{M}_2} + \&c.$$

where n_1, n_2, &c., are the proportions of the different ingredients in unity of weight of the mixture, so that $n_1 + n_2 + \&c. = 1$.

Equation (25) agrees exactly with equation (6) in the first section of my original paper on the Theory of the Mechanical Action of Heat. It is the fundamental equation of that theory; and I shall now proceed to deduce the more important consequences from it.

(10.) *Equivalence of Heat and Expansive Power.* JOULE'S *Law.*—From the variation of the heat communicated to the body, let us subtract the variation of the expansive power given out by it, or

$$\mathrm{P}\, \delta \mathrm{V} = \{p + f(\mathrm{V})\}\, \delta \mathrm{V}$$

The result is the variation of the total power exercised upon or communicated to

* This coefficient corresponds to $-\frac{\mathrm{U}}{\kappa}$ in the notation of my previous paper on the Mechanical Action of Heat.

unity of weight of the substance, supposing that there is no chemical, electrical, magnetic, or other action except heat and pressure; and its value is:—

$$\delta \Psi = \delta Q + \delta Q' - P\,\delta V = \delta\tau \,.\, \left\{ \mathfrak{k} + \frac{h\mu}{M}\left(\frac{1}{\tau} - \frac{\kappa}{\tau^2}\right) + (\tau-\kappa)\int \frac{d^2 p}{d\tau^2} dV \right\}$$
$$+ \delta V \,.\, \left\{ (\tau-\kappa)\frac{dp}{d\tau} - p - f(V) \right\} \quad . \quad . \quad . \quad (26.)$$

This expression is obviously *an exact differential*, and its integral is the following function of the volume and temperature:—

$$\Psi = \mathfrak{k}(\tau-\kappa) + \frac{h\mu}{M}\left(\log_e \tau + \frac{\kappa}{\tau}\right) + \int (\tau-\kappa)\frac{dp}{d\tau} dV - \int f(V)\, dV \quad . \quad (27.)$$

Accordingly, the total amount of power which must be exercised upon unity of weight of a substance, to make it pass from the absolute temperature τ_0 and volume V_0 to the absolute temperature τ_1 and volume V_1, is

$$\Psi(V_1, \tau_1) - \Psi(V_0, \tau_0)$$

This quantity consists partly of expansive or compressive power, and partly of heat, in proportions depending on the mode in which the intermediate changes of temperature and volume take place; but the total amount is independent of these changes.

Hence, *if a body be made to pass through a variety of changes of temperature and volume, and at length be brought back to its primitive volume and temperature, the algebraical sum of the portions of power applied to and evolved from the body, whether in the form of expansion and compression, or in that of heat, is equal to zero.*

This is one form of the law proved experimentally by Mr Joule, of the equivalence of heat and mechanical power. In my original paper on the Mechanical Action of Heat, I used this law as an axiom, to assist in the investigation of the Equation of Latent Heat. I have now deduced it from the hypothesis on which my researches are based;—not in order to prove the law, but to verify the correctness of the mode of investigation which I have followed.

Equations (26) and (27), like equation (23), are made applicable to unity of weight of a mixture, by putting $\Sigma n \mathfrak{k}$ for $\mathfrak{k}$, and $\Sigma n \frac{h\mu}{M}$ for $\frac{h\mu}{M}$.

The train of reasoning in this article is the converse of that followed by Professor William Thomson of Glasgow, in article 20 of his paper on the Dynamical Theory of Heat, where he proves from Joule's law, that the quantity corresponding to $\delta\Psi$ is an exact differential.

(11.) *Mutual Conversion of Heat and Expansive Power.* Carnot's *Law of the Action of Expansive Machines.*—If a body be made to pass from the volume V_0 and absolute temperature τ_0 to the volume V_1 and absolute temperature τ_1, and be then brought back to the original volume and temperature, the total power exerted

(Ψ) will have, in those two operations, equal arithmetical values, of opposite signs. Each of the quantities Ψ consists partly of heat and partly of expansive power, the proportion depending on the mode of intermediate variation of the volume and temperature, which is arbitrary. If the mode of variation be different in the two operations, the effect of the double operation will be to transform a portion of heat into expansive power, or *vice versâ*.

Let (a) denote the first operation: (b) the *reverse* of the second. Then

$$\Psi_b = \Psi_a$$

The terms of Ψ which involve functions of τ only, or of V only, are not affected by the mode of intermediate variation of those quantities. The term on which the mutual conversion of heat and expansive power depends, is therefore

$$\int (\tau - \kappa) \frac{dp}{d\tau} d\mathrm{V}\,(b) = \int (\tau - \kappa) \frac{dp}{d\tau} d\mathrm{V}\,(a)$$

or,

$$\int \left(\frac{dQ'}{d\mathrm{V}} - p \right) d\mathrm{V}\,(b) = \int \left(\frac{dQ'}{d\mathrm{V}} - p \right) d\mathrm{V}\,(a)$$

Hence,

$$\int \frac{dQ'}{d\mathrm{V}} d\mathrm{V}\,(a) - \int \frac{dQ'}{d\mathrm{V}} d\mathrm{V}\,(b) = \int p\, d\mathrm{V}\,(a) - \int p\, d\mathrm{V}\,(b)$$

which last quantity is the amount of the *heat transformed into expansive power*, or the total latent heat of expansion in the *double operation*.

Let

$$\int \frac{dp}{d\tau} d\mathrm{V} = \int \frac{1}{\tau - \kappa} \cdot \frac{dQ'}{d\mathrm{V}} d\mathrm{V} = \mathrm{F}$$

Then because

$$\frac{dQ'}{d\mathrm{V}} d\mathrm{V} = (\tau - \kappa)\, d\mathrm{F}$$

we have

$$\int_{\mathrm{V}_0}^{\mathrm{V}_1} p\, d\mathrm{V}\,(a) - \int_{\mathrm{V}_0}^{\mathrm{V}_1} p\, d\mathrm{V}\,(b) = \int_{\mathrm{F}_0}^{\mathrm{F}_1} (\tau - \kappa)\, d\mathrm{F}\,(a) - \int_{\mathrm{F}_0}^{\mathrm{F}_1} (\tau - \kappa)\, d\mathrm{F}\,(b)$$

$$= \int_{\mathrm{F}_0}^{\mathrm{F}_1} (\tau_a - \tau_b)\, d\mathrm{F} = \int_{\mathrm{V}_0}^{\mathrm{V}_1} \frac{\tau_a - \tau_b}{\tau_a - \kappa} \frac{dQ'_a}{d\mathrm{V}} d\mathrm{V} \quad . \quad . \quad . \quad . \quad (28.)$$

In which τ_a and τ_b are the pair of absolute temperatures, in the two operations respectively, corresponding to *equal values of* F.

This equation gives a relation between the heat transformed into expansive power by a given pair of operations on a body, the latent heat of expansion in the first operation, and the mode of variation of temperature in the two operations. It shews that the proportion of the original latent heat of expansion finally transformed into expansive power, is a function of the temperatures alone, and is therefore independent of the nature of the body employed.

Equation (28) includes CARNOT'S law as a particular case. Let the limits of

variation of temperature and volume be made indefinitely small. Then

$$dp\,d\mathrm{V} = \frac{d\tau}{\tau-\kappa} \cdot \frac{d\mathrm{Q}'}{d\mathrm{V}}\,d\mathrm{V}$$

and dividing by $d\tau\,d\mathrm{V}$

$$\frac{dp}{d\tau} = \frac{1}{\tau-\kappa} \cdot \frac{d\mathrm{Q}'}{d\mathrm{V}}$$

This differential equation is also an immediate consequence of equation (25.) If $\frac{\mu}{\mathrm{J}}$ be put for $\frac{1}{\tau-\kappa}$, and J M for $\frac{d\mathrm{Q}'}{d\mathrm{V}}$ it becomes identical with the equation by which Professor WILLIAM THOMSON expresses CARNOT'S law, as deduced by him and by Mr CLAUSIUS from the principle, that *it is impossible to transfer heat from a colder to a hotter body, without expenditure of mechanical power.*

The investigation which I have now given is identical in principle with that in the fifth section of my paper on the Mechanical Action of Heat; but the result is expressed in a more comprehensive form.

Equation (28) like (25), (26), and (27), is applicable to a mixture, composed of any number of different substances, in any proportions, provided the temperature, the pressure, and the coefficients $\frac{dp}{d\tau}, \frac{d^2p}{d\tau^2}$, are the same throughout the mass.

(12.) *Apparent Specific Heat.*—The general value of apparent specific heat of unity of weight, is

$$\mathrm{K} = \frac{d\mathrm{Q}}{d\tau} + \frac{d\mathrm{Q}'}{d\tau} + \frac{d\mathrm{Q}'}{d\mathrm{V}} \cdot \frac{d\mathrm{V}}{d\tau} = \mathrm{k} + (\tau-\kappa)\left\{\frac{h\mu}{\mathrm{M}\tau^2} + \int \frac{d^2p}{d\tau^2}\,d\mathrm{V} + \frac{d\mathrm{V}}{d\tau} \cdot \frac{dp}{d\tau}\right\} \quad . \quad (29.)$$

agreeing with equation 13 of my previous paper.

The value in each particular case depends on the mode of variation of volume with temperature. Specific heat at constant volume, is

$$\mathrm{K}_v = \mathrm{k} + (\tau-\kappa)\left(\frac{h\mu}{\mathrm{M}\tau^2} + \int \frac{d^2p}{d\tau^2}\,d\mathrm{V}\right) \quad . \quad . \quad . \quad . \quad (30.)$$

When the pressure is constant, we must have

$$\frac{d\mathrm{P}}{d\mathrm{V}}\,d\mathrm{V} + \frac{dp}{d\tau}\,d\tau = 0$$

and, consequently,

$$\frac{d\mathrm{V}}{d\tau} = -\frac{\frac{dp}{d\tau}}{\frac{d\mathrm{P}}{d\mathrm{V}}}$$

therefore specific heat at constant pressure, is

$$\mathrm{K}_p = \mathrm{K}_v + (\tau-\kappa)\frac{\left(\frac{dp}{d\tau}\right)^2}{-\frac{d\mathrm{P}}{d\mathrm{V}}} \quad . \quad . \quad . \quad . \quad . \quad (31.)$$

This agrees with equation (16) of Professor THOMSON'S paper, if J μ in his notation $= \tau - \kappa$.

If the body be a perfect gas, then

$$\left.\begin{aligned} \mathrm{K} &= \frac{\mathrm{V}_0}{\tau_0}\left(\frac{k_1^2\mathrm{M}}{2\mu}+\frac{\kappa_4}{\tau^3}-\frac{\kappa^2}{\tau^2}+\frac{\tau-\kappa}{\mathrm{V}}\cdot\frac{d\,\mathrm{V}}{d\,\tau}\right) \\ \mathrm{K}_v &= \frac{\mathrm{V}_0}{\tau_0}\left(\frac{k\,\mathrm{M}}{2\mu}+\frac{\kappa}{\tau}-\frac{\kappa^2}{\tau^2}\right) \\ \mathrm{K}_p &= \mathrm{K}_v+\frac{\mathrm{V}_0}{\tau_0}\left(1-\frac{\kappa}{\tau}\right)=\frac{\mathrm{V}_0}{\tau_0}\left(\frac{k\,\mathrm{M}}{2\mu}+1-\frac{\kappa^2}{\tau^2}\right) \end{aligned}\right\} \quad (32.)$$

The fact that the specific heats of all simple gases for unity of weight are inversely proportional to their specific gravities, shews that $\frac{k\,\mathrm{M}}{2\mu}$ is the same for them all.

(13.) *Velocity of Sound in Fluids.*—Let a denote the velocity of sound in a fluid, and $d\,.\,\mathrm{P}$ the total differential of the pressure. Then

$$a=\sqrt{\left(g\cdot\frac{d\cdot\mathrm{P}}{d\cdot\frac{1}{\mathrm{V}}}\right)}=\sqrt{\left\{g\,\mathrm{V}^2\left(-\frac{d\,\mathrm{P}}{d\,\mathrm{V}}-\frac{d\,\mathrm{P}}{d\,\tau}\cdot\frac{d\,\tau}{d\,\mathrm{V}}\right)\right\}} \quad . \quad . \quad (33.)$$

If it were possible to maintain the temperature of each particle of the fluid invariable during the passage of sound, this velocity would be simply

$$\sqrt{\left(g\cdot\frac{d\,\mathrm{P}}{d\cdot\frac{1}{\mathrm{V}}}\right)}$$

But we have reason to believe, that there is not time, during the passage of sound, for an appreciable transfer of heat from atom to atom; so that for each particle

$$d\,\mathrm{Q}+d\,\mathrm{Q}'=0\,;\ \text{or, } \mathrm{K}=0 \text{ in equation (29).}$$

To fulfil this condition, we must have

$$\frac{d\,\tau}{d\,\mathrm{V}}=-\frac{\tau-\kappa}{\mathrm{K}_v}\cdot\frac{d\,p}{d\,\tau}$$

Consequently,

$$a=\sqrt{\left\{g\,\mathrm{V}^2\left(-\frac{d\,\mathrm{P}}{d\,\mathrm{V}}+\frac{\tau-\kappa}{\mathrm{K}_v}\cdot\left(\frac{d\,p}{d\,\tau}\right)^2\right)\right\}}$$

or, by equation (31)

$$a=\sqrt{\left(g\,\frac{d\,\mathrm{P}}{d\cdot\frac{1}{\mathrm{V}}}\cdot\frac{\mathrm{K}_p}{\mathrm{K}_v}\right)} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (34).$$

That is to say, *the action of heat increases the velocity of sound in a fluid, beyond what it would be, if heat did not act, in the ratio of the square root of the specific heat at constant pressure, to the square root of the specific heat at constant volume.*

This is LAPLACE's law of the propagation of sound; which is here shewn to be applicable, not only to perfect gases, but to all fluids whatsoever.

XXVIII.—*On the Computation of the Specific Heat of Liquid Water at various Temperatures, from the Experiments of M. Regnault.* By William John Macquorn Rankine, Civil Engineer, F.R.S.E., F.R.S.S.A., &c.

(Read December 15, 1851.)

Correction of M. Regnault's Experiments for the Effect of Agitation.

The discovery by Mr Joule of the fact, that mechanical power expended in the agitation of liquids is converted into heat as the visible agitation subsides, renders a certain correction necessary in calculating the results of experiments on specific heat in which such agitation has occurred.

Of this kind are the experiments of M. Regnault on the apparent specific heat of liquid water at different temperatures. Water at a high temperature, T_3, was emitted from a boiler into a calorimeter containing water at a low temperature, T_1, and the resulting intermediate temperature of the whole mass, T_2, was used as the means of calculating the ratio of the mean specific heat of water between T_3 and T_2, to its mean specific heat between T_2 and T_1. Now, the upper part of the boiler contained steam at a high pressure, so that the hot water was expelled with great force. The vis-viva thus communicated to the water, having been converted by fluid friction into heat, ought to be allowed for in computing the results of the experiments,

Let W_1 be the weight of water originally contained in the calorimeter, at the temperature T_1 :

W_3, The weight of water introduced into the calorimeter from the boiler, at the temperature T_3 ;

T_2, the resulting temperature, corrected, as has been done by M. Regnault, for the effect of conduction.

Let $K_{1,2}$ be the mean dynamical specific heat of water between the temperatures T_1 and T_2,—

$K_{2,3}$, its mean dynamical specific heat between T_2 and T_3.

Let P be the pressure of steam of saturation at the temperature T_3,—

ϖ, the pressure of the atmosphere,—

And v, the volume of unity of weight of water at the temperature T_3.

Then the following equation must be fulfilled ;—

$$W_1 K_{1,2} (T_2 - T_1) - W_3 K_{2,3} (T_3 - T_2) - W_3 (P - \varpi) v = 0 :$$

Consequently,

$$\frac{K_{2,3}}{K_{1,2}} = \frac{W_1 (T_2 - T_1)}{W_3 (T_3 - T_2)} - \frac{(P - \varpi) v}{K_{1,2} (T_3 - T_2)} \quad . \quad . \quad . \quad (1.)$$

The first term of this expression corresponds to the formula employed by M. REGNAULT. To correct the results given in his table of experiments, we must, therefore, subtract from each of them the quantity

$$\frac{(P-\varpi)\,v}{K_{1,2}\,(T_3-T_2)}$$

As T_1 and T_2 were always low temperatures, I have treated $K_{1,2}$ as a constant quantity in computing the corrections, its value being

	Number.	Common Logarithm.
In feet per degree of Fahrenheit, ...	772	2·8876173
In feet per centigrade degree, ...	1389·6	3·1428898
In mètres per centigrade degree, ...	423·54	2·6268944

In the following table, the numbers in the first column refer to certain groups of experiments in M. REGNAULT'S table, the mean results of which are given in the succeeding columns.

The correction, which is scarcely appreciable for temperatures near the ordinary boiling point, increases rapidly as the temperature in the boiler rises.

The temperatures are all stated according to the scale of a centigrade air thermometer.

TABLE I.

Reference to M. REGNAULT'S Experiments.	T_1	T_2	T_3	$\frac{K_{2,3}}{K_{1,2}}$ as computed by M. REGNAULT.	Correction to be subtracted.	$\frac{K_{2,3}}{K_{1,2}}$ Corrected.
1, 2, 3.	11°·97	20°·77	107°·79	1·00384	0·00009	1·00375
4, 5, 6, 7.	8·39	17·70	109·29	1·00665	0·00010	1·00655
26, 27, 28, 29.	12·96	26·31	159·74	1·00871	0·00092	1·00779
30, 31, 32, 33.	8·95	23·94	172·69	1·01140	0·00121	1·01019
36, 37, 38.	12·97	28·69	186·51	1·01581	0·00162	1·01419

Empirical Formulæ.

The results of experiment, as thus corrected, agree very nearly with those of the following empirical formula, in which K is the apparent specific heat of liquid water at the temperature T, and K_0, its apparent specific heat at the temperature T_0, which is that of the maximum density of water; viz., 4°·1 centigrade, or 39°·4 Fahr.

a is a constant coefficient, whose value is,

For the centigrade scale, ...	0·000001
For Fahrenheit's scale, ...	0·000000309

$$\frac{K}{K_0} = 1 + a(T - T_0)^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

$$\frac{K_{1,2}}{K_0} = \frac{1}{K_0(T_2 - T_1)} \int_{T_1}^{T_2} K\,dT \quad . \quad . \quad . \quad . \quad (3.)$$

$$= 1 + \frac{a}{3}\left\{(T_2 - T_0)^2 + (T_2 - T_0)(T_1 - T_0) + (T_1 - T_0)^2\right\}$$

The following Table exhibits a comparison between the results of equation (3.) and those of the five groups of experiments already referred to :—

TABLE II.

T_1	T_2	T_3	$\frac{K_{2,3}}{K_{1,2}}$ by Experiment.	$\frac{K_{2,3}}{K_{1,2}}$ by the Empirical Formula.	Difference.
11°·97	20°·77	107°·79	1·00375	1·00409	+0·00034
8·39	17·70	109·29	1·00655	1·00414	−0·00241
12·96	26·31	159·74	1·00779	1·00959	+0·00180
8·95	23·94	172·69	1·01019	1·01055	+0·00036
12·97	28·69	186·51	1·01419	1·01248	−0·00171

A third Table is annexed, which may be found practically useful. It contains the results of the empirical formulæ (2.) and (3.), for every tenth degree of the centigrade scale from 0° to 260°.

The column headed $\frac{K_T}{K_0}$ shews the ratio of the specific heat at T to the specific heat at 0°. That headed $\frac{K_{0,T}}{K_0}$ shews the ratio of the mean specific heat between 0° and T to the specific heat at 0°.

The column headed $\frac{1}{K_0}\int_0^T K\,dT$ shews the ratio of the heat required to raise a given weight of water from 0° to T, to the heat required to raise the same weight of water from the temperature of maximum density to one degree above it.

T Centigr.	$\frac{K_T}{K_0}$	$\frac{K_{0,T}}{K_0}$	$\frac{\int_0^T K\,dT}{K_0}$	T Centigr.	$\frac{K_T}{K_0}$	$\frac{K_{0,T}}{K_0}$	$\frac{\int_0^T K\,dT}{K_0}$
0°	1·0000	1·0000	0°·00	140°	1·0185	1·0060	140°·84
10	1·0000	1·0000	10·00	150	1·0213	1·0069	151·04
20	1·0003	1·0001	20·00	160	1·0243	1·0079	161·26
30	1·0007	1·0002	30·01	170	1·0275	1·0090	171·53
40	1·0013	1·0004	40·02	180	1·0309	1·0101	181·82
50	1·0021	1·0006	50·03	190	1·0346	1·0113	192·15
60	1·0031	1·0010	60·06	200	1·0384	1·0125	202·50
70	1·0043	1·0014	70·10	210	1·0424	1·0139	212·92
80	1·0058	1·0018	80·14	220	1·0466	1·0153	223·36
90	1·0074	1·0023	90·21	230	1·0510	1·0167	233·84
100	1·0092	1·0029	100·29	240	1·0556	1·0182	244·37
110	1·0112	1·0036	110·40	250	1·0605	1·0198	254·95
120	1·0134	1·0043	120·51	260	1·0655	1·0215	265·59
130	1·0159	1·0051	130·66				

XXIX.—*On the Red Prominences seen during Total Eclipses of the Sun.* Part I.

By William Swan, F.R.S.E.

(Read April 5, 1852.)

The red prominences seen during total solar eclipses, are conspicuous rose-coloured objects which appear round the dark edge of the moon, as soon as the last rays of the sun have disappeared. In preparing my account of the total eclipse of the 28th July 1851, it was at first my intention to have stated some hypothetical views which I had formed regarding those remarkable objects, and other appearances I had observed during the total phase of the eclipse. I found, however, that the mere description of phenomena extended to so great a length, as to render such a course inexpedient; and I have since delayed resuming the subject, in order that by comparing a number of other observations with my own, I might be enabled, either to confirm or to modify my views.

The object of the first part of this paper is, To discuss the evidence afforded by the observations of the late eclipse to which I have obtained access, as to the *nature* and *locality* of the red prominences; and, of the second part, To state the views which I have been led to form regarding the cause of those singular objects, and their probable connexion with other solar phenomena.

In inquiring into the nature of the red prominences, I shall examine in succession different opinions, which have either been formally announced, or are likely to be entertained, regarding them, in order to ascertain which of those hypotheses is most accordant with actual observation. The hypotheses I shall discuss are the following, namely, 1*st*, That the prominences are *optical phenomena*, caused by the telescope used in viewing the eclipse,—by the unequally heated state of the earth's atmosphere,—or by the action of the moon's edge on the rays of light; and, 2*d*, That they are *material objects*, existing in the sun or in the moon.

I. *On the Hypothesis that the Red Prominences are Optical Phenomena.*

1. *On the Visibility of the Red Prominences to the Naked Eye.*

1. It may be supposed that the red prominences are optical phenomena caused by the telescope used in viewing the eclipse; but this opinion is at once disproved by the fact, that they are visible to the naked eye. At the late eclipse, although I was unable to distinguish the *forms* of the prominences with the naked eye, I had no difficulty in seeing the position of at least one of them, by the strong red tinge it imparted to the adjacent portions of the corona. It was also seen by Mr Lane, who observed the eclipse along with me. Mr Adie saw the same pro-

minence "distinctly," "with its marked colour;"* and it was seen by so many persons at Göteborg, that its visibility to the naked eye was a common subject of conversation for some days after the eclipse.

Mr Williams observes, that "the largest red prominence was visible by the unaided eye;"† and Mr Airy states, in his Account of the Total Eclipse of 1842, that an observer who accompanied him saw these objects with the naked eye.‡ The only observer of the late eclipse who formally states that he could not see the prominences without using a telescope, is Lieutenant Krag;§ but such negative evidence cannot affect the concurring testimony of so many observers who saw them distinctly by unaided vision; and we must therefore reject the idea that they are caused by the telescopes used in observing the eclipse.‖

2. *On the Hypothesis that the Red Prominences are Phenomena arising from the Action of unequally heated Strata of Air on the Sun's rays.*

Another opinion regarding the red prominences is that advanced by M. Faye, who conceives them to arise from a species of mirage, occasioned by the unequally heated state of the atmosphere during a total eclipse. The air all round the moon's shadow is heated by the sun, while that within the shadow is sheltered from his rays. This he conceives occasions a reduction of temperature in the air within the shadow; and the warm air without, communicating its heat to that within, gives rise to a succession of concentric layers gradually decreasing in temperature, from the surface of the shadow inwards. These layers of unequal density, acting on the rays proceeding from the edge of the moon to the observer's eye, will, it is assumed,—like the unequally heated strata of air which sometimes exist near the horizon,—produce the well known phenomena of mirage. The red prominences, he supposes, are then merely the magnified and distorted images of lunar mountains, illuminated obliquely by the sun.¶

* Edinburgh New Philosophical Journal, Oct. 1851, p. 375.

† Royal Ast. Soc. Notice, Jan. 1852, p. 54.

‡ Royal Ast. Soc. Notice, for Nov. 1842, p. 220. § Royal Ast. Soc. Notice, Jan. 1852, p. 47.

‖ M. Arago's highly interesting Account of the Total Eclipse of July 1842, in the *Annuaire* for 1846, contains ample evidence of the visibility of the red prominences to the naked eye. The following are some of the testimonies to that fact. M. Arago says, "A Perpignan, plusieurs personnes *virent les protubérances à l'œil nu.* Le fait n'est pas douteux." (p. 412.) M. Flauguergues, who observed at Toulon, remarks, "Je n'avais point encore repris le télescope, lorsque je fus surpris par l'apparition *d'un point lumineux rouge;* puis, *d'un second point semblable.*" (p. 418.) M. Santini, who observed the eclipse at Padua, relates that several persons saw the prominences with the naked eye, (p. 427.)

I did not obtain access to M. Arago's admirable Memoir until after this paper had been read; otherwise I should have gladly availed myself more fully of its valuable contents than is now possible.

¶ "—— Cette atmosphère conique ['le cône d'ombre'] doit produire, dans ses couches successives, concentriques et de plus en plus froides, les phénomènes analogues aux réfractions qui s'opèrent près de l'horizon, en un mot, des phénomènes de mirage." "Les montagnes roses qui apparurent alors [8 Juillet 1842], ne seraient autre chose que les images démesurément agrandies et déformées de quelques parties des montagnes lunaires, éclairées obliquement par le Soleil, et visibles à travers des vallées qui se trouvent çà et là, dans une direction favorable, sur le bord apparent de la Lune."—*Comptes Rendus de l'Academie*, 4 Nov. 1850, p. 643.

In reference to this opinion, Mr AIRY has observed, "that in the rapid passage of the moon's shadow he conceived it impossible to find air in the state required for the explanation"* proposed by M. FAYE. But even if the atmosphere exist in the state he has supposed, it is evident that the inequality of temperature in the successive layers of air, must decrease rapidly from the surface of the shadow inwards, and hence the phenomena of mirage must vary, according as the observer is situated near the edge of the shadow or near its centre. If then, the prominences are caused by the unequal heating of the air, on the two sides of the path of light; we might expect them to attain their maximum size and distinctness near the beginning and end of the total phase of the eclipse, and about those times, they ought to vary rapidly in appearance: for the light passing near the surface of the shadow then traverses, in succession, strata of air of rapidly decreasing temperature. Near the middle of the totality, on the contrary, the phenomenon ought to be almost insensible, as the rays then traverse air far removed from the heating action of the sun, and of nearly uniform temperature. Let us inquire how this agrees with what was actually observed at the late eclipse.

Mr DUNKIN remarks, that one of the prominences "was most curiously formed, having something of a horned shape;" that his "eye was intently fixed upon it for about a minute of time, and during that interval not the slightest change took place in its form."† Lieutenant PETTERSSON "observed no change [in the form of the prominences] that was not due to the motion of the moon."‡ According to Mr ADIE, "no change was observed in the form or position of the prominences, or in the position of the detached mass of light [relatively] to that of the crescent, farther than that due to the motion of the moon; nor did there appear any instability or wavering, in their colour or intensity."§ Mr CARRINGTON "cannot depose to have seen the slightest change" of outline in the large prominence: and he afterwards states, that the prominences had "hard and well-defined outlines."‖ Mr LASSELL states, that "the prominences were of a most brilliant lake colour, a splendid pink, quite defined and hard. They appeared to him 'not quite quiescent, but the moon by her movement might cause an idea of motion.'"¶ With reference to the largest prominence, Mr HIND says he "perceived no change of form or motion, and it was visible four seconds after the sun reappeared, but detached from the sun, the strong white light of the corona being visible between it and the sun."** Mr DAWES observes, regarding the same prominence, that "its apex was paler than the base, and of a purplish tinge; and it certainly had a flicker-

* Lecture by Mr AIRY on the Total Solar Eclipse of 1851, July 28, p. 6.—*Athenæum*, No. 1230, p. 559.

† Ast. Soc. Notice, p. 46. ‡ Ibid., p. 58. § Edin. New Phil. Journal, 1851, p. 375.

‖ An Account of the late Total Eclipse of the Sun, by R. C. CARRINGTON, Esq., pp. 7, 10.

¶ Ast. Soc. Notice, p. 53. ** Ibid., p. 67.

ing motion. Its base was from first to last sharply bounded by the edge of the moon." "To my great astonishment," he adds, "this marvellous object *continued visible for about five seconds*, as nearly as I could judge, *after the sun began to reappear*, which took place many degrees to the south of the situation it occupied on the moon's circumference. It then rapidly faded away, but it did not vanish instantaneously."*

These observations seem quite inexplicable, on the hypothesis that the prominences result from mirage occasioned by the unequal heating of the air. For not only did they preserve their forms unchanged during a period at which little or no unequal heating of the air could have taken place; but according to the very important observations of Mr Dawes and Mr Hind, they continued visible, apparently without change of form, even after the reappearance of the sun. Now, at the reappearance of the sun, the air in the path of light would rapidly pass through the three states, of being first entirely protected from the sun's rays, then heated on one side at the moment of reappearance, and finally heated on both sides.† About that time, then, if phenomena of the nature of mirage existed, we might expect the most rapid and conspicuous changes of form to occur; but instead of this being the case, the prominences retained their forms unaltered, until they vanished before the direct light of the sun. On these grounds, we must therefore regard the hypothesis which would refer them to the unequal heating of the air, as quite untenable.

* Ast. Soc. Notice, p. 69; or Astronomische Nachrichten, No. 777.

M. Mayette at the eclipse of 1842, saw one of the red prominences after the sun had reappeared (*quelques instantes* apres *l'emersion du Soleil.*)—*Annuaire*, for 1846, p. 411; see also p. 421. M. Conti saw the prominences for a long time (*per lungo tempo*), after the reappearance of the sun; and M. Biela for some seconds, pp. 428, 429. The statement of the latter observer is particularly explicit. "Les premiers rayons du Soleil se montrerent en divers points séparés. Bientôt ces points se réunirents et formerent une lunule tres-déliée. *Quelques secondes après* la formation de cette lunule, les pyramides rougeâtres cessèrent de se voir."

† May not the unequal heating of the air on the two sides of the path of the solar rays be the chief cause of the remarkable fluctuations in the sun's light, which have been observed at the beginning and end of the total phase of a solar eclipse? M. Savournin, an observer of the eclipse of July 1842, relates, "On a vu ici des ombres et des taches lumineuses courir les unes après les autres, comme paraissent le faire les ombres produites par de petits nuages qui passent successivement sur le Soleil. Ces taches n'étaient pas de la même couleur; il y en avait de rouges, de jaunes, de bleues, de blanches. Les enfants les poursuivaient et essayaient de mettre la main dessus. Ce phénomène extraordinaire fut remarqué quelques instants seulement avant la disparition complète du Soleil."—*Annuaire* for 1846, p. 393. The strata of illuminated and dark air at the surface of the moon's shadow, if their temperatures, and consequently their densities differ, cannot fail to mingle irregularly, and occasion fluctuating movements in the transmitted rays of light, similar to those which cause the dancing motion of objects seen through an ascending current of heated air, or through liquids of unequal densities which are in the act of mixing. This may also serve to explain the flickering appearance of the prominences noticed by some observers; which, from the terms used in describing it, was evidently not a permanent change of outline, but merely a fluctuation of their forms about a mean condition. Thus Mr Dawes and Mr Good, who saw on the moon's southern limb a long range of low prominences, both describe it as in motion. Mr Dawes, however, says, its irregularities appeared permanent, and he ascribes its undulation to our own atmosphere.

3. *On the Hypothesis which would refer the Phenomenon of the Red Prominences to the Action of the Moon's Limb on the Sun's Rays.*

If we suppose the prominences to be caused by some action of the moon's limb on the rays of light, whatever hypothesis we form regarding the precise nature of that action, it is evident, that the effect produced will depend in some way upon the relative positions of the luminous object,—of the body acting on its light,—and of the observer's eye. Any change in the position of the observer relatively to the sun and moon, would seem to necessitate some change in the appearance of the red prominences, supposing them optical phenomena of the nature of reflexion or diffraction; and these are the only known species of phenomena which the action of the moon's limb on the sun's light would occasion.

Now, as the moon and earth are in rapid motion, the position of an observer relatively to the line joining their centres is continually changing; and in order to see the supposed optical phenomena always from the same point of view, it would literally become necessary for him to run a race with the moon's shadow. It thus seems difficult to avoid the conclusion, that if the red prominences were caused by the action of the moon's limb on the sun's light, their appearance should rapidly change during the progress of the eclipse. But it has already been seen, that their forms remained unaltered; and it is therefore in the last degree improbable that they are optical phenomena, caused by the action of the moon's limb on the sun's light.

II. *On the Hypothesis that the Red Prominences exist in the Sun or Moon.*

On these grounds it seems impossible to regard the prominences as mere optical phenomena. Let us now inquire whether equal difficulties attend the supposition that they are *objects really existing* in the sun or moon.

1. *On the Discrepancies in the Observed Positions of the Red Prominences.*

The observers of the late eclipse seem frequently to have adopted no better means of ascertaining the angles of position of the red prominences, than estimation by the eye, with reference either to the sun's vertex or north point; and in many cases the point of reference is confessedly only roughly estimated. In some instances also, the angles have been merely guessed by the editor of the Royal Astronomical Society's Transactions, from the drawings furnished by the observers;* and in such circumstances, we may be prepared to expect notable discrepancies in the observations.

In other cases, however, greater care was taken to ensure accuracy. Thus Mr Dawes observed the eclipse with a telescope equatorially mounted, having cross

* Royal Ast. Soc. Notice, p. 43.

spider-lines in the eye-piece, which were carefully adjusted to polar and equatorial directions.* By this arrangement, the moon's limb could be readily divided into four quadrants, so as to facilitate the estimation of angles of position.

I employed a position micrometer, expressly devised for the purpose of registering the places of the red prominences;† and although we witnessed the eclipse under very different circumstances, and I failed to see a number of prominences which Mr DAWES has figured, yet our observations of the objects which we both saw, agree so closely, as to render it probable, that if some efficient means of ascertaining angles of position had been generally adopted, the observations would usually have been accordant.

On comparing the different observations, it appears that at least two isolated red prominences were seen to the east of the sun's north point; a long sierra or range of red prominences on the sun's southern limb; two detached prominences towards the west of the sun's vertex; a large hook-shaped prominence also to the west; a small prominence detached from the moon's limb, a little to the south of the hook-shaped prominence; and two prominences between the large one and the western end of the sierra.

These objects were by no means equally remarkable in appearance; and, accordingly, they did not all receive the same share of attention. Probably on this account, differences, so great, occur among the observed angles of position, in the case of some of the less conspicuous prominences, as to render it impossible, in some instances, to determine with certainty to which of them the observations refer. I think it then sufficient, to select the hook-shaped prominence already noticed, as the object which, on the whole, excited most attention,—whose place may thus be assumed to have been the best ascertained,—and of which the observed angles of position are therefore the most likely to throw light on the nature of the red prominences.

In the absence of information regarding the manner in which the different observers ascertained their angles of position, I have given all the observations equal weight; and the following table exhibits the several positions assigned to the hook-shaped prominence, with the difference of each from the mean of the whole.

Such of the angles as were reckoned from the sun's vertex, I have reduced to his north point, by means of the latitudes of the stations, and the times of observation, supposing the observations to be made at the middle of the total phase of the eclipse. As, however, those data are sometimes only approximately known, the reduction of the observed angle is not always quite correct; yet, I believe, the error in no case will be found to amount to 1°, so that the comparison of the observations is sufficiently exact for the purpose intended.

* Astronomische Nachrichten, No. 777. † P. 337.

Observed Angles of Position of the Hook-shaped Red Prominence.

Observer.	Angle of Position from Sun's North Point.	Difference.
Adie, . .	293°·6	+17°·5
Airy, . .	302·6	+26·5
Carrington, .	272·2	− 3·9
Dawes, . .	282·5	+ 6·4
Dunkin, . .	245·3	−30·8
Good, . .	287·1	+11·0
Gray, . .	255	−21·1
Hind, . .	275	− 1·1
Humphreys, .	259·9	−16·2
Jackson, .	280·5	+ 4·4
Lassell, . .	270	− 6·1
Pettersson, .	282·6	+ 6·5
Snow, . .	265·3	−10·8
Swan, . .	282·1	+ 6·0
Wichmann, .	284	+ 7·9
Williams, .	280	+ 3·9
Mean of all, .	276·1	0·0

I believe the discrepancies exhibited by these observations are fairly within the limits of error, when it is considered that the angles of position were roughly estimated during the haste and excitement unavoidably attending observations of a total solar eclipse.* If, on the other hand, they are regarded as too great to arise from mere errors of observation,—and it be attempted to reconcile the observations, by supposing that the prominences are merely optical phenomena, which actually appeared differently at different stations,—it can easily be shewn, that nothing is gained by such a course.

Granting, for the sake of argument, that the prominences are optical phenomena, it would still follow, that they should have appeared in exactly the same positions to observers situated at precisely the same point on the earth's surface. Yet we find Mr Lassell and Mr Williams differ by 10°, in assigning the position of the hook-shaped prominence, although they observed from the same house.†

* M. Arago observes, "Admettons un moment que les flammes étaient des *parties intégrantes du Soleil*,"—"Deux quelconques de ces flammes ayant été visibles dans deux stations différentes, a Montpellier et à Turin par exemple, ne purent manquer de s'y présenter dans les mêmes positions relatives et avec des formes identiques. Or les relations ne s'accordent pas toutes avec ce principe. Je m'empresse d'ajouter que la brièvéte du temps dont les astronomes purent disposer pour mesurer les protuberances, pour determiner leurs assiette, et par-dessus tout, que la surpris que chacun éprouva au moment d'une apparition si inattendue, durent beaucoup nuire à l'exactitude des observations."—*Annuaire* for 1846, p. 453. The observers of the late eclipse, certainly cannot plead the surprise occasioned by an unforeseen appearance as a reason why their accounts of the red prominences are not more consistent. But I believe they will agree with me in thinking, that a closer coincidence cannot be expected in observations so hastily conducted, and where the phenomenon observed was one whose novelty and grandeur were fitted to excite the most powerful emotions.

† Ast. Soc. Notice, pp. 53, 54.

Mr Dunkin and Mr Snow, who were both stationed near the observatory in Christiania, differ by 20′ in their observations of the same remarkable object. Lieutenant Pettersson, Mr Adie, Mr Airy, and myself, were all situated within a circle of about two miles radius, yet while Lieutenant Pettersson's observation of the hook-shaped prominence agrees almost exactly with mine, Mr Adie, and Mr Airy differ from us by 13′ and 22′ respectively. Here then, where, even on the hypothesis that the prominences are merely optical phenomena, we should expect identity of position, we meet with alarming discrepancies.

On the other hand, although Mr Dawes was stationed above 100 miles from Göteborg, the position he assigns to the hook-shaped prominence, agrees almost exactly with that given by Lieutenant Pettersson and by me; and Mr Hind also, who was near Mr Dawes, differs from us by less than 8°. Now, as Göteborg was near the middle of the moon's shadow, while Ravelsberg, where Mr Hind and Mr Dawes observed, was near the southern edge of the shadow, the eclipse was seen at the two stations under widely different circumstances; and on the optical hypothesis, we might expect great discrepancy in the angles of position. The coincidence in the observations is therefore strongly in favour of the view that the prominences are material objects; and this conclusion is strengthened, when it is borne in mind that the hook-shaped prominence being seen near one of Mr Dawes's cross-wires, its position could be estimated with great accuracy, and my angles of position were actually measured; so that the close agreement of our observations is by no means to be attributed to chance. The only other person, so far as I am at present aware, who has determined the position of the hook-shaped prominence by actual measurement, is M. Wichmann, who observed the eclipse with the Königsberg heliometer. He states his determination as somewhat doubtful; but it agrees so well with that of Mr Dawes, and with my own, as to render it highly probable that the positions of that prominence, as seen from stations nearly 400 miles distant, were identical. The following table contains the observations to which I have now referred; and I have added those of the spots on the sun, in order that it may be seen that the discrepancies in the observed positions of the prominence scarcely exceed those in the positions of the spots. As the spots are objects which, while they last, have their positions, if not permanent, at least subject only to small and slow changes, we cannot attribute the variations in their observed positions to change of place. We must therefore refer these discrepancies to errors of observation; from which it follows inevitably, that the variations in the observed positions of the prominence, as they scarcely exceed those in the positions of the spots, are also within the limits of errors of observation.

The agreement of the measured angles of position of that object appear, indeed, sufficiently close, when we advert to the circumstance that it must have

* Astron., Nachrichten, No. 787, p. 323.

subtended an angle of not less than 4°·5 on the moon's limb;* and its figure being irregular, the different observers may have estimated the position of points in it whose places varied considerably.

Angles of Position of the Hook-Shaped Prominence.

Observer.	Station.	Angle of Position of Hook-Shaped Prominence from Sun's North Point.	Angles of Position of Spots near the Sun's Limb.	
Dawes,	Ravelsberg,	282° 30′	88° 0′	
Swan,	Göteborg,	282 8	87 17	288° 47′
Wichmann,	Königsberg,	284 0	86 40	287 7

It thus appears, that the discrepancies in the angles of position, cannot be explained on the hypothesis that the prominences are merely optical phenomena which appeared differently at different stations; for as great differences occur between observations made at the same place, as between those made at stations widely removed from each other. It has also been seen, that where the angles of position were carefully ascertained, the places of prominences seen at distant stations, situated very differently in the moon's shadow agreed closely; which is unfavourable to the idea that these objects are merely optical phenomena.

2. *On the Discrepancies in the Forms assigned to the Red Prominences by different Observers.*

The forms assigned by different observers to the red prominences exhibit, as might be expected, considerable diversity. The large hook-shaped prominence, to which reference has been so often made, was seen by every one, and engrossed a large share of attention. Several drawings of this remarkable object by different observers, are given in fig. 9, Plate XI.† In its neighbourhood was a small red spot, completely detached from the sun's limb, and also a low prominence, neither of which was seen by all the observers. The drawings of this group differ in the occasional absence of the smaller detached prominence, or of the low one, and also in the form assigned to the hook-shaped prominence; but all agree in giving the latter a form curved in the same direction. Considering the hasty nature of the observations, the various powers possessed by different individuals of delineating objects, and the fact that the drawings must either have been

* In this estimation it is supposed that the breadth of the prominence was about two-thirds of its height, or 80″; an assumption which seems fully warranted by the drawings of the prominence given by most of the observers.

† The drawings of the hook-shaped prominence in fig. 9, are all taken from the Royal Astronomical Society's Notice for January, with the exception of Mr Adie's, which is enlarged from the plate accompanying his account of the eclipse in the Edinburgh New Philosophical Journal.

made from memory, or hastily sketched during the totality; there seems sufficient resemblance between them, to shew that they all represent the same object.* But here again, if the differences in the drawings are thought sufficient to shew that the objects were optical phenomena,—differently delineated by the observers, because their forms actually varied when seen at different stations,—it will be found that the difficulties are as great as before.

If the figures given by Mr LASSELL, Mr WILLIAMS, and Mr STANNISTREET (See fig. 9, Nos. 1, 2, 3), who observed from the windows of the same house, be compared, it will be found that they exhibit as great inconsistencies as any of the other drawings: for all assign to the hook-shaped prominence different forms; and while Mr WILLIAMS did not see the detached prominence, nor Mr LASSELL the low one, Mr STANNISTREET saw both. Again, Professor CHEVALLIER and Mr ADIE (See fig. 9, Nos. 4 and 5), who observed from the roof of the same house, differ greatly in the delineation of the hook-shaped prominence; for while the latter saw the detached prominence, the former did not see it. Lieutenant PETTERSSON also, who was scarcely a mile distant from them, gives a figure of the hook-shaped prominence totally unlike their drawings (See No. 6). Contrasted with this, we have Mr AIRY'S and Mr CHEVALLIER'S drawings agreeing well with that of Mr WILLIAMS (See Nos. 7, 4, and 2), who was distant 40 miles from them; and Mr HIND'S and Mr DAWES' resemble closely my own (fig. 9, Nos. 8 and 9, and fig. 8), although we observed at a distance of nearly 100 miles. In this case, then, as formerly, the optical hypothesis is of no service in reconciling discrepancies between the observations; for we have the observations agreeing, where on that hypothesis we should expect them to differ, and differing where they ought to agree.

It thus appears, that any objections to the hypothesis, that the prominences are objects existing in the sun or moon, founded on a want of agreement in the observed angles of position, and in the forms assigned to those objects, apply with at least equal force to the hypotheses that they are optical phenomena; while it has already been shewn, that the latter hypotheses labour under insurmountable difficulties peculiar to themselves. The objections to the idea, that the prominences are material objects being thus more than neutralised, the coincidence in the observations of position at tolerably distant stations in cases where the angles were carefully ascertained, affords the undiminished weight of

* While the causes now enumerated account sufficiently for much of the general diversity in the representations of the hook-shaped prominence, there are at the same time certain different *types of form* which may be observed among the drawings, and which can scarcely be referred to these causes. On comparing Nos. 2, 4, 7 with Nos. 8, 9, fig. 9, and also with fig. 8, it will be seen that the first three drawings are very like each other, as are also the last three, while there is little resemblance between the two sets. The first three represent the hook-shaped prominence as seen through rather large telescopes; the second, through small ones; and, as it is well known that certain telescopic objects vary greatly in appearance according to the instrumental power brought to bear on them, it may be worth inquiry, whether the same is not also the case with the red prominences.

its evidence in favour of that hypothesis. That they really are material objects, and that they are situated in the sun, and not in the moon, is rendered still more evident by the following facts.

3. *On the Different Appearances of the Red Prominences, as seen at Different Stations, compared with the Effects which Parallax would produce, if the Prominences existed in the Sun.*

If the prominences were in the moon, they ought to have been seen almost precisely in the same positions, and of the same forms by all the observers. If, on the other hand, they belong to the sun, when seen at all, their positions, and the forms of such parts of them as were visible, ought to have been identical in every case:—but owing to parallax, the moon would overlap the sun more on one side or the other, according to the observer's position with reference to the line of central eclipse; and thus a low prominence near the sun's north point might be hidden from an observer on the southern edge of the shadow, while a prominence near his south point might, in like manner, be invisible to an observer at the northern edge of the shadow.

It follows from this, that while differences in the angles of position, or forms, assigned to the prominence by different observers, are equally unfavourable to the supposition that they are real objects existing in the sun, or in the moon; differences in the number and magnitudes of the prominences, although unfavourable to the supposition that they exist in the moon, may admit of explanation on the hypothesis that they belong to the sun.

If, then, the prominences existed in the sun, the effect of parallax, to observers situated near the edge of the moon's shadow, would be to disclose the prominences on one side of the moon, while it hid those on the other side. Accordingly, we find that Mr Hind, Mr Dawes, and Mr Good, who were situated near the southern edge of the moon's shadow, saw a long sierra of prominences extending over about 120° of the moon's southern limb, while they all failed to see the prominences, situated near the sun's north point.* Even after making allowance for the effect of irradiation, which would diminish the apparent diameter of the moon, and thus increase the apparent height of the prominences, it appears that if their estimated height be correct, the parallax would be insufficient to hide any of them completely. Still, however, it might diminish their apparent heights so much, that in the haste with which the observations were necessarily made, they might be overlooked; and the discrepancy noticed above is therefore so far in favour of the hypothesis that the prominences belong to the sun.

4. *On the Occultation of the Red Prominences by the Moon.*

That the prominences belong to the sun, seems to be proved most decidedly

* Ast. Soc. Notice, pp. 67, 69; Edin. New Phil. Journal, Oct. 1851, pp. 365, 366.

by the fact, that the moon was seen by degrees to cover those which were situated on the side towards which it was moving, while it as gradually exposed those on the other side.

Of all the phenomena of the eclipse, there is none on which the testimony of the observers is more unanimous, than it is regarding this; and there is certainly none which, at the time, seemed to me more striking and beautiful, or which is now more strongly impressed on my memory.

The only observer whose testimony is decidedly opposed to the fact, that the moon occulted the prominences on one side, and disclosed them on the other, is Mr Dunkin, who watched the hook-shaped prominence for more than a minute, without perceiving the slightest change in its appearance. "It seemed to me," he remarks, "from the excessive steadiness of this prominence, and from the fact that I had zealously watched it for so long an interval without its undergoing any change, that this object had some connexion with the moon." He adds, however, "as my observations have been all made under rather difficult circumstances, it is possible I may be deceived."*

In opposition to this observation, we have the testimony of a large number of persons who saw the moon gradually occult those prominences which were situated on the sun's eastern limb, while those on the western limb were gradually elongated; and, in some instances, additional ones were seen on the west side, towards the end of the eclipse, which were not visible at the beginning.

Thus Mr Jackson states, that "on a second view, a little before the sun reappeared, a fourth prominence shewed itself at about 45′ from the vertex towards the west, and the other prominences, especially the hook-shaped one, were elongated."† Mr Stephenson remarks, that the large hook-shaped prominence "increased in size very rapidly, and then, other two rose-coloured prominences, one on the right and the other on the left, started out." "These red prominences began as red specks, which almost immediately became summits, by the extension below into bases.‡" Mr Lassell says, the prominences "were evidently belonging to the sun; for, especially on the western side, I observed that the moon passed over them, leaving them behind, and revealing successive portions as she advanced. I observed only the summit of *one* on the western side, although my friends in the adjoining room had seen two. The moon had covered one, and probably three fourths of the other, while I was engaged in registering the time, and making my observations with the naked eye."§ Mr Williams saw "two conical red prominences on the following or east limb;" and, "as the moon advanced, she speedily covered these." He again states, that "as the moon progressed and left it behind," the hook-shaped prominence on the west side "increased in size and brilliancy."‖ Mr Stannistreet says, the same prominence "appeared to alter

* Ast. Soc. Notice, p. 46. † Ib., p. 49. ‡ Ib., p. 50. § Ib., p. 53. ‖ Ib., p. 54.

its shape rapidly, unfolding more and more of the curve, as the phase proceeded."* Mr CARRINGTON saw a small pink prominence at an angle of 100° from the upper limb reckoned towards the east; it was of the form of a hay-rick, and rapidly diminished," so that "in 10ˢ it was no longer seen;" on the other hand, he had "no manner of doubt" that the prominences to the west of the sun's vertex "increased in size so as to be five times as large" as when they first appeared. He afterwards adds, "that these changes are fully accounted for by the moon's motion;" and he concludes, that "the prominences are appendages to the sun."† Lieutenant PETTERSSON says, that the movement of the prominences relatively to the limb of the moon, and above all, the successive removal of a detached prominence, which was at first in contact with it, convinced him that they belong to the sun.‡

According to Mr AIRY's observations, a prominence at first seen to the east "disappeared, the moon having overlapped it, and the two to the west, which touched the moon, were lengthened; the moon evidently having uncovered more of their bases;" while the detached mass "was further removed from the moon's limb," and "now a conical prominence came into sight" at about 60° to the west, measured from the sun's vertex." "Just before the sun reappeared all these objects were still further lengthened from the moon's motion," while a *sierra* or range of serrated eminences came into view." § Professor CHEVALLIER, who observed the eclipse with the high power of 180, is of opinion that the prominences "were certainly connected with the sun, for the separation of the edge of the moon from them, as she moved onwards, could be distinctly seen."‖ Mr HIND estimated the height of the hook-shaped prominence at 45″, about 20ˢ after the sun disappeared, and towards the end of the totality at 2′; "the moon having apparently left more and more of it visible, as she travelled across the sun." There was no change in the form of this object; and while the moon moved *away* from the detached prominence, the latter "*preserved its relative position*" to the hook-shaped one.¶ Mr ADIE says, that "as the moon advanced, the crescent," or hook-shaped prominence "increased in altitude," as did another prominence below it; while that "to the eastern side diminished to less than one-half the altitude it had when first observed;" and these changes, he thinks, afford the most satisfactory proof "that the prominences belong to the sun and not to the moon."** Mr DAWES states that the height of the hook-shaped prominence was perhaps 1′·5 when first seen, and that its height "increased to two minutes or more, as the moon's progress revealed it more entirely." The detached prominence "was sepa-

* Notice of R. Ast. Soc., p. 55.

† Account of the late Total Eclipse of the Sun, by R. C. CARRINGTON, pp. 6, 7, 10.

‡ "Le mouvements de ces dernières relativement au bord de la lune, et surtout l'éloignement successif de *d* du bord obscur, avec lequel je la vis premièrement en contact m'ont convaincu qu'elles appartenaient au Soléil." The letter *d* refers to his drawing of the detached prominence. See Plate XI., Fig. 9., No. 6.—*Manuscript Letter to the Author, dated 26th January* 1852.

§ Ast. Soc. Notice, p. 60. ‖ Ib., p. 65. ¶ Ib., p. 67.

** Edin. New Phil. Journal, for October 1851, pp. 374, 375.

rated from the moon's edge when first seen, and the separation increased as the moon advanced."*

My own observations of the prominences are accordant with those which have now been stated. A little after the commencement of the total phase, I determined their positions, and then left the telescope to make some other observations. On returning to the telescope, I found that the prominences on the moon's western limb had increased very sensibly in height; and on watching the hook-shaped prominence,—which I did until a few seconds before the end of the totality,—it seemed to rise from behind the moon, its base increasing in breadth, while the contour of the portions which were already visible, remained quite unaltered. Its motion, relatively to the moon, seemed to me quite sensible; but, although I may posssibly have been mistaken in this, I feel, no doubt whatever as to the striking difference between its height when first seen and that which it finally attained. Figs. 7 and 8, Plate XI., which are taken from a sketch made immediately after the total phase, represent this prominence as it was first and last seen. From its accidental resemblance to an object with whose form I happened to be familiar,† its shape was very distinctly impressed on my memory; and I feel satisfied that the change which took place in its appearance as the eclipse advanced, was precisely such as would have happened to a body of permanent form belonging to the sun, from which the moon gradually receded and left more and more of it exposed.

Numerous observers of the late eclipse, therefore, bear decided testimony to the fact, that the prominences situated on the side towards which the moon was moving, were occulted by it, while those on the opposite side were gradually exposed; and, at the same time, all are equally certain that the forms of those objects were in no other respect altered. I conceive, then, that unless we suppose they were deceived as to one or other of these points, we cannot hesitate to admit that the prominences are material objects, and that they exist in the sun. For if they were optical phenomena, it is quite inconceivable that the moon's motion should alter their *height alone*, while it did not at the same time affect their *forms*.‡

The discussion of the observations of the late eclipse seems, then, to lead to the following results:—

1. The red prominences are not caused by the telescopes used in observing the eclipse; for they were seen with the naked eye.

* Ast. Nachricht., No. 777, p. 157. † P. 342.

‡ The occultation of the prominences on the east side by the advancing moon, serves to explain some of the variations in the statements of different observers, as to their number. Mr LASSELL's observations already cited, shew that an observer might be too late on the outlook to see some of the prominences on the east side. Mr DUNKIN, Mr JACKSON, Mr HIND, Mr PETTERSSON, and myself, all saw no prominences to the east of the sun's vertex. At least three of these observers had their attention withdrawn from the red prominences by registering the time, and by making naked eye observations at the commencement of the total phase; while in Mr DUNKIN's case, the sun was covered with a cloud shortly after the commencement of the totality, and the prominences were not looked for until after it had passed away.

2. The red prominences cannot be regarded as optical phenomena, produced either by unequally heated strata of air, or by the action of the moon's limb on the sun's light; for these hypotheses are inconsistent, both with the permanency of their forms, and with the similarity of their appearance as seen from stations differently situated in the moon's shadow.

3. The discrepancies in the observations are as unfavourable to the supposition that the prominences are optical phenomena, as to the hypothesis that they are material objects belonging to the sun.

4. The observed differences in the numbers and appearance of the prominences as seen from stations differently situated in the moon's shadow, are, upon the whole, accordant with the effects which parallax would produce, if they existed in the sun.

5. The hypothesis that the prominences exist in the sun, seems to afford the only explanation of the facts, that the moon gradually occulted them on one side, and exposed them on the other, while their outlines remained unaltered.

6. On these grounds it is inferred, that the red prominences are material objects, actually belonging to the sun.

XXX.—*On the Red Prominences seen during Total Eclipses of the Sun.* Part II. By William Swan, F.R.S.E.

(Read April 19, 1852.)

In the first part of this paper, I have endeavoured to prove, that the red prominences seen during total solar eclipses, exist in the sun; and I now propose to state some views which have occurred to me as to the nature of those remarkable objects, and their possible connexion with other solar phenomena. It is not, however, without great misgivings that I venture on this subject; for we know so little of the sun, that any hypothesis regarding the constitution of his atmosphere, can amount only to a conjecture, possessing more or less probability according to the variety of the appearances it serves to explain, and the exactness with which theory and observation correspond in each case: and I am well aware that views which may seem probable to myself may not appear equally so to others, whose greater experience in observing the sun constitutes them better judges of such questions.

1. *On the Nature of the Red Prominences, and their Mode of Distribution in the Solar Atmosphere.*

For the reasons stated in the first part of this paper, I shall assume that the red prominences exist in the sun's atmosphere. They must, then, to use the words of Sir John Herschel, be "*cloudy masses, of the most excessive tenuity;*" * for being placed so near the sun, if their density approached that of the rarest terrestrial clouds, they could not fail to reflect an intensely brilliant light. Now this is far from being the case; for although they are by no means faint objects, neither are they very bright ones.

Another circumstance, which proves that the red prominences are gaseous and not solid bodies, is the overhanging form sometimes assumed by them, which, in the case of solid bodies, would result in the impending portions breaking off, and falling under the action of the sun's gravitation; and the same conclusion follows inevitably from the appearance, at the late eclipse, of a red mass completely detached from the moon's limb, and therefore evidently floating in the sun's atmosphere.†

The red prominences being thus obviously vaporous masses, I shall inquire, first, into the manner of their distribution in the solar atmosphere. Now the ob-

* Herschel's Outlines of Astronomy, par. 395.

† M. Arago reasons in the same manner, in the Annuaire for 1852, p. 344.

servations of the late eclipse lead to the conclusion, that the cloudy matter composing them is diffused over the surface of the sun, to an extent much greater than probably has hitherto been suspected. This appears from the following facts:—

Mr HIND,* Mr DAWES,† and Mr GOOD,‡ who were situated near the southern edge of the moon's shadow, so as to witness a nearly tangential eclipse, saw a long range of red prominences extending, with *little interruption, over nearly a third part of the moon's limb.* Observers who were situated near the middle of the shadow failed to see the greater part of this long range or sierra, which, owing to the effects of parallax, was probably almost entirely hid from them by the moon; but they saw isolated prominences, which were distributed pretty uniformly all round the moon's circumference. A band of red light on the moon's limb, which preceded the reappearance of the sun, was likewise seen by Professor CHEVALLIER, Lieutenant PETTERSSON, Mr JACKSON, and Mr GRAY; and a similar fringe of red light was observed by HALLEY at the total solar eclipse of 1715.§ In describing this remarkable phenomenon, Lieutenant PETTERSSON relates, that "about 5^s before the reappearance" of the sun, he "saw along the edge of the moon, just where he expected the sun" to reappear, "a red fringe of rose colour," on which the hook-shaped prominence, to which reference has so often been made already, "seemed to rest, and of which it seemed a part."‖

Obviously, the simplest view that can be taken of this phenomenon, is to regard the red fringe and the red prominences as of the same nature; and all the observations will then confirm the idea that the matter composing those objects *is distributed all round the sun.* It therefore seems probable, that when we are furnished with observations of a tangential phase of the eclipse from stations on the north side of the moon's shadow, it will be found that a sierra appeared towards the sun's north point, of which the detached prominences seen in that region, by observers situated near the middle of the moon's shadow, were only the highest peaks.

Now, in order to account for the phenomena exhibited by the spots on the sun, it has been supposed that they are portions of his comparatively dark body, seen

* Notice of R. Ast. Soc. for Jan. 1852, p. 67. † Ib., p. 69.

‡ Edinburgh New Philosophical Journal for Oct. 1851; or Astron. Nachrichten, No. 777.

§ Phil. Trans., vol. xxix.

‖ Notice of R. Ast. Soc., p. 59.

A red fringe also appeared towards the beginning and end of the total phase at the eclipse of 1842. Thus M. SCHUMACHER, who witnessed the eclipse at Vienna, relates,—"Peu de temps avant la fin de l'éclipse totale, il s'éleva vers cette partie du disque lunaire d'ou devait jaillir le premiere rayon de lumière, une étroite couche d'un rouge rosé qui s'étendait, peut-etre, sur un espace de 70 à 80 degrés le long du bord de la lune, et qui disparut, ainsi que l'anneau lumineux et les montagnes rouges, aussitôt que le premiere rayon du soleil jaillit."—*Annuaire* for 1846, p. 433.

M. STRUVE remarks.—"Je crois avoir vu, un instant avant la disparition du dernier rayon solaire, *une couche rouge au bord de la lune, à 45 degrés environ du point où le soleil disparaissait.*" —Ibid, p. 437.

through apertures, such as *i k* fig. 10, in a luminous envelope, *a b*, which surrounds him; and Sir WILLIAM HERSCHEL,* to explain the penumbra which generally encircles a solar spot, considers the luminous strata to be sustained far above the level of the sun's solid body, by a transparent elastic medium, carrying on its upper surface (or rather at some considerably lower level within its depth) a cloudy stratum *c d*, which being strongly illuminated from above, reflects a considerable portion of light to our eyes, and forms the penumbra, while the solid body shaded by the clouds reflects none. The temporary removal of both strata, but more of the upper than the lower, as represented in the figure, he supposes effected by powerful upward currents of the atmosphere, arising, perhaps, from spiracles in the body, or from local agitations.†

Since, then, it has been shewn to be highly probable that the matter composing the red prominences is distributed with little interruption all round the sun, we may conceive the luminous strata of the solar atmosphere to be surmounted by an *envelope* (*e f*,) *of clouds*, of which only the higher portions are visible beyond the moon's limb, at the central phase of a total eclipse; and which then constitute the red prominences. If it be thought that the hypothesis of two envelopes of cloud, one above and another below the luminous strata of the sun's atmosphere, introduces too great complication, we may avoid the objection, by supposing that the envelope which occasions the penumbræ around the spots penetrates the luminous stratum, and exists, although in greatly different degrees of density, both above and below it.

If, then, we conceive that a stratum of cloudy matter surrounds the sun, of which the red prominences are the higher portions, the serrated appearance of the long range of prominences, seen by Mr DAWES and Mr HIND, sufficiently indicates that its general surface is exceedingly uneven, presenting the appearance of being covered with numerous eminences or ridges. But these irregularities are small when compared with the large hook-shaped prominence, and its companion the detached cloud, which were seen by most of the observers of the eclipse. The altitude of the hook-shaped prominence has been variously estimated at from 1′·5 to 3′; and, by actual micrometrical measurement, it was found to be 1′ 41″·5 just before the sun reappeared.‡ Adopting this measurement, its actual height must have exceeded 47,000 miles, or about six times the diameter of the earth. The existence of bodies of such magnitude indicates some immense local disturbance in the sun's atmosphere, but not greater than that indicated by the solar spots, some of which Sir JOHN HERSCHEL states to have been observed, "whose linear diameter has been upwards of 45,000 miles, and even, if some records are to be

* Philosophical Transactions, 1801. † HERSCHEL's Outlines of Astronomy, par. 389.

‡ This is the mean of observations by Mr WILLIAMS and Mr STANNISTREET. Notice of R. Ast. Soc. for January 1852, pp. 54, 55.

trusted, of very much greater extent;"* and yet spots of such immense magnitude seldom last much longer than six weeks, so that their edges must often approach at the rate of 1000 miles a-day.†

Now, as the spots have been supposed to arise from upward currents causing apertures in the sun's luminous atmosphere, I conceive the higher red prominences, or those which remain visible at the middle of the total phase of a central eclipse, may in like manner be formed (as represented at *f* and *g*, fig. 10) by the same, or similar currents, in the sun's atmosphere, breaking through the envelope of cloud that surrounds him, bending back the edges of the apertures they have formed, and sometimes carrying up detached masses of cloud, such as that which was seen at the late eclipse. We may, however, suppose the envelope of cloud to be sometimes simply raised (as at *h*), without being broken through; and in that state it may form the conical prominences which were observed at the late eclipse.

Since the prominences reflect, they must also absorb light;‡ and thus, the hypothesis which has been proposed regarding them, *assumes the presence of an envelope of cloud surrounding the sun's luminous atmosphere, capable of absorbing part of his light, and subject to occasional interruptions of its continuity.*

If, then, such an envelope surrounds the sun, it will probably be connected with various solar phenomena. Let us now inquire whether any appearances presented by the sun afford additional evidence of its existence.

2. *On the Increased Brightness of the Corona in the neighbourhood of the Red Prominences.*

According to my observations of the late eclipse, the hook-shaped prominence was accompanied by increased brightness of the corona in its neighbourhood. (Plate XII.). Now this is a necessary consequence of the supposition, that the higher red prominences are the upturned edges of apertures in the envelope of cloud surrounding the sun; for the absorbent medium having been removed in forming the apertures, the sun's light ought to illuminate the corona more powerfully over an aperture than elsewhere. At the same time, it does not follow that all red prominences should be near bright portions of the corona; for a prominence may be formed by the cloudy stratum being simply raised, without being perforated.

* Dr Wilson of Glasgow, who, by observing a large solar spot, was led to the discovery that it was an aperture in the sun's luminous atmosphere, estimates the depth of its nucleus as "not less than a semidiameter of the earth below the level of the sun's spherical surface." See his highly interesting paper, Philosophical Transactions, vol. lxiv., 1774.

† Herschel's Outlines of Astronomy, 1851, par. 386.

‡ In certain circumstances steam has a red colour. May not the rose colour of the prominences indicate a property of the vapour composing them analogous to that possessed by steam, or, if they consist of aqueous vapour, identical with it? Professor Forbes, to whom we are indebted for our knowledge of the red colour of steam, regards it as the principal or only cause of the rosy tint observed in clouds.—See his interesting paper, Edinburgh Transactions, vol. xiv., p. 371.

3. *On the Diminished Brightness of the Sun's Disc towards its Edges.*

That the sun is surrounded by some medium capable of absorbing his light, has been considered to be satisfactorily proved by the rapidly diminishing brightness of his disc towards the edges; which, Sir JOHN HERSCHEL remarks, "can only arise from the circumferential rays having undergone the absorptive action of a much greater thickness of some imperfectly transparent medium (due to greater obliquity of their passage through it) than the central rays."* He further states, that if the sun had not an atmosphere capable of reflecting light, the sky ought to appear completely dark at a total eclipse of the sun. The existence of the corona round the moon is therefore a proof, he adds, that the sun has an atmosphere capable of reflecting, and therefore of absorbing light.

Now, the absorbing medium indicated by the corona is evidently an extremely diffuse atmosphere, extending to a great distance from the sun's surface; for the breadth of the corona is certainly not less than the sun's radius. It can, however, be easily shewn, that the darkness of the sun's limb compared with his centre, if it arises from the absorption of light by the solar atmosphere, must be occasioned, almost exclusively, by the absorptive power of those portions of the atmosphere which are near the surface of the sun.

It will be sufficiently accurate, for this purpose, to assume that the sun is a sphere, of which A B, fig. 11, represents the surface, and $a\,b\,b'\,a'$ a stratum of the solar atmosphere, of so small thickness that its absorptive power may be regarded as uniform. As the whole absorption, in passing through such a thin stratum, will be very small, we may safely assume that the light, in traversing a certain thickness of the absorbent medium, will thereby acquire no additional facility for penetrating the remaining portions of it; equal aliquot parts of the incident light will therefore be absorbed in passing through successive equal thicknesses of the stratum.

Suppose all the sun's atmosphere excepting $a\,b\,b'\,a'$ to be removed.

Let 1 = the number of intromitted rays,

$\frac{1}{m}$ = the number of rays lost by absorption and dispersion when the light has traversed a unit of thickness,

$$\mathrm{M} = 1 - \frac{1}{m},$$

t = the thickness $a\,a'$ of the stratum;

Then the number of rays that escape absorption after passing to the eye in the direction $a\,a'$, perpendicularly through the stratum, will be

$$\left(1 - \frac{1}{m}\right)^t = \mathrm{M}^t.$$

* Outlines of Astronomy, par. 395.

On the other hand, rays proceeding to the eye from the edge of the sun in the direction B b, will pass obliquely through the stratum, which, from its small thickness, may be regarded as a lamina, bounded by parallel planes perpendicular to B b. The thickness $b\,b'$ of the medium traversed by the rays, will therefore be

$$t \sec \mathrm{B}\,b\,\mathrm{C} = t \operatorname{cosec} \alpha,$$

where α is the angle B C b; and the number of rays that escape absorption will be

$$\left(1 - \frac{1}{m}\right)^{t \operatorname{cosec} \alpha} = \mathrm{M}^{t \operatorname{cosec} \alpha}$$

Then, putting h_1, h_2, for the apparent brightnesses of the sun's disc at A and B, as seen from the earth, the ratio of h_2 to h_1 will be that of the numbers of rays transmitted at the points $b'\,a'$; or,

$$\frac{h_2}{h_1} = \frac{\mathrm{M}^{t \operatorname{cosec} \alpha}}{\mathrm{M}^t} = \mathrm{M}^{t\,(\operatorname{cosec} \alpha - 1)}$$

Now, the nearer $a\,b$ is to the surface of the sun, the smaller is α, and for a small angle, cosec α will be very large; hence, for a stratum near the surface, since M is less than unity, the ratio of h_1 to h_2 will be very great. On the other hand, as the distance of the stratum from the surface increases, cosec α approaches to unity, and h_1 becomes nearly equal to h_2.

It thus appears that the effect of the oblique transmission of the lateral rays through the sun's atmosphere in increasing the absorption of those rays, and hence diminishing the apparent brightness of the sun's limb, as compared with his centre, is very great in the case of those strata of the atmosphere which are near his surface; but that it rapidly diminishes, as the strata are more and more removed from the surface of the sun. Hence, even if we suppose the solar atmosphere equally dense and equally absorptive throughout its whole extent, the diminished brightness of the sun's limb contrasted with that of his centre, would be due chiefly to the action of those portions of the atmosphere which are near his surface. But, besides this, from the necessarily rapid diminution of their density, the absorptive action of the successive strata of the solar atmosphere on light must quickly diminish, as their height above the surface increases; and this will conspire, with the continually lessening obliquity with which the sun's lateral rays traverse the higher strata of his atmosphere, so as to render their action in causing the diminished brightness of the sun's limb probably insensible.

Since, then, the observed darkness of the sun's limb, is due chiefly to a comparatively thin layer of atmosphere near his surface, the very notable amount of that darkness renders it necessary for us to regard those portions of the atmosphere as very highly absorptive. The thin envelope of cloud which has been supposed to surround the sun, near his surface, is precisely such an agent as would produce the phenomenon now under consideration; and we may thus, perhaps, regard the diminished brightness of the sun's limb as a corroborative proof of the existence of such an envelope.

4. *On the Faculæ and Luculi seen on the Sun's Disc.*

The supposition that the sun is surrounded by an envelope of cloud, occasionally penetrated by apertures, may serve to explain the *faculæ* and *luculi* seen on the solar disc. These are portions of the sun's surface brighter than the rest; and they have been supposed to be ridges in his luminous atmosphere, indicating violent agitation in their neighbourhood.* May they not, however, be simply apertures in the envelope of cloud? If such apertures exist, the sun's surface seen through them will appear more luminous than elsewhere; for his light passing through the apertures will escape, more or less, the absorption suffered by the rays which traverse the envelope itself. This explanation of the faculæ is quite consistent with the well-known fact, that they are best seen when near the sun's edge. Thus, if the dotted line $h\,i$ (fig. 11) represent the surface of the envelope, and $e\,f$ $e'\,f'$ two apertures in it, seen from a distant point in the prolongation of C A; the ray g will be contrasted with A h, which has passed perpendicularly through the envelope, while the ray g' is contrasted with B i, which has passed obliquely through the envelope, and therefore suffered more absorption than A a. It is evident, then, that the nearer a facula approaches the sun's limb, the more strongly will it contrast with the brightness of the surface in its neighbourhood, and the more distinctly will it be seen.

If, however, the faculæ be regarded as ridges in the sun's luminous atmosphere, their brightness, compared with the rest of the sun's disc, and their increased distinctness when near his limb, may also be explained by supposing that a thin envelope of cloud surrounds the sun; for if A B b' a' be the envelope, and A a the height of a ridge, the point a will appear brighter than the rest of the surface, because its light has only traversed the thickness $a\,a'$ of the absorbing medium, while that from the rest of the sun's surface has passed through the whole thickness A a', and has consequently been more absorbed. Again, if a ridge has the position $b\,d$, so as to be seen projected on the sun's limb,—supposing all the atmosphere above $a\,b$ removed,—we should have the light from a and b, which would not then suffer absorption, contrasted in the one case with light which had passed through the thickness A a, and in the other through the greater thickness B b of absorbent matter. The contrast at b would evidently then be greater than at a. If now the rays from A a traverse together the remaining portions of the sun's atmosphere, they will be almost equally absorbed, and the relative brightness of the points a A will not be sensibly altered; the same will be true with regard to B b. Hence the contrast of the brightness of the ridge, when compared with that of the sun's general surface, will still be greatest at the sun's limb.†

* Sir William Herschel. Philosophical Transactions, 1801.

† Sir William Herschel, who regards the faculæ as ridges, does not seem to hold that opinion very confidently. In his paper, already noticed, he only once states that they "have the appearance of elevations."—(Phil. Trans. 1801, p. 84.) Mr Dawes, however, has recently

It may be further observed, that if the red prominences are portions of the envelope of cloud, thrown upwards by currents in the sun's atmosphere, which tend to produce faculæ by ultimately rupturing the envelope, the prominences will have a physical connexion with the faculæ, but no *necessary* connexion with the solar spots.

It is well known, however, that conspicuous faculæ often occur in the neighbourhood of groups of spots; and the same upward currents in the sun's atmosphere, which are supposed to occasion the spots, by removing a portion of his luminous envelope, may also occasion the faculæ, by removing a portion of the superincumbent cloudy strata. The occurrence, immediately before an eclipse, of faculæ near the sun's limb, with or without accompanying spots, ought therefore to indicate a quarter where red prominences might be expected to appear; and at least one coincidence in the position of spots and red prominences was actually observed at the late eclipse. A group of spots was seen by me immediately before the eclipse, about 1′·5 from the sun's limb, and to the west of his vertex. The angle of position of this group, reduced to the sun's north point, was ascertained to be 288° 47′; and the conspicuous hook-shaped prominence, which appeared during the totality, was found to be situated at 282° 8′ from the sun's north point. The hook-shaped prominence may therefore have been connected with faculæ in the neighbourhood of the group of spots. The region where spots occur is limited to a zone, extending about 25° or 30° on either side of the sun's equator; and Mr Lassell observes, that, as at the late eclipse "some prominences appeared on parts of the sun's limb not usually traversed by spots, the connexion between the two is not made out."* This remark, however, does not apply to the supposed occasional relation between the prominences and the faculæ; for although faculæ are seen most abundantly in the neighbourhood of spots, they also occur by themselves, and have been observed all over the sun's disc; so that the prominences which are supposed to be connected with them might appear at any point in the sun's limb.

5. *On the Irregular Illumination of the Corona seen during Total Eclipses of the Sun.*

Let us next inquire how the hypothesis of an envelope of cloud surrounding the sun's luminous atmosphere, consists with the phenomena of the corona.

There are remarkable appearances in the corona, which seem quite inexpli-

observed a facula projecting beyond the sun's limb (R. Ast. Soc. Notice for April 1852). With reference to that observation, it may be permitted to remark,—that considering the rapid degradation in brightness of the sun's disc towards the edges,—it is evident that the dark glasses used in observing the sun may, to some extent, diminish its apparent diameter, the light from the extreme edge of the disc being possibly too feeble to be seen through the glass. A portion of the sun's limb brighter than the rest would be less encroached on from this cause, and would thus appear to project beyond the general outline. Irradiation would also conspire to increase this effect. The action of the dark glasses, in diminishing the sun's apparent diameter, may also be one of the causes of the anomalous variations in observations of the sun's diameter.

* R. Ast. Soc. Notice, p. 53.

cable, on the mere hypothesis that the sun possesses an atmosphere capable of absorbing light uniformly, but which are perfectly consistent with the supposition that he is surrounded by a stratum of cloudy matter, strongly absorbing his light, but subject to frequent interruptions of continuity. These are, the distinctly radiated structure of the corona, and the appearance of bundles of rays, separated by comparatively dark intervals. The different observers of the late eclipse are not quite agreed as to the appearance of the corona; but, upon the whole, there is a strong preponderating testimony to the effect that its illumination was far from uniform. Thus Mr SNOW says,—The corona "appeared not to be uniformly disposed, but in irregularly radiating bundles or masses."* M. LASSELL states, that the corona "was radiating, some of the rays appearing *longer* than the rest, but the irregularity was not great;"† and Mr WILLIAMS, that "it was divided by radial lines, and presented the appearance of luminous brushes shot out from behind the moon."‡ Dr BOUSTED saw the corona "somewhat rugged, and more extended towards the left of its upper part."§ In a drawing of the eclipse, communicated to me by Lieutenant PETTERSSON, he represents the corona as consisting of detached brushes of light; and he particularly points out the occurrence of a dark interval, 100° to the east of the sun's vertex. Mr ADIE saw "brighter coruscations shooting through it all round, extending beyond the general light of the corona, and having a kind of flickering appearance;"‖ and, according to Mr HIND, rays of light extended through and beyond the corona.¶ Mr AIRY represents the corona as consisting of bundles of rays, very strongly marked; and he describes its structure as radiated, and as terminating, though very indefinitely, in a manner resembling the ornament round a mariner's compass.** To me the corona seemed strongly radiated (see Plate XII.), the bright rays being separated by intervals of comparative darkness; and there were brilliant beams of light at particular points, brighter than the rest of the corona, and visible beyond its general outline. The largest and brightest of those beams or masses of light was situated 61° 8′ to the east of the sun's north point. Its form was conoidal, having its base towards the sun. The other beams were in the form of extremely acute cones, with their vertices towards the sun, and their sides apparently converging towards his centre.

It appears from these observations that there is something existing at the surface of the sun capable of intercepting his light unequally, and of causing a want of regularity in the illumination of the corona. For if we suppose the light to be transmitted uniformly by the sun's atmosphere, it would follow that rays proceeding from different points of his surface would cross each other's paths, and blend their effects; so that however irregular the luminosity of the surface, the illumination of the corona, at a little distance, would approach to uniformity. Its

* R. Ast. Soc. Notice, p. 47. † Ibid., p. 52. ‡ Ibid., p. 54. § Ibid., p. 57.
‖ Edinburgh New Philosophical Journal for October, p. 375. ¶ R. Ast. Soc. Notice, p. 67.
** Ibid., p. 60.

radiated structure seems therefore to indicate the existence of something tending to limit the transmission of the rays to directions normal to the sun's surface, and capable of absorbing them more powerfully at certain points than at others. The bright beams of light in the corona strongly resembled sunbeams shining through narrow apertures in clouds; and it was indeed that resemblance which first led me to entertain the idea that an envelope of cloudy matter surrounds the sun. Immediately after witnessing the late eclipse, when I reflected on the striking want of continuity I had observed in the illumination of the corona, I was strongly impressed with the conviction that something existed near the surface of the sun which intercepted his light more at certain points than at others. It then occurred to me, that as the red prominences, from their power of reflecting light, must also absorb it, the medium which absorbed the sun's light irregularly, and caused the unequal illumination of the corona, might be no other than the matter composing the red prominences; that matter being supposed to constitute an envelope surrounding the sun, of which the red prominences are only the higher, and probably the rarer portions.

If the faculæ are apertures in the envelope of cloud, as has been supposed, it will follow that faculæ near the sun's limb may be connected with the bright beams in the corona. If, then, a considerable portion of the sun's surface near his limb were intersected by numerous branching faculæ, with openings gradually increasing in width towards the centre of the group, we should probably have a mass of light in the corona like that represented in Plate XII., which was seen at the late eclipse about 30° to the east of the sun's vertex. On the other hand, light proceeding from a single long facula in the sun's limb, presented to the eye endwise, might occasion the appearance of the narrow bright beams which were seen in the corona; and a considerable variety of effects might be produced by faculæ whose positions were differently inclined to the visual direction.

The following queries embody the hypothesis which I have now ventured to propose regarding the red prominences, and the other solar phenomena which I have supposed to be connected with them :—

1. May not the sun's luminous atmosphere be surrounded by an *envelope of cloud* capable of absorbing part of his light, and having the property of appearing red when seen by reflected light?

2. As the spots on the sun have been supposed to be formed by upward currents in his atmosphere, may not the same, or similar currents, force up portions of the envelope of cloud, and sometimes actually rupture it?

3. May not the higher portions of the envelope of cloud be seen projecting beyond the moon's limb during the total phase of a solar eclipse, and thus constitute the *red prominences?*

4. May not this envelope be the chief agent in causing the diminished brightness of the sun's disc towards his edges, owing to the greater thickness of the

envelope, traversed obliquely by the rays, which reach the eye from points near his limb?

5. May not the *faculæ* and *luculi*, or bright portions of the sun's disc, be *apertures* in the *envelope*, through which his light passes with less absorption than at other places?

6. Will not the supposed connexion of the higher red prominences with the faculæ,—or apertures in the absorbent envelope,—explain the observed increase in the brightness of the corona in the neighbourhood of such prominences?

7. Will not the supposition, that an envelope of cloud surrounds the sun, capable of absorbing his light, but penetrated with apertures, so as to allow more light to escape at certain points than at others, explain the want of uniformity in the brightness of the corona, and the brilliant beams of light which occur in it?

When the preceding paper was read, I had not, as has already been noticed, seen M. ARAGO's Memoirs in the Annuaires for 1846 and 1852; nor had I seen the fourth volume of HUMBOLDT's Cosmos, recently published, which contains an exposition of M. ARAGO's views regarding the red prominences. In these circumstances, it may be proper to insert here some passages from those works, which, if I had obtained access to them at an earlier period, I should have embodied in the preceding pages.

" Après avoir constaté que le soleil se compose d'un corps obscur central, d'une atmosphère nuageuse réfléchissante et d'une photosphère, nous devons naturellement nous demander s'il n'y a rien au delà, si la photosphère finit brusquement et sans être entourée d'une atmosphère gazeuse peu lumineuse par elle-même, ou faiblement réfléchissante. Cette troisième atmosphère disparâitrait ordinairement dans l'océan de lumière dont le soleil parait toujours entouré."*

" Il faut admettre une enveloppe extérieure qui diminue (étient) moins la lumière qui vient du centre, que les rayons qui viennent sur le long trajet du bord à l'œil. Cette enveloppe extérieure forme la couronne blanchâtre dans l'eclipses totales du soleil." †

" L'eclipse de 1842 nous a mis sur la trace d'une troisième enveloppe située au-dessus de la photosphère, et formée nuages obscurs, ou faiblement lumineux."‡

" Jetons un coup d'oeil rapid sur une quatrième hypothèse, celle suivant laquelle les protubérances seraient assimilées à des nuages solaires nageant dans une atmosphère gazeuse. Nous ne trouverions aucun principe de physique qui put nous empêcher d'admettre l'existence de masses nuageuses de 25,000 à 30,000 lieues de long, à contours arrêtés et affectant des formes les plus tourmentées."§

" Dans la nombreuse catégorie des taches du soleil, quelle est la place qu'

* Annuaire for 1852, p. 342.
† ARAGO, quoted by HUMBOLDT, Cosmos, MURRAY's Edit., vol. iv., Notes, p. ciii.
‡ Annuaire, for 1846, p. 464.
§ Annuaire for 1852, p. 345.

occuperaient les nuages de la troisième enveloppe? *Peut-être* ces nuages produisent-ils les pénombres isolées, les pénombres sans noyau. Les taches de ce genre ne sont pas très-communes; jamais leur étendue totale n'est un partie aliquote considerable de la surface solaire."*

From these passages, it appears that in M. Arago's opinion, the sun, besides being surrounded by a stratum of dark clouds, and a *photosphere*, has, beyond them, and enveloping them, a third atmosphere, in which are floating clouds, which, when seen during a total eclipse, form the red prominences.

This is the same view as that stated at the beginning of my paper, on the authority of Sir John Herschel, and to which I have ventured to add the hypothesis that the clouds causing the prominences form of themselves a nearly continuous envelope, floating in the third atmosphere, and above the photosphere. If, then, a formal enumeration of the sun's envelopes were made, according to *my view* there would be *four*. 1*st*, The dark clouds below the photosphere. 2*d*, The photosphere itself. 3*d*, The envelope of cloud so often referred to; and, 4*th*, The sun's atmosphere surrounding all, and in which the other three solar envelopes may be supposed to float.

That M. Arago does not regard the clouds which occasion the red prominences as forming a continuous envelope, appears evident from several considerations. In enumerating the envelopes surrounding the sun, he never mentions more than *three*,—viz., the cloudy envelope below the photosphere, the photosphere itself, and a third atmosphere surrounding it. This third atmosphere is also spoken of as a third envelope, and as an exterior envelope; but these different expressions are evidently employed to denote, as one envelope, the sun's exterior *atmosphere, along with the clouds floating in it;* for if the atmosphere and the clouds were reckoned *separately*, there would be *four* envelopes and not *three*. The passage where the third envelope is said to be formed of clouds (*formée de nuages*) would, indeed, if read by itself, seem to convey the idea of an envelope, composed of clouds exclusively; but other passages sufficiently prove that this is not M. Arago's view. Thus the expression, "the clouds *of* the third envelope" indicates the idea that those clouds are not themselves the envelope, but detached masses floating in it. Moreover, since the terms, third envelope, third atmosphere, and exterior envelope, are obviously all used in the same sense,—the exterior envelope or third envelope cannot consist alone of a continuous stratum of clouds forming the *red* prominences; for it is elsewhere described as causing the *white* corona (*couronne blanchâtre*), which, in the case supposed, would be *red*, and not *white*.

It seems also highly probable, that if M. Arago had supposed that the clouds forming the red prominences also constitute a continuous stratum surrounding the sun, the phenomenon of a band of red light seen at the end of the total phase of solar eclipses,—which he believes† is caused by the presence of red promi-

* Annuaire for 1846, p. 465.

† Ibid. for 1842, p. 440, *et seq.*

nences,—would have been adduced in proof of the existence of such a stratum; but no such inference is drawn from that phenomenon.

Besides this, M. ARAGO supposes that the clouds forming the red prominences probably occasion the isolated spots which have no central nucleus (*penombres isolées, penombres sans noyau*); but, as he immediately adds, that those spots are by no means common, and when they occur, occupy but an inconsiderable part of the sun's disc, it is evident that he must conceive the clouds which form them to be isolated, and widely scattered,—an idea quite inconsistent with the supposition, that they form a continuous envelope surrounding the sun.

From these considerations, it appears evident that M. ARAGO's hypothesis regarding the red prominences, involves simply the idea that those objects are clouds floating in the sun's atmosphere; and therefore, that his opinion is the same as Sir JOHN HERSCHEL's, whose views, stated at p. 461, form the groundwork of the hypothesis I have now proposed. In addition to the idea that the red prominences are clouds,—which, according to HUMBOLDT, was first announced by M. ARAGO,—I have endeavoured to shew, that those clouds probably form a continuous envelope surrounding the sun; and I have further supposed that this envelope is the chief agent in causing the diminished brightness of the sun's disc towards the edges,—that when apertures occur in the envelope, they possibly constitute the faculæ on the solar disc,—and that those apertures also occasion the bright rays in the corona seen during total eclipses of the sun.

XXXI.—*On the Dynamical Theory of Heat.* Part V.* *On the Quantities of Mechanical Energy contained in a Fluid in Different States, as to Temperature and Density.* By William Thomson, M.A., Professor of Natural Philosophy in the University of Glasgow.

(Read December 15, 1851.)

81. A body which is either emitting heat, or altering its dimensions against resisting forces, is doing work upon matter external to it. The mechanical effect of this work, in one case, is the excitation of thermal motions, and in the other, the overcoming of resistances. The body must itself be altering in its circumstances, so as to contain a less store of work within it, by an amount precisely equal to the aggregate value of the mechanical effects produced: and conversely, the aggregate value of the mechanical effects produced, must depend solely on the initial and final states of the body, and is therefore the same, whatever be the intermediate states through which the body passes, provided the *initial* and *final* states be the same.

82. The total mechanical energy of a body might be defined as the mechanical value of all the effect it would produce, in heat emitted and in resistances overcome, if it were cooled to the utmost, and allowed to contract indefinitely or to expand indefinitely according as the forces between its particles are attractive or repulsive, when the thermal motions within it are all stopped; but in our present state of ignorance regarding perfect cold, and the nature of molecular forces, we cannot determine this "total mechanical energy" for any portion of matter, nor even can we be sure that it is not infinitely great for a finite portion of matter. Hence it is convenient to choose a certain state, as standard for the body under consideration, and, to use the unqualified term, *mechanical energy*, with reference to this standard state; so that the "mechanical energy of a body in a given state," will denote the mechanical value of the effects the body would produce in passing from the state in which it is given, to the standard state, or the mechanical value of the whole agency that would be required to bring the body from the standard state to the state in which it is given.

83. In the present communication, a system of formulæ founded on proposi-

* A preceding communication (April 21, 1851) published in the Transactions (Vol. xx., Part ii.), under the title, "On a Method of Discovering Experimentally the Relation between the Mechanical Work spent, and the Heat produced by the Compression of a Gaseous Fluid," will be referred to as Part IV. of a series of Papers on the Dynamical Theory of Heat; and the numbers of its sections will be altered accordingly, so that its first section will be referred to as § 61, and its 20th and last, as § 80.

tions established in the first part of my paper on the Dynamical Theory of Heat, and expressing relations between the pressure of a fluid, and the thermal capacities and mechanical energy of a given mass of it, all considered as functions of the temperature and volume, and CARNOT'S function of the temperature, are brought forward for the purpose of pointing out the importance of making the *mechanical energy* of a fluid in different states an object of research, along with the other elements which have hitherto been considered, and partially investigated in some cases.

84. If we consider the circumstances of a stated quantity (a unit of matter, a pound, for instance) of a fluid, we find that its condition, whether it be wholly in the liquid state, or wholly gaseous, or partly liquid and partly gaseous, is completely defined when its temperature, and the volume of the space within which it is contained, are specified (§§ 20, 53, ...56), it being understood, of course, that the dimensions of this space are so limited, that no sensible differences of density in different parts of the fluid are produced by gravity. We shall therefore consider the temperature, and the volume of unity of mass, of a fluid as the independent variables of which its pressure, thermal capacities, and mechanical energy, are functions. The volume and temperature being denoted respectively by v and t, let e be the mechanical energy, p the pressure, K the thermal capacity under constant pressure, and N the thermal capacity in constant volume; and let M be such a function of these elements, that

$$\mathrm{K}=\mathrm{N}+\frac{\frac{dp}{dt}}{-\frac{dp}{dv}}\mathrm{M} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1),$$

or (§§ 48, 20), such a quantity that

$$\mathrm{M}\,dv+\mathrm{N}\,dt \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2),$$

may express the quantity of heat that must be added to the fluid mass, to elevate its temperature by dt, when its volume is augmented by dv.

85. The mechanical value of the heat added to the fluid in any operation, or the quantity of heat added multiplied by J (the mechanical equivalent of the thermal unit), must be diminished by the work done by the fluid in expanding against resistance, to find the actual increase of mechanical energy which the body acquires. Hence, (de, of course, denoting the complete increment of e, when v and t are increased by dv and dt,) we have

$$de=\mathrm{J}\,(\mathrm{M}\,dv+\mathrm{N}\,dt)-p\,dv \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3).$$

Hence, according to the usual notation for partial differential coefficients, we have

$$\frac{de}{dv}=\mathrm{J}\,\mathrm{M}-p \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4),$$

$$\frac{de}{dt} = \mathrm{J\,N} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5).$$

Lastly, if we denote, as formerly, CARNOT's function of the temperature t, by μ, we have (§ 21)

$$\frac{dp}{dt} = \mu\,\mathrm{M} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6).$$

86. The use that may be made of these formulæ in investigations of the physical properties of any particular fluid must depend on the extent and accuracy of the general data belonging to the theory of the mechanical action of heat, that are available. Thus, if nothing be known by experiment regarding the values of J and μ, we may, in the first place, use equations (4) and (5), or the following deduced from them (§ 20) by eliminating e,

$$\frac{dp}{dt} = \mathrm{J}\left(\frac{d\mathrm{M}}{dt} - \frac{d\mathrm{N}}{dv}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7),$$

and equation (6), as tests of the accuracy of experimental researches on the pressure and thermal capacities of a fluid, on account of the knowledge we have from theory, that J is *certainly* an absolute constant, and that, in all probability if not with absolute certainty, we may regard μ as independent of v, and as the same for all fluids at the same temperature; and, with experimental data of sufficient extent, we may use these equations as means of actually determining the values of J and μ. No other way than this has yet been attempted for determining μ; and, if we except a conceivable but certainly not at present practicable mode of determining this element by experiments on thermo-electric currents, no other way is yet known. CARNOT's original determination of μ, was effected by means of an expression equivalent to that of equation (6) applied to the case of a mass of air; and the determinations by CLAPEYRON, and those shewn in Table I. of my Account of CARNOT's Theory, were calculated by the formula which is obtained when the same equation is applied to the case of a fluid mass, partly liquid and partly in the state of saturated vapour (§ 55).

87. As yet experiments have not been made on the pressure and thermal capacities of fluids to a sufficient extent to supply data for the evaluation, even in the roughest manner, of the expression given for J by equation (7); and it may be doubted whether such data can even be had with accuracy enough to give as exact a determination of this important element as may be effected by direct experiments on the generation of heat by means of friction. At present we may regard J as known, probably within $\frac{1}{300}$ of its own amount, by experiments of this kind.

88. The value of J being known, equations (4) and (5) may be used for determining the mechanical energy of a particular fluid mass in different states, from special experimental data regarding its pressure and thermal capacities, but not necessarily comprehending the values of each of these elements for all states of the fluid. The theory of the integration of functions of two independent variables

will, when any set of data are proposed, make it manifest whether or not they are sufficient, and will point out the methods, whether of summation or of analytical integration, according to the forms in which the data are furnished, to be followed for determining the value of e for every value of v. Or the data may be such that, while the thermal capacities would be derived from them by differentiation, values of e may be obtained from them without integration. Thus, if the fluid mass consist of water and vapour of water at the temperature t, weighing in all one pound, and occupying the volume v,* and if we regard the *zero* or "standard" state of the mass as being liquid water at the temperature 0 ; the mechanical energy of the mass, in the given state, will be the mechanical value of the heat required to raise the temperature of a pound of water from 0° to t, and to convert $\frac{v-\lambda}{\gamma-\lambda}$ of it into vapour, diminished by the work done in the expansion from the volume λ, to the volume v; that is, we have

$$e=\mathrm{J}\left(c\,t+\mathrm{L}\,\frac{v-\lambda}{\gamma-\lambda}\right)-p\,(v-\lambda) \quad . \quad . \quad . \quad . \quad . \quad (8).$$

The variables, c, L, and p (which depend on t alone) in this expression have been experimentally determined by Regnault, for all temperatures from 0° to 230°, and when γ is also determined, by experiments on the density of saturated steam, the elements for the determination of e in this case will be complete. The expressions investigated formerly for M and N in this case (§ 54) may be readily obtained by means of (4) and (5 of § 84), by the differentiation of (8).

89. If Carnot's function has once been determined by means of observations of any kind, whether on a single fluid, or on different fluids, for a certain range of temperatures, then, according to (6) of § 85, the value of $\frac{\frac{dp}{dt}}{\mathrm{M}}$ for any substance whatever, is known for all temperatures within that range. It follows that when the values of M for different states of a fluid have been determined experimentally, the law of pressures for all temperatures and volumes (with an arbitrary function of v to be determined by experiments on the pressure of the fluid at one particular temperature) may be deduced, by means of equation (6); or conversely, which is more likely to be the case for any particular fluid, if the law of pressures is completely known, M may be deduced without farther experimenting. Hence the second member of (4) becomes completely known, the equation assuming the following form when, for M, its value according to (6) is substituted:—

* The same notation is used here, as formerly in § 54, viz. p is the pressure of saturated vapour at the temperature t, γ the volume, and L the latent heat of a pound of the vapour, λ the volume of a pound of liquid water, and c the mean thermal capacity of a pound of water between the temperatures 0 and t. A mass weighing a pound, and occupying the volume v, when at the temperature t, must consist of a weight $\frac{v-\lambda}{\gamma-\lambda}$ of vapour, and $\frac{\gamma-v}{\gamma-\lambda}$ of water.

$$\frac{de}{dv} = \frac{J}{\mu}\frac{dp}{dt} - p \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8).$$

The integration of this equation with reference to v, leads to an expression for e, involving an arbitrary function of t, for the determination of which more data from experiment are required. It would, for instance, be sufficient for this purpose, to have the mechanical energy of the fluid for all temperatures when contained in a constant volume; or, what amounts to the same (it being now supposed that J is known), to have the thermal capacity of the fluid in constant volume, for a particular volume and all temperatures. Hence, we conclude, that when the elements J and μ belonging to the general theory of the mechanical action of heat are known, the mechanical energy of a particular fluid may be investigated without experiment, from determinations of its pressure for all temperatures and volumes, and its thermal capacity for any particular constant volume and all temperatures.

90. For example, let the fluid be atmospheric air, or any other subject to the "gaseous" laws. Then if v_0 be the volume of a unit of weight of the fluid, and 0 the temperature, in the standard state from which the mechanical energy in any other state is reckoned, and if p_0 denote the corresponding pressure, we have

$$p = \frac{p_0 v_0}{v}(1+\mathrm{E}t), \quad \frac{dp}{dt} = \frac{p_0 v_0 \mathrm{E}}{v},$$

and

$$\int_{v_0}^{v}\left(\frac{\mathrm{J}}{\mu}\frac{dp}{dt} - p\right)dv = p_0 v_0 \left\{\frac{\mathrm{JE}}{\mu} - (1+\mathrm{E}t)\right\}\log\frac{v}{v_0}$$

Hence, if we denote by N_0 the value of N when $v=v_0$, whatever be the temperature, we have, as the general expression for the mechanical energy of a unit weight of a fluid subject to the gaseous laws,

$$e = p_0 v_0 \left\{\frac{\mathrm{JE}}{\mu} - (1+\mathrm{E}t)\right\}\log\frac{v}{v_0} + \mathrm{J}\int_0^t \mathrm{N}_0\, dt \quad . \quad . \quad . \quad . \quad (9).$$

91. Let us now suppose the mechanical energy of a particular fluid mass in various states to have been determined in any way, and let us find what results regarding its pressure and thermal capacities may be deduced. In the first place, by integrating equation (8), considered as a differential equation with reference to t, for p, we find

$$p = \epsilon^{\frac{1}{\mathrm{J}}\int_0^t \mu\, dt}\int_0^t \mu\frac{de}{dv}\epsilon^{-\frac{1}{\mathrm{J}}\int_0^t \mu\, dt}\, dt + \psi(v)\,\epsilon^{\frac{1}{\mathrm{J}}\int_0^t \mu\, dt} \quad . \quad . \quad . \quad (10),$$

where $\psi(v)$ denotes a constant with reference to t, which may vary with v, and cannot be determined without experiment. Again, we have, from (5), (4), and (1),

$$\left.\begin{aligned} \mathrm{N} &= \frac{1}{\mathrm{J}}\frac{de}{dt} \\ \mathrm{K} &= \frac{1}{\mathrm{J}}\frac{de}{dt} + \frac{1}{\mathrm{J}}\left(\frac{de}{dv} + p\right)\frac{\frac{dp}{dt}}{-\frac{dp}{dv}} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (11).$$

From the first of these equations we infer that with a complete knowledge of the mechanical energy of a particular fluid, we have enough of data for determining for every state, its thermal capacity in constant volume. From equation (9) we infer, that with, besides, a knowledge of the pressure for all volumes and a particular temperature, or for all volumes and a particular series of temperatures, we have enough to determine completely the pressure, and consequently also, according to equation (11), to determine the two thermal capacities, for all states of the fluid.

92. For example, let these equations be applied to the case of a fluid subject to the gaseous laws. If we use for $\frac{de}{dv}$ its value derived from (9), in equation (10), we find

$$p = \frac{p_0 v_0}{v}(1+\mathrm{E}t) + \chi(v)\,\epsilon^{\frac{1}{\mathrm{J}}\int \mu\, dt} \quad . \quad . \quad . \quad . \quad . \quad . \quad (12),$$

where $\chi(v)$, denoting an arbitrary function of v, is used instead of $\psi(v) - \frac{p_0 v_0}{v}$. We conclude that the same expression for the mechanical energy holds for any fluid whose pressure is expressed by this equation, as for one subject to the gaseous laws. Again, by using for $\frac{de}{dt}$, and $\frac{de}{dv}$, their values derived from (9), in equation (11), we have

$$\mathrm{N} = \mathrm{N}_0 + \frac{1}{\mathrm{J}} p_0 v_0 \log\frac{v}{v_0}\, d\,\frac{\left\{\frac{\mathrm{JE}}{\mu} - (1+\mathrm{E}t)\right\}}{dt} \quad . \quad . \quad . \quad . \quad (13),$$

$$\mathrm{K} = \mathrm{N}_0 + \frac{1}{\mathrm{J}} p_0 v_0 \log\frac{v}{v_0}\, d\,\frac{\left\{\frac{\mathrm{JE}}{\mu} - (1+\mathrm{E}t)\right\}}{dt} + \frac{\mathrm{E}^2 p_0 v_0}{\mu(1+\mathrm{E}t)} \quad . \quad . \quad (14).$$

The first of these equations shews that, unless MAYER's hypothesis be true, there is a difference in the thermal capacities in constant volume, of the same gas at the same temperatures for different densities, proportional in amount to the difference of the logarithms of the densities. The second compared with the first, leads to an expression for the difference between the thermal capacities of a gas in constant volume, and under constant pressure, agreeing with results arrived at formerly. (Account of CARNOT's Theory, Appendix iii., and Dyn. Th. of Heat, § 48.)

93. It may be, that more or less information, regarding explicitly the pressure and thermal capacities of the fluid, may have been had as the data for determining the mechanical energy; but these converse deductions are still interesting, as shewing how much information regarding its physical properties, is comprehended in a knowledge of the mechanical energy of a fluid mass, and how useful a table of the values of this function for different temperatures and volumes, or an Empirical Function of two variables expressing it, would be, whatever be the experimental

data from which it is deduced. It is not improbable that such a Table or Empirical Function, and a similar representation of the pressure, may be found to be the most convenient expression for results of complete observations on the compressibility, the law of expansion by heat, and the thermal capacities of a vapour or gas.

94. The principles brought forward in a former communication "On a Means of discovering experimentally, &c." (which is now referred to as Part IV. of a series of papers on the Dynamical Theory of Heat), may be expressed in a more convenient, and in a somewhat more comprehensive manner than in the formulæ contained in that paper, by introducing the notations and principles which form the subject of the present communication. Thus, let t be the temperature, and u the volume of a pound, of air flowing gently in a pipe (under very high pressure it may be) towards a very narrow passage (a nearly closed stopcock, for instance), and let p be its pressure. Let t', u', and p' be the corresponding qualities of the air, flowing gently through a continuation of the pipe, after having passed the "rapids" in and near the narrow passage. Let Q be the quantity of heat (which, according to circumstances, may be positive, zero, or negative) emitted by a pound of air during its whole passage from the former locality through the narrow passage, to the latter; and let S denote the mechanical value of the sound emitted from the "rapids." The only other external mechanical effect, besides these two, produced by the air, is the excess (which, according to circumstances, may be negative, zero, or positive) of the work done by the air in pressing out through the second part of the pipe above that spent in pressing it in through the first; the amount of which, for each pound of air that passes, is of course $p' u' - p u$. Hence, the whole mechanical value of the effects produced externally by each pound of the air, from its own mechanical energy, is

$$\mathrm{J}\,\mathrm{Q} + \mathrm{S} + p' u' - p u, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15).$$

Hence, if $\phi\,(v, t)$ denote the value of e expressed as a function of the independent variables, v and t; so that $\phi\,(u, t)$ may express the mechanical energy of a pound of air before, and $\phi\,(u', t')$ the mechanical energy of a pound of air after, passing the rapids; we have

$$\phi\,(u', t') = \phi\,(v, t) - \{\mathrm{J}\,\mathrm{Q} + \mathrm{S} + p' u' - p u\} \quad . \quad . \quad . \quad . \quad (16).$$

95. If the circumstances be arranged (as is always possible), so as to prevent the air from experiencing either gain or loss of heat by conduction through the pipe and stopcock, we shall have $\mathrm{Q} = 0$; and if (as is perhaps also possible) only a mechanically inappreciable amount of sound be allowed to escape, we may take $\mathrm{S} = 0$. Then the preceding equation becomes

$$\phi\,(u', t') = \phi\,(u, t) - (p' u' - p u) \quad . \quad . \quad . \quad . \quad . \quad (17).$$

If by experimenting in such circumstances it be found that t' does not differ sensibly from t, MAYER's hypothesis is verified for air at the temperature t, and, as $p'u'$ would then be equal to pu, according to BOYLE and MARIOTTE's law, we should have

$$\phi(u', t) = \phi(u, t)$$

which is in fact the expression of MAYER's hypothesis, in terms of the notation for mechanical energy introduced in this paper. If, on the other hand, t' be found to differ from t;* let values of p, p', t, and t' be observed in various experiments of this kind, and, from the known laws of density of air, let u and u' be calculated. We then have, by an application of (13), to the results of each experiment, an equation shewing the difference between the mechanical energies of a pound of air in two particular specified states as to temperature and density. All the particular equations thus obtained, may be used towards forming, or for correcting, a table of the values of the mechanical energy of a mass of air, at various temperatures and densities.

96. If, according to the plan proposed in my former communication (§ 72), the air, on leaving the narrow passage, be made to pass through a spiral pipe immersed in water in a calorimetrical apparatus, and be so brought back exactly to the primitive temperature t, we should have, according to BOYLE's and MARIOTTE's law, $p'u' - pu = 0$; and if H denote the value of Q, in this particular case (or the quantity of heat measured by means of the calorimetric apparatus), the general equation (16) takes the form,

$$\phi(u', t) = \phi(u, t) - (\mathrm{J\,H} + \mathrm{S}) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18).$$

If in this we neglect S, as probably insensible, and if we substitute for $\phi(u, t)$ and $\phi(u', t)$ expressions deduced from (9), we find,

$$\mathrm{H} = \left\{ \frac{1}{\mathrm{J}} - \frac{\mathrm{E}}{\mu(1 + \mathrm{E}t)} \right\} p u \log \frac{u'}{u} \quad . \quad . \quad . \quad . \quad . \quad . \quad (19),$$

which agrees exactly with the expression obtained by a synthetical process, founded on the same principles, in my former communication (§ 76).

* If the values of μ I have used formerly be correct, t' would be less than t, for all cases in which t is lower than about 30° cent.; but on the contrary, if t be considerably above 30° cent., t' would be found to exceed t. (See Account of CARNOT's Theory, Appendix II.) It may be shewn, that if they are correct, air at the temperature 0° forced up with a pressure of ten atmospheres towards a small orifice, and expanding through it to the atmospheric pressure, would go down in temperature by about 4°·4; but that if it had the temperature of 100° in approaching the orifice, it would leave at a temperature about 5°·2 higher; provided that in each case there is no appreciable expenditure of mechanical energy on sound.

XXXII.—*On two New Processes for the detection of Fluorine when accompanied by Silica; and on the presence of Fluorine in Granite, Trap, and other Igneous Rocks, and in the Ashes of Recent and Fossil Plants.* By GEORGE WILSON, M.D.

(Read April 19, 1852.)

In several communications made to this Society and to the British Association, I have announced the results of a series of observations on the distribution of Fluorine throughout the mineral, vegetable, and animal kingdoms. To myself, the least satisfactory part of these investigations has been the inquiry into the presence of fluorine in plants, for I have been more frequently foiled than successful in my attempts to detect it in them. Others have not, apparently, been more successful. DAUBENY was as unable as SPRENGEL at an earlier period had been, to obtain evidence that the element under notice is present in vegetable structures; and WILL of Giessen, the discoverer of fluorine in plants, speaks only of "traces" of it having been detected in barley. Later observers have not spoken more confidently concerning its abundance in vegetables; and in the many analyses of the ashes of plants which have recently been published, it seldom, if ever, finds a place.

That one cause of this apparent rarity of fluorine in vegetables, is the small extent to which it occurs in them is certain; but I have never doubted that the chief reason why it appeared to be so scanty a constituent of plants, was its occurrence along with silica, which makes its recognition very difficult. I had given up, accordingly, all hopes of satisfactorily demonstrating its wide distribution, till better processes than are at present in use, were devised for its detection when accompanied by silica.

For the same reason I have thought it hitherto useless to endeavour to trace back fluorine from the plants, animals, natural waters, and more accessible strata which are the main seats of life at the present day, to those earlier rocks and geological formations which have furnished our soils, and have contributed the chief soluble matters which are found in the lakes, rivers, and seas of the globe. The more ancient rocks abound in silica, and, with our present processes, the prospect of discovering fluorine in trap and similar siliceous masses, was not encouraging. A representation, however, from Professor JAMESON, as to the importance attaching to the detection of fluorine in the most ancient rocks, led me to reconsider the geological and mineralogical interest which the inquiry possessed; and within the last six weeks I have put in practice two methods of investigation, which I shall now explain.

The processes at present in use for the separation of fluorine from silica, are in many respects satisfactory; but they imply the rejection of glass apparatus, and the use of vessels of platina, which, from their costliness, cannot be employed of any considerable size, and, from their opacity, render the observation of phenomena occurring within them impossible. They are thus inadmissible for operations where large quantities of material must be dealt with; and to the impossibility of employing glass and porcelain vessels, must be largely attributed the comparatively limited extent of our information as to the distribution of fluorine.

The following processes, which, in the meanwhile, are offered only as qualitative (although I hope to succeed in rendering the second of them quantitative), may be carried on in the ordinary glass and porcelain vessels of the laboratory, and admit of everything visible being observed. They are applicable to all siliceous compounds or mixtures containing fluorine, provided it be present in the form of a fluoride which admits of decomposition by oil of vitriol at its boiling point. The first stage of the process consists, in both cases, in heating the silicated fluoride in a flask along with strong sulphuric acid, so as to occasion the evolution of the fluoride of silicon, $Si F_3$. This gas is conducted by a bent tube into water, where it deposits a portion of gelatinous silica; and the liquid, after filtration (which, however, is not essential), is treated as follows:—

In the first process, I adopted one of Berzelius' well-known methods for the isolation of silicon. The filtered liquid was neutralised with potass: and the resulting gelatinous precipitate of fluoride of silicon and potassium ($2 Si F_3 + 3 KF$), after being washed, was dried, and transferred to a small metallic crucible, in which it was heated with potassium, so as to separate and set free the silicon, and convert the whole of the fluorine into fluoride of potassium. This fluoride was then dissolved out by water, evaporated to dryness, and treated in the ordinary way with oil of vitriol, so as to evolve hydrofluoric acid, which could be made to record its evolution by the etching which its vapour occasioned on a plate of waxed glass, with lines written on it through the wax.

This process is necessarily tedious, and is liable to several objections. The most serious of these is the impossibility of effecting the complete decomposition of the fluoride of silicon and potassium, by potassium, so as to liberate the whole of the silicon; and the risk of the latter undergoing oxidation into silica during the washing of the ignited mass. Accordingly, though this method gives good results, and has enabled me to detect fluorine in coal, in which I could not previously detect more than the faintest traces of it, yet it almost unavoidably necessitates a loss of the element in question, and is much inferior in simplicity and certainty to the process which I am about to describe.

In the second process, as in the first, the substance under examination is heated with oil of vitriol so as to yield fluoride of silicon, which is conducted into water. The resulting solution (with or without filtration) is neutralised with

ammonia instead of potass, and then evaporated to dryness, which has the effect of rendering the silica produced insoluble. On digesting water on the residue, fluoride of ammonium is dissolved, and the solution requires only to be evaporated to dryness and moistened with sulphuric acid to give off hydrofluoric acid, which readily etches glass. The stages in the ammonia process are thus :—

1*st*, Distillation of the substance with oil of vitriol, so as to produce fluoride of silicon, $Si\,F_3$.

2*d*, Neutralisation of the aqueous solution of the distillate, with ammonia in excess, so as to produce fluoride of silicon and ammonium, $2\,SiF_3 + 3\,NH_4F$.

3*d*, Evaporation of the neutralised liquid to dryness, so as to separate silica, and render it insoluble.

4*th*, Exhaustion of the residue with water, and evaporation to dryness, so as to leave fluoride of ammonium.

5*th*, Moistening of the ammonio-fluoride with oil of vitriol, so as to liberate hydrofluoric acid; which will act upon glass.

I have tried this process with Aberdeen and Peterhead granite; with three trap rocks from the neighbourhood of Edinburgh, namely, basalt from Arthur Seat, greenstone from Corstorphine Hill, and clinkstone from Blackford Hill; with a deposit from the boiler of the Atlantic steamer, Canada; with a fossil bone; with the ashes of charcoal, of barley-straw, and of hay; and in all with such success that the applicability of the process to the end proposed is certain. The pieces of glass, etched by hydrofluoric acid evolved from the substances referred to, which I lay upon the table, are not selected successful specimens, but represent the whole of the trials made by the ammonia process. The etchings on the majority of them are as deep as could be obtained from pure fluorspar and oil of vitriol; and, with the experience which I have now acquired, I have no doubt that I shall be more successful in succeeding trials with vegetable ashes, which, for reasons to be presently mentioned, require more precautions than fragments of rock do.

The examination of a hard crystalline mineral, such as granite, or an unweathered trap, presents no difficulties. It must be reduced to a tolerably fine powder, and employed in considerable quantity. A little sulphurous acid is always evolved during the action of the oil of vitriol, from the dust which is gathered during a protracted process of powdering; but the presence of this acid in small quantity is of no importance, and the powdering of the rock is the most troublesome part of the investigation.

It is otherwise with weathered granite and trap, which contain chlorides and carbonates, and give off hydrochloric and carbonic acids when treated with sulphuric acid. These gaseous acids materially interfere with the processes described by the frothing which they occasion, and by their tendency to sweep away the hydrofluoric acid which may accompany them. In my earlier trials, accordingly,

I treated the powdered pieces of rock with hydrochloric acid, and washed them with water, then dried them, and heated them with oil of vitriol. The preliminary treatment, however, risked, and I have no doubt occasioned, the loss of the fluorides present in the mineral, which were soluble in water or in hydrochloric acid; and latterly I abandoned this process. I refer to it here only because it explains certain of the less perfect etchings which are exhibited.

In later trials, a simpler and more satisfactory process has been put in practice. The powdered rock has been added to oil of vitriol in the cold, in small quantities at a time, so as to prevent any great rise in temperature. So long as the heat evolved is not considerable, there is no risk of fluorine escaping, either as hydrofluoric acid or as fluoride of silicon, whilst any chlorides or carbonates present are decomposed, and the hydrochloric and carbonic acids evolved, are carried away, before their escape can interfere with the evolution of fluorine. When the oil of vitriol is afterwards raised to its boiling-point, the fluoride of silicon is liberated, and little difficulty attends its collection and identification.

The ashes of plants are somewhat less easily examined. They almost invariably contain charcoal, which occasions the evolution of sulphurous acid with hot oil of vitriol. Sulphurous acid, however, does not very materially interfere with the detection of fluorine, as it can be expelled by heating the distillate before adding ammonia, which is the process I have hitherto generally followed. It may also be converted into sulphuric acid by the cautious addition of nitric acid, and then its presence is quite immaterial. But in several quite successful trials no steps were adopted to separate the sulphurous acid.

The specimen laid upon the table, of glass etched by fluorine from barley-straw, will illustrate the applicability of the process to plant-ashes largely charged with silica, and which yielded, with oil of vitriol, carbonic and hydrochloric acid, besides much sulphurous acid.

The glass etched by the fluorine of charcoal-ashes is still more deeply corroded, although they were subjected to no preliminary process to remove the volatile acids which they contained, or to set free or separate the sulphurous acid which they yielded.

In truth, the ammonia process has succeeded with every substance upon which I have tried it. The worst result has been with the ashes of hay, but they had been washed with water and hydrochloric acid to remove chlorides and carbonates; and in former papers I have shewn that such washings remove fluorides. Notwithstanding this, the evidence of the presence of fluorine in hay, afforded by the specimen, is such as has not hitherto (so far as I am aware) been afforded by any analyst, and the omission of the washings will, I have no doubt, yield a still more satisfactory result on a repetition of the analysis. The same remark applies to coal-ashes, by the fluorine of which I have only one etching to shew. It is not a favourable specimen; the ashes were washed with a considerable volume of

hydrochloric acid and water; the product of distillation was tested by the less perfect potassium-process; and the lines etched by the hydrofluoric acid were drawn too fine. Experience has taught my assistants that the wax should be spread thin, and the lines through it be made with a broad point, if a distinct etching is to be obtained. But, withal, the results with coal-ashes are sufficiently marked.

I have further tested the sufficiency of the ammonia process in the following stringent way. A fossil bone from the Himálayas, which I had already ascertained to contain a fluoride, and which was full of crystals of carbonate of lime, was reduced to powder, and mixed with powdered glass so as to add to it excess of silica. It was then subjected to the ammonia process, and has yielded an etching as deep as the purest fluorspar could have given with oil of vitriol.

The result is so marked, that I should recommend the deliberate addition of silica to bodies suspected to contain fluorine, as a provision for permitting such substances to be analysed in *glass* vessels, in which the largest quantities may be subjected to examination without risk of missing the element in search, or permitting it to escape.

Five points call for further notice.

1*st*, When a silicated fluoride, as I may, for the sake of brevity, call it, is distilled with oil of vitriol, the whole of the fluoride of silicon comes away as gas, as soon as the oil of vitriol has reached its boiling-point. It is not necessary, accordingly, to subject a body supposed to contain fluorine to any lengthened ebullition; and, in the case of plant-ashes, it is desirable to arrest the boiling as soon as all the fluorine has been evolved, for protracted ebullition only occasions evolution of sulphurous acid. Besides the ultimate glass-etching, the escape of fluorine is rendered manifest by the appearance of a white gelatinous body in the water, through which the gas evolved ($Si F_3$) is passed; and by the production of a gelatinous, flocculent precipitate, when the solution of this gas is neutralised with potass. The coal-ashes gave all those results.

2*d*, It appears exceedingly probable, that much of the silica occurring in the forms of quartz, chalcedony, opal, sinter, and the like, which is generally supposed to have been deposited from aqueous or alkaline solution, has owed its origin to the decomposition of fluoride of silicon by water, or has otherwise been related to fluorine as its solvent or transferring agent. This, or rather the less precise notion of fluorine conveying silica, has been suggested by my friend Mr A. Bryson, and by Dr H. Buchanan, E.I.C.S.

3*d*, The occurrence of fluorspar in drusy cavities in greenstone, along with silica, as in the specimens obtained from Bishopton, on the Clyde; the similar occurrence of apophyllite in the cavities of trap; the association of topaz, pycnite, lepidolite, and most of the other compound fluorides, with granite, gneiss,

and mica slate, will acquire additional significance from the discovery that fluorine occurs in the rocks which form their matrices.

4th, The presence of fluorine in plants is now rendered doubly probable, as it may enter them alike in combination with a metal such as potassium, sodium, or calcium, or in association with silica.

5th, The presence of fluorine in animals may now be fully accounted for; as it not only enters their bodies in the water they drink, but is contained in the vegetable food, by which, directly or indirectly, the whole animal kingdom is sustained. The prosecution of these views, however, will be taken up in succeeding papers.

BRITISH MUSEUM
HISTORY

AT THE CAPE OF GOOD HOPE, JULY 1845

XXXIII.—*Contributions to a Knowledge of the Phenomena of the Zodiacal Light.*

By Professor C. Piazzi Smyth.

(Read February 7, 1848.)

When preparing to make a night journey over one of the plains of South Africa, in the month of June 1843, a friend called my attention to the peculiar appearance of the sky in the west, as offering a very decided proof, "agreeably with theory," that there was no "Solar atmosphere" to be seen at that season of the year.

On looking in the direction mentioned, the last portion of the twilight was just visible, and forming a peculiarly level line above the place where the sun had set, for an extent in azimuth, of perhaps 40°, and at a height of about 5°. All the gorgeous colours which had attended the setting of the sun had long since vanished, and there only remained sufficient light within the flattened arc described, to make the space included between it and the horizon appear *light* blue, while all the rest of the sky had attained a deeper colour, nay was almost black, and thickly spangled with small as well as large stars.

There most decidedly was not then any symptom of the so-called "way of the twilight shooting upward." But as soon as the last illuminated portion of the western sky had set, the phenomenon, *i. e.*, the zodiacal light, appeared in an unmistakeable manner, rising up in the ecliptic, to a height of 50°, with a breadth of perhaps 12° at the horizon; and forming, with the vast extent of its illuminated surface, and the regularity of its contour, one of the most remarkable objects in the starry sky. The form was that usually described, viz., a wedge pointing upwards, with curved sides, and of excessively indefinite outline; but still, as far as could be judged, free from any irregularities; while the light, which was more delicate and transparent than that of the milky way, increased in intensity transversely from either side to the central longitudinal axis; and augmented also in the axis, from the apex downwards, until overpowered by the haze on the horizon.

Now, two circumstances worthy of notice, were pointed out by this night's observation, *firstly*, that persons did not always know exactly when to look for the zodiacal light, nor what sort of object to expect; and, *secondly*, that the theory was greatly in error; and, for an astronomical matter, grievously wrong. Both classes of mistakes may have been brought about in no small degree by the injudicious mixing up of erroneous theoretical and speculative views with the simple nomenclature of the phenomenon. All that can be asserted from a single observation, and, perhaps indeed, from all the observations that have been made, up to the present time, is that a light appears in the zodiac, and if it be called "the

zodiacal light," no idea except the visible fact itself is included. But to call it the "Sun's atmosphere," is taking for granted a supposed fact which has never been proved, and is imagining the body to obey peculiar laws, to which it may not really be subject. Moreover, as in the case of a phenomenon which is so extremely faint as scarcely to be seen at all, a person may too easily persuade himself that he sees it as he *ought* to see it,—so there seemed to be much necessity for making further observations, which though they might prove, after all, to be not entirely free from errors of judgment and idea, yet would probably not be affected in the same way as those of other observers.

The circumstances in which I was placed were very favourable, so far as the clearness of the sky, the purity of the atmosphere, and the advantage of geographical position were concerned; but being engaged in the active duties of a trigonometrical survey, sometimes on the top of high mountains and sometimes in the plains below, the different character of the stations exercised too great an influence on the phenomenon to be observed, to allow of strict comparison being made between the observations at the various places. But there was at least the possibility of being able to determine a good method of making the observations, by endeavouring to reduce to practice some plan by which the results should be expressed more in numbers than has generally been the case; and of ascertaining at least the degree of accuracy with which observations of place, *i. e.*, of AR. and Decl., could be made, in more or less favourable localities and seasons.

I proposed to myself, therefore, to endeavour to determine each night the AR. and Decl. of the apex of the light; though the only method which was then available, viz., observing the particular stars amongst which the point was situated, was only of use when there were large stars close by; since even if there had been star-maps to refer to in the desert, to identify the smaller ones, the phenomenon to be observed was not one that would bear close and direct investigation. It was only after having shut the eyes for some little time, or having turned them to some dark part of the sky or earth, that on suddenly directing them to the region of the zodiacal light, but not exactly to the middle of it,—it was seen of a well-defined figure; for by looking straight at it, and still more by coming into contact with any artificial light, the situation of the apex appeared to vary many degrees, or could not be decided on at all. At length, therefore, in 1844, I made a little wooden instrument with equatorial motion, plain sights, and roughly divided circles; which being placed in position in some spot free from any artificial light, readily gave the means of determining the object sought. Then, by the sort of side glance above described, a good notion of the position of the apex being obtained, the plain sights were immediately pointed to the spot, the circles read off, and their index error obtained by reference to known stars on either side. This was usually done two or three times each night, and the mean has been entered in the accompanying table as a single observation.

Observations of the Zodiacal Light in Latitude 33° South, and Longitude 1h 14m West.

Date.	Place of Observation.	Eastern apex of Z. L.		Sun's		Western apex.		Eastern elongation.	Western elong.	Remarks on	
		AR.	D.	AR.	D.	AR.	D.			Eastern or following portion of Z. L., or that seen in the morning.	Western or preceding portion of Z. L., or that seen in the evening.
1843.		h. m.	°	h. m.	°	h. m.	°	°	°		
June, . . .	Plain, Ft.	...	...	...	...	...	...	...	...	Very conspicuous.	Seen, but not well.
July, ...	Mountain, 1500	...	...	...	...	...	...	...	...	Do.	
August, . . .	Plain, . .	...	...	...	...	...	...	...	...	Do.	Not seen in the morning, though looked for.
September, . .	Mountain, 2000	...	...	...	...	...	...	...	...	Seen in first part of month.	
1844.											
February, . .	Plain, . .	...	...	...	...	...	...	...	...	...	Nothing visible.
May, ...	Mountain, 3500	...	...	...	...	...	...	...	...	...	Very strong, long, narrow, and bright.
July, ...	Do. 6500	...	...	...	...	...	...	...	...	Very bright.	
August and Sept.,	...	...	...	...	...	...	...	...	...	Do.	
Oct. 2. at 7h P.M.	...	16 15	−24	12 34	− 4	...	...	56			
... 5. at 7h 50m P.M. .	...	16 18	−24	12 45	− 5	...	...	53			
... 10. at 7h 50m P.M. .	...	16 20	−22	13 4	− 7	...	...	50			
December, . .	Do. 4500	...	...	...	...	...	...	...	...	Very doubtful, but a quantity of light in the west long after the time for twilight to cease; the ill-defined apex being observed on Dec. 5, at 9h P.M., had AR. 20h 50m D − 36°.	Not visible.
1845.											
February, . .	Do. 4500	...	...	...	...	...	...	...	...	Not visible.	Well seen in the east, latter part of this month.
March and April,	Do. ...	...	...	...	...	...	...	...	...	Do.	Very strong.
May, ...	Do. ...	...	...	...	...	...	...	...	...	Seen towards the end of the month.	Do.
June 5 at 6h 46m	Do. ...	9 25	+12	5 3	+23	...	...	...			
... at 17h 40m	Do. ...	...	...	...	...	1 21	+17	66	55		
July 6 at 7h .	Do. ...	12 27	+ 6	7 2	+23	...	...	...			
Do. do. . .	Do. ...	...	...	...	...	3 29	+12	79	53		
Sept. 21 at 8h .	Plain, . .	16 30	−23	11 54	+ 1	...	...	69			
Oct. 3. at 8h, 8h 30m, and 9h,	Do.	17 0?	−22?	12 37	− 4	...	...	66	...	Apex of the zodiacal light concealed in the milky way; besides which the brightness and position of Venus render the observations very difficult.	

A few days after the last observation I left the Cape, and the passage thence by St Helena through the tropics, was so uniformly cloudy, that I was unable to obtain another satisfactory look at the zodiacal light, and ever since my residence in Edinburgh in the middle of a city glowing at night with gas, and reeking with smoke, and under a sky but rarely clear, and when it is so, not unfrequently illuminated by the Aurora Borealis, I have been equally unfortunate.

To be able to make a good observation of the zodiacal light, the sky should be quite free from clouds, the air pure and transparent; not the slightest vestige of the twilight should remain, the milky way should be far from the neighbourhood, the moon should not be visible, or the brighter planets, such as Venus and Jupiter. If these circumstances be secured, and a person look out at that period of the year, as hereinafter detailed, when the ecliptic makes a large angle with the horizon of the place of observation, he can hardly fail to see the phenomenon in the most marked degree. A beginner must be especially cautioned not to begin looking too soon in the twilight to discern the "Sun's atmosphere," under the idea, happily, of catching it before all traces of the sun's light on the horizon are completely gone; and he should also be forewarned of the immense influence which climate and geographical position have on the visibility and apparently on the form and size of the phenomenon. Thus, in 56° N. Lat., and still less further north, even were the elongation of the light E. and W., equal in every respect, it would still never appear equally visible, and would but seldom be seen either way. In the summer the twilight would render the sight impossible, and in winter, the sun's path is too low and oblique. In the spring evenings, the light would be well seen, because then the twilight is of a moderate length, and the zodiacal light rises at an angle to the horizon of 30° greater than the equator, and therefore does not set till long after the twilight has disappeared; but as the other end rises in the morning at that season, necessarily at an angle 30° less than that of the equator, the apex in its standing position, hardly rises above the mists of the horizon before the twilight illumines the sky. In the month of September matters are just reversed, and the zodiacal light rising in the morning at a greater angle than the equator, is then well seen, but is not at all visible in the evening, when, from the standing position of the body, the apex sets very soon after the sun. And in these two short appropriate seasons, so many of the nights may be rendered untoward by clouds, strong moonlight, and other causes, that an opportunity of seeing the zodiacal light may even then but very rarely be enjoyed.

Similarly in the southern hemisphere in 56° S. Lat., supposing also, as before, that the zodiacal light stretches out equally from the sun on every side in the plane of his equator, the two most favourable opportunities in the course of the year for viewing the body would be, in the evening in the month of September, and in the morning in the month of March. And that this would really be the case, the observations made in Lat. 33 S. sufficiently attest.

In order to give a clearer description of what may be expected to be seen, than can be conveyed in words alone, I have subjoined a number of drawings,* both of what the zodiacal light is, and what it is not, the latter being the great comet of 1668 and 1843, mistaken on both occasions for the more permanent members of the system. The object in the construction of the drawings has been, in so far as it was possible to be compassed by the small skill of the author, to give so complete a reproduction of all the attendant phenomena, and circumstances of climate or country, as to enable any one who looks at them, to form a tolerable idea, whether any of the accompanying conditions under which the original view was obtained, were likely to produce an erroneous judgment in the spectator, of the exact form and appearance of the zodiacal light. A larger portion of the landscape has therefore been introduced, than would otherwise have been altogether appropriate in astronomical drawings.

A more important addition is, however, that which I was advised to make by my friend Mr W. A. Cadell, viz., the insertion in the margin of the circles of Right Ascension and Declination, which shew what particular projection has been employed, and serve to identify the stars, fix the latitude in which the observations were made, the time at the instant, and to give an idea of the dimensions of the body under discussion, and the region of the sky in which it is found.

To represent the eastern or western portion of the sky, in their perfection, or as would be thrown on paper by the camera lucida, as Mr Cadell has shewn, the horizon should be defined by a straight line in the picture, and the E. or W. point must be in the middle of that line; then the eye of the spectator being directed toward it as such, the equator, being a great circle, will be represented by a straight line drawn through that point, and rising at an angle to the horizon, equal to the latitude of the place; and the meridian lines on the parallels of right ascension, being also great circles, must be expressed by straight lines crossing the equator at right angles; on the other hand, the parallels of declination being small circles, will appear as conoidal curves. A great circle becomes a straight line on the picture, since it is a plane passing through the eye, and the common section of this plane with the planes of the picture, is a straight line. A small circle is a conoidal curve on the picture, because a small circle is seen as a cone of which the apex is at the eye, and the common section of this cone with the plane of the picture is a conic section. The form of the conic section will vary as the inclination of the cone's axis to the plane of the picture varies.

In all the drawings given herewith, the line of sight is seldom directed exactly to the E. or W. points, but generally between them and the northern or (point of culmination for the southern hemisphere.) Were the spectator to face the

* On account of the expense of first-class engravings, one of the drawings only has been put upon metal. This one, "the appearance of the zodiacal light at the Cape of Good Hope, in July 1845," will be found amongst the plates at the end of the volume.

northern point exactly, then the equator would be represented by a straight line, parallel with the horizon, and elevated above it at an angle equal to the colatitude of the place. And, according to the degree to which the spectator turns round from the E. or W. points towards the north, the inclination of the equatorial line will vary, from the angle of the latitude of the place, to perfect horizontality.

This varied inclination of the equator has been strictly attended to in all the accompanying cases; but it has been found advisable for simplicity in practice, to represent the declination circles also by straight lines, for there is hardly any sensible difference caused thereby in the central region of the picture, where all the important part of the subject to be delineated, lies; and although the configuration of stars near the borders might not be such as would exactly appear to the eye of a spectator, or as they should be represented by the usual rules of perspective, still the amount of discordance is so extremely small, that the unassisted eye would hardly perceive it; and, what is more important than having a representation perfectly similar, in the minutest particular, to that found in the retina,—the particular projection of the sphere which was actually employed, being inserted in the margin, gives just as good, and rather simpler, means, than would have been available on the other plan, for the identification of the stars.

The application of instrumental measurement, hereinbefore described, to determine the phenomena of the zodiacal light, is believed to be new; and the observations so obtained, seem to shew very decidedly, especially those of October 1844, that numerical measures of the place of the apex of the light may easily be obtained,—with a probable error of not more than two degrees: so that vague estimations and notes of mere ideas should not be allowed to form the data in this particular branch of astronomy any longer.

The general results to be deduced from the data given in the Table, are,

1*st*, That the zodiacal light is a body of a lenticular form, spread out nearly in the plane of the earth's orbit, and extending almost equally from the sun in every direction. Were the ordinary European observations made about the time of the spring equinox the only ones existing, we should merely be entitled to conclude the existence of a one-lobed projection from the sun; but when we combine therewith the Cape observations, we find that the body is seen all through the year, and on both sides of the sun, of pretty nearly the same size and shape, viz., a curvilinear-sided wedge, in which the light continually increases from the borders towards the centre of the base, or the actual position of the sun; appearances which can only be satisfied by a lenticular body seen in section.

2*d*, The zodiacal light is proved to be excentrically disposed about the sun, by the elongations observed east and west on the same day being different; shewing, indeed, at various times an excentricity of from $\frac{1}{20}$ to $\frac{1}{10}$.

3*d*, The zodiacal light may also be considered to rotate about the sun, and to be brighter in some parts than in others; because it is observed to be of different lengths and degrees of brightness at corresponding periods in successive years. For

although such an effect might be produced by a periodical alteration in the size and general lustre of the body, still the supposition of such a rapid material change in so large a member of the solar system, is extremely improbable: whilst this body's revolving may be held to be necessary according to the principles of gravitation; for otherwise the component particles would speedily fall into the sun; and that some portions are brighter than others, follows partly as a consequence of the observed unequal size of the two sides. Were the light stationary, then the greatest and least lengths and brightnesses should occur at the same time in successive years, because on arriving again at the same points of its orbit, the earth should again see the same parts of the zodiacal light pointing to the same direction in space; but, as already stated, the contrary to this has been observed.

The greatest elongation observed was 79°, and the least 50°, but from the varying circumstances and positions in which the observations were made, the short period of time over which they extend, together with the small number of favourable opportunities, and the distance of the place from the equator, which these conditions afforded, no numerical results of much accuracy can be derived from them alone; but some advantage may be gained by comparing them with the results of former observers.

The number, however, of these, *i. e.*, of actual observers, is comparatively small, and they are all very recent; for, strange to say, no notice of the zodiacal light is found amongst the writings of astronomers or natural philosophers until 1661. And indeed, when we consider that this phenomenon may be generally described as a broad and tall light seen in the western sky after sunset, and in the eastern before sunrise, with a length of about 60°, a breadth of 20°, combining with a brightness nearly equal to that of the milky way, a regular mathematical figure, which makes it far more remarkable, and rising, as it does, at a greater angle to the horizon, so as to be better seen in countries nearer the equator than ourselves, and being probably of as great antiquity as the sun itself,—truly it is astonishing that all these notabilia should have been passed over in the earlier ages of the world, when civilization flourished more to the south, and the men of ancient Athens and Babylon lived under a clear sky, in a genial climate, which invited rather than forbade the contemplation of the firmament by night. It remained, however, for the inhabitants of these cloud-vexed northern islands to be the first to take notice of the phenomenon, and so supply another instance of the indomitable perseverance of an iron race overcoming all the untoward obstacles of an unpropitious position, and rising superior to other races revelling in the most luxurious advantages of nature.

Claims have been put up for Kepler and Descartes, as being the original discoverers of the zodiacal light; but the passages in their respective works*

* Kepler's Epit. Astron. Copernicanæ, t. i., p. 57; and t. ii., p. 893; Descartes, Principes, iii., Art. 136, and 137.

are so very meagre and obscure, that they require all the knowledge of the phenomenon acquired up to the present day, to be applied to make them mean anything. Mairan, with whose theory Kepler's fancy seems to agree, when discussing, in 1754, the history of the phenomenon, gives the German full credit; but Humboldt, in 1844, with different theoretical views, dismisses the case of his countryman in a very summary way.

An earlier claim still has been brought forward, on account of the mention, in a letter from Rothmann to Tycho Brahe, that in the spring the twilight ceased not till the sun was 24° below the horizon; and as the true twilight would have ceased long before the sun was so low,—it is contended that Rothmann must have seen the zodiacal light, though without remarking anything peculiar in it, or different from the ordinary course of the evening.

So that the first satisfactory and clear description is still that of Childrey in 1661, already alluded to. "There is another thing," says he, in his Britannia Baconica, p. 183, "which I recommend to the observation of mathematical men: which is, that in February, and for a little before and after that month (as I have observed for several years together), about six in the evening, when the twilight hath almost deserted the horizon, you shall see a plainly discernible way of the twilight, striking up towards the Pleiades, and seeming almost to touch them. It is so observed any clear night, but it is best *illâ nocte*. There is no such way to be observed at any other time of the year. But what the cause of it in nature should be, I cannot yet imagine, but leave it to further inquiry."

Here, then, is a clear and simple account of one phase of the phenomenon, marking it as a something unusual, as different from ordinary twilight, as constant in that anomalous difference, and therefore well worthy of being carefully inquired into.

In his Travels in Persia in 1668,* Chardin mentions having seen the tail of the great comet of that year above the western horizon after sunset; the head being visible only in the southern hemisphere. Cassini and Mairan, writing some years after, under the influence of the then new discovery of the zodiacal light, asserted that it must have been this which Chardin saw; and he is even made out by Delambre to have been the original discoverer of it. The comet of 1668 having, however, appeared again in 1843 (that is, they are supposed, with the greatest probability, to be identical; and if not identical, still they are at least both specimens of the comet genus), has given us the opportunity of determining whether Chardin's description applies to the zodiacal light or to the comet, which though so very unlike each other, were not only confounded at the former apparition, but at the latter also; when the tail, as before, was the only part visible in the northern hemisphere. The slightest glance at the accompanying drawings

* Edit. de Langles, t. iv., p. 326; and t. x., p. 97.

of the two objects, however, will probably convince every one, that CHARDIN's Persian expression "niazouk," or in French "petit lance," which was applied by the Persians to the phenomenon they saw, could only be considered as at all suitable in the case of the comet's tail.

In 1683, the subject was taken up by DOMINIC CASSINI, and to him belongs the merit of first scientifically investigating the laws of the phenomenon, determining its cosmical nature, and giving it the appropriate name of the zodiacal light. His series of observations, extending over nearly six years, is still unrivalled; and if he is not correct in all his conclusions, it is chiefly because his observations were almost entirely confined to his own high northern latitude; and were therefore affected to a great and unknown extent by circumstances of climate and geographical position. He had much wished to eliminate these effects by means of observations made in the southern hemisphere, but unfortunately was not able to obtain any; and indeed those which have been made by the author, and recorded in this paper, are perhaps the first which have been published and brought to bear on the theory of the subject.

CASSINI's conclusions were, that the zodiacal light is a flat luminous ring encircling the sun, nearly in the plane of his equator, and is therefore seen always more or less in profile, and perfectly so at two periods of the year, April and August, when like Saturn's ring, and for similar reasons, he supposed it to vanish to our sight; while the nonvisibility at *any* period between these two months, he considered to be produced mainly by the overpowering effect of the lengthened summer twilight. But these ideas, on being tested by the Cape observations, completely fall to the ground; for during the *whole* period of invisibility to CASSINI (caused in reality by the lengthened twilight of summer in his northern hemisphere), the phenomenon was most visible at the Cape, as winter then prevails in the southern hemisphere; and, indeed, the very reverse effect from that expected by CASSINI should follow, when a transparent and oblate luminous ring is viewed in profile, for it will then be seen at its brightest, on account of all the infinitely small light-giving particles being brought closer together; so small are they, that they can by no means be distinguished separately, or when thinly scattered over the sky, but only make themselves sensible to the eye, and the telescope, when they are crowded together in a smaller space. The idea, moreover, of the zodiacal light being in the form of a *ring* at all, is discountenanced by the observed appearances, they being all conformable to the phenomena which would be afforded by a thin *lenticular* body, excentrically situated and revolving about the sun.

CASSINI's friend, M. FATIO, made observations of the zodiacal light about the same time, as did also M. KIRCH, and EIMMART, and Mr DERHAM.

But the subject was not carried further, until taken up by MAIRAN, in 1731. He was rather wild in his notion of the manner in which the body was formed,

viz., by particles thrown off from the sun, in consequence of the rapidity of his rotation; nor was he very happy in his name of the "Sun's atmosphere," by which he led both himself and others to reason upon it, as if it were proved to be, and actually was of a kindred nature with the earth's atmosphere. His conclusions, however, that the whole of the luminous body was of a lenticular form, nearly in the plane of the earth's orbit, somewhat excentric with regard to the sun, and, indeed, with a rotation about that luminary, seem to be remarkably good. And his opinion, so far as the lenticular shape is concerned, is also held by Olbers and by Sir J. Herschel, both of them observers.

Olbers in a letter to Humboldt in 1833, says, "What you tell me of the changes of brightness in the zodiacal light, and the causes to which, in the tropics, you ascribe such variations, has excited my interest the more, because I have been for a long time past particularly attentive every spring to this phenomenon in our northern latitudes. I, too, have always believed the zodiacal light to rotate; but I assumed it (contrary to Poisson's opinion, which you communicate to me), to extend the whole way to the sun, increasing rapidly in intensity. The luminous circle which in total eclipses shews itself round the darkened sun, I have supposed to be this brightest portion of the zodiacal light. I have satisfied myself that the light is very different in different years, sometimes for several years being very bright and extended, and in other years scarcely perceptible. I have not myself been able to observe the sudden fluctuations in the light, probably on account of the faintness with which it appears to us in this part of the world. You are certainly right in ascribing the rapid variations in the light of celestial objects, which you have perceived in the climate of the tropics, to changes taking place in our atmosphere, and especially in its higher regions. This shews itself in a more striking manner in the tails of great comets. Often, and particularly in the clearest weather, pulsations in the tails of comets are seen to commence from the head or nucleus as the lowest part, and to run in one or two seconds through the whole extent of the tail, which, in consequence, appears to lengthen several degrees, and contract again. That these undulations, which engaged the attention of Robert Hooke, and in later times of Schröeter, and Chladni, *do not take place in the cometary tails themselves*, but are produced in our atmosphere, appears evident if we reflect that the several particles of these cometary tails (which are many millions of miles in length) are at *very different distances* from us, and that the light from them can only reach our eyes at intervals of times which differ several minutes from each other. I will not attempt to decide, whether what you saw on the banks of the Orinoco, not at intervals of seconds, but of minutes, were actual coruscations of the zodiacal light, or whether they belonged solely to the upper strata of our atmosphere. Nor can I explain the remarkable lightness of entire nights, or the anomalous increase and prolongation of this light in the year

1831, particularly if, as it has been said, the *lightest part* of these singular twilights did not coincide with the place of the sun below the horizon."

Sir JOHN HERSCHEL's views, published only five years ago, were called forth by the tail of the great comet of 1843 having been by some so pertinaciously mistaken for the zodiacal light.

"The zodiacal light," said he, "as its name imports, invariably appears in the zodiac, or, to speak more precisely, in the plane of the sun's equator, which is 7° inclined to the zodiac, and which plane, seen from the sun, intersects the ecliptic in longitude 78° and 258°, or so much in advance of the equinoctial points. In consequence, it is seen to the best advantage at, or a little after, the equinoxes, after sunset at the spring, and before sunrise at the autumnal equinox, not only because the direction of its apparent axis lies at those times more perpendicular to the horizon, but also because at those epochs we are approaching the situation in which it is seen most completely in section.

"At the vernal equinox, the appearance of the zodiacal light is that of a pretty broad pyramidal, or rather lenticular, body of light, which begins to be visible as soon as the twilight decays. It is very bright at its lower or broader part near the horizon, and (if there be broken clouds about) often appears like the glow of a distant conflagration, or of the rising moon, only less red; giving rise, in short, to amorphous masses of light, such as have been noticed by some as possibly appertaining to the comet. At higher altitudes its light fades gradually, and is seldom traceable much beyond the Pleiades, which it usually however attains and involves; and (what is most to my present purpose) its axis at the vernal equinox is always inclined (to the nortward of the equator) at an angle of between 60° and 70° to the horizon; and it is most luminous at its base, resting on the horizon, where also it is broadest, occupying, in fact, an angular breadth of somewhere about 10° or 12° in ordinary clear weather."

The ring hypothesis of CASSINI has, however, been followed in a greater or less degree, by LA PLACE, SCHERBERT, and POISSON, as well as by HUMBOLDT, who is an observer, and publishing in 1844 is the latest of all the authorities.

His description of the general appearance of the light is most vivid and truthful, and can perhaps only be fully appreciated by those who have seen it under similar favourable circumstances.

"Those who have dwelt long," says he, "in the zone of Palms, must retain a pleasing remembrance of the mild radiance of this phenomenon, which, rising pyramidally, illumines a portion of the unvarying length of the tropical nights. I have seen it occasionally shine with a brightness greater than that of the milky way, near the constellation of Sagittarius; and this not only in the dry and highly rarified atmosphere of the summits of the Andes, at elevations of thirteen to fifteen thousand feet, but also in the boundless grassy plains or *llanos* of Venezuela, and on the sea-coast under the ever-clear sky of Cumana. The phenomenon is

one of peculiar beauty, when a small fleecy cloud is projected against the zodiacal light, and detaches itself picturesquely from the illuminated back-ground. A passage in my journal during a voyage from Lima to the West Coast of Mexico, notices such a picture. 'For the last three or four nights (between 10° and 14° of north latitude), the zodiacal light has appeared with a magnificence which I have never before seen. Judging also from the brightness of the stars and nebulæ, the transparency of the atmosphere in this part of the Pacific must be extremely great. From the 14th to the 19th of March, during a very regular interval of three quarters of an hour after the disc of the sun had sunk below the horizon, no trace of the zodiacal light could be seen, although the night was perfectly dark; but an hour after sunset, it became suddenly visible, extending in great brightness and beauty, between Aldebaran and the Pleiades, and, on the 18th of March, attaining an altitude of 39° 5′. Long narrow clouds, scattered over the lovely azure of the sky, appeared low down on the horizon, as if in front of a golden curtain, while bright varied tints played from time to time on the higher clouds: it seemed a second sunset. Towards that side of the heavens the light diffused appeared almost equal to that of the moon in her first quarter. Towards ten o'clock, in this part of the Pacific, the zodiacal light usually becomes very faint, and at midnight I could only see a trace of it remaining. On the 16th of March, when its brightness was greatest, a mild reflected glow was visible in the east.'"

He describes also several anomalous features, as that sometimes it did not appear for three-quarters of an hour after sunset, though the twilight had been for some time ended; that then it appeared suddenly, and continued long of very great brightness; that at other times it would continue to shorten and lengthen many degrees in a few minutes, and have an undulatory sort of motion. But these peculiarities, when not accounted for by the atmospheric circumstances of which he himself takes notice, seem rather to be produced in the eye of the observer by reason of the extreme faintness of the object to be observed; by the length of time that a retina,—which has been initiated by watching the setting sun, or even when acted on by ordinary daylight,—requires to recover its full degree of sensitiveness; as well as by the deceptive phantasmagoric effect produced on the nerves when strained to a greater extent than they can well bear.

Taking all the above facts into consideration, we are perhaps entitled to conclude, on pretty good foundation, that the zodiacal light is an extremely oblate, lenticular, revolving body, nearly in the plane of the sun's equator, rather excentrically situated, of so vast a size as nearly to fill the whole orbit of the earth, and sometimes actually to reach it. But whether it does actually at the present time correspond exactly with the sun's equator, and if it has always done so, and always will; whether the manifest changes in the intrinsic brightness, and the form and size of the light that have been observed, be due merely to a rotation of the excentric or oval body, or to a real periodical increase of the intensity of its

emanation, or an enlargement of its dimensions; and whether this be any concomitant symptom with the appearance of spots on the sun, or magnetical disturbances on the earth,—are matters still to be determined by observation.

The physical constitution of the zodiacal light seems also well worthy of being inquired into. The most probable supposition is, that which makes it consist of innumerable small planetary particles revolving about the sun, and shining by reflected, or not impossibly by direct light. Not impossibly, because while, on the one hand, the occasional crossing of the earth's orbit by the extremer portions of the zodiacal light, has been by many held to be the origin of the shooting stars; and many of them have been found to be, at the time of their incandescence, several hundred miles above the earth's surface, and thus far above the limits of the atmosphere, whose friction might have imparted such a degree of heat to a body at a lower altitude, moving with a velocity of 1000 miles per minute;—on the other hand, M. Mathieson has recently made some most interesting experiments, in which the thermomultiplier shewed evident indications of *radiant*, and therefore *direct heat*, proceeding from the zodiacal light.*

But in its present stage, the subject can only be profitably and successfully prosecuted in other climates, in countries where the twilight is shorter, where the ecliptic makes, all through the year, a larger angle with the horizon than here, and where there are clearer skies, and a more transparent atmosphere.

As such conditions are well commanded by some of the magnetical and meteorological observatories which have been lately established on a similar footing to that of Makerstoun, in connection with the Royal Society of Edinburgh; and as the species of phenomenon is one that belongs eminently to those departments,—we might expect ere long to enjoy much more intimate and exact knowledge of the laws and relations of this wondrous and extensive member of the solar system, if the Royal Society were to give its testimony that the phenomenon was one of a nature worthy of scientific investigation; as well as that all that has been done hitherto is insufficient, except for mere approximative purposes, and has been labouring under geographical disadvantages, which need not by any means continue to shackle observation in the present day.

* Comptes Rendus, t. xvi., p. 687; Ap. 1843.

GETTERAL EFFECT OF THE TOTAL ECLIPSE OF THE SUN JULY 28TH 1851

AS SEEN ON BUE ISLAND, W. COAST OF NORWAY.

XXXIV.—*On the Total Solar Eclipse of* 1851. By Professor C. Piazzi Smyth.

(Read December 1, 1851.)

Eclipses are still, as they have ever been, very important phenomena for the astronomical observer; partly on account of the crucial test which they afford for the examination of the truth of the theory and calculation of the motions, real and apparent, of the Sun and Moon, partly also for the special opportunities which they furnish of inquiring into some of the arcana of the physical characteristics of those bodies.

For the former purpose, a partial eclipse will serve almost as well as a total one; while the continued improvement of the observation of meridian passages is now raising these daily measures fully to the importance of the other occasional phenomena, as a test of the theory. But for inquiry into the physics of the Sun, a perfectly total eclipse of that body is necessary; revelations may then happily be procured, which no observation of any other phenomena at any other time, can hope to afford any suspicion of.

As the occurrence however of a total eclipse near any inhabited and civilised region of the earth, is very rare; and as even when it does occur, the observation lasts but for three short minutes,—the utmost extremity of importance attaches to the occasion in the eyes of all practical astronomers. So many circumstances, too, have to be noted, observed, and measured, within a few seconds, that it is necessary to adopt some systematic division of labour amongst a number of observers, and for each to be previously practised and expert in his particular part.

Much of this arrangement was organised for the eclipse of July 28, 1851; and while other observers were distributing themselves along various parts of the line of totality, I gladly seized the opportunity of occupying, in company with the Rev. T. R. Robinson, D.D., the western coast of Norway, where the path of the moon's shadow first entered Europe. On the importance of the occasion being represented to the Commissioners of Northern Lighthouses, then sitting in Edinburgh, that Board, who can so well appreciate science, and who have introduced so many of its more recondite appliances into their admirable establishments on the coasts of Scotland,—finding that their steam-vessel, the Pharos, would be engaged amongst the Shetland Isles about the time of the eclipse, most liberally undertook to convey Dr Robinson and myself to the selected part of the Norwegian coast; a boon of so much the more importance, as that portion was unvisited, so far as we could learn, by any sort of vessels available to ordinary passengers.

Being taken across the North Sea, then, in this manner, and having been pro-

vided, through the Admiralty, with a recommendation from the Swedish ambassador to the local authorities, which opened the whole coast to us without let or hindrance, we landed on the Bue Island, north of Bergen, on the morning of the eclipse,—erected the instruments, many of which had kindly been lent to us by Admiral Sir F. Beaufort, from the Hydrographical Department, and having the zealous co-operation of Messrs Commissioners Hunter, Thomson, and Urquhart, Mr Secretary Cuningham, and Mr Alan Stevenson, the able Engineer of the Board, together with the officers of the vessel, we were enabled to detail a distinct observer for each and every phenomenon that could well be expected during the obscuration.

Our preparations, however, met the fate but too frequently suffered by astronomers in these northern regions, viz., that they were rendered futile through clouds; clouds so dense that nothing whatever was seen of the heavenly bodies during the middle of the eclipse. But we had a remarkably good opportunity of judging of the general effect of a total eclipse; and what with our partial experience, and the impartiality with which we could judge of the observations of the more delicate phenomena by others, from not having any of our own to bring forward,—we are perhaps peculiarly qualified to point out, wherein observers may have failed in doing all that it is desired should be done on such an occasion, and how they may probably succeed another time.

The general effect of a total eclipse, however interesting and instructive, as one of the most sublime phenomena in nature, may yet appear unconnected with the more scientific portion of the observations; and so it is directly, but indirectly it has the greatest influence. For its effects on the minds of men are so overpowering, that if they have never had the opportunity of seeing it before, they forget their appointed tasks of observation, and *will* look round during the few seconds of total obscuration, to witness the scene. Although it is not impossible, but that some frigid man of metal nerve may be found capable of resisting the temptation, yet certain it is, that no man of ordinary feelings and human heart and soul, can withstand it. In the eclipse of 1842, it was not only the volatile Frenchman who was carried away in the impulses of the moment, and had afterwards to plead his being no more than a man, as an excuse for his unfulfilled part in the observations,—but the same was the case with the staid Englishman, and the stolid German. Nor was the history of this experience enough to guard against similar results on a second occasion; for in 1851, much the same unintended perversion of observation took place; and on asking a worthy American who had come with his instruments from the other side of the world, pointedly to observe this eclipse,—what he had succeeded in doing?—he merely answered, with much quiet impressiveness, that if it was to be observed over again, he hoped that he would then be able to do something, but that as it was, he had

done nothing, it had been too much for him. In fact, the general scene of a total eclipse, is a potent Siren's song, which no human mind can withstand: and the only way in which its witcheries can be guarded against, is that by which ULYSSES passed the fatal shore in safety. Let, then, those who on a future occasion have to make the more accurate telescopic observations, surround themselves by some high wall, which shall prevent their seeing anything but a very small portion of the sky round about the sun and moon. And let those to whom the observation of the general effects may have been confided, be competent and prepared to put whatever they see, pictorially on paper, so that others may afterwards profit by their opportunity.

First, as to this latter department, viz., the recording of the *general* effect. The result of my partial experience is, that during the progress of the earlier part of the eclipse, the observer may be sketching in a something of the general forms of the landscape, on six separate boards, giving 60° of azimuth to each, so as to include the whole panorama; or one long board properly supported may be better still, as there is no knowing beforehand where the most effective displays will take place. Moist water-colours in tin tubes and rough drawing-paper, I am disposed to consider, after much practice with all the varieties of water-colours, crayons, and oils, to be the most effective and convenient medium, all things considered, for general field-work. Being seated then in an open place, with abundant paper-surface before him, the observer should have a powerful lamp, to throw its light on his work and the colours, which should all be mixed up beforehand, and arranged on a large pallet.

Then on the instant that the total obscuration begins, and it is complete almost the instant that it begins, so well-defined is the shadow of the moon,—he should immediately put in the colours, shadows, and forms, at once and boldly, with a large brush; every stroke of which at the time, will enable his memory afterwards, to add multitudes of those little indescribable details, which together form the impression made on the eye; whose power was confessed at the time, but which are nevertheless easily and completely forgotten, unless actually seen again.

But to be able to put in even the groundwork of these six pictures in so short a space of time as the total obscuration lasts, hardly three minutes, requires something more than the mere wish to be able to do them, though this is unhappily all that astronomers have generally taken with them to this most difficult problem in art. So difficult is it to paint a tolerable picture, even under the most favourable circumstances, that it has been a matter of frequent remark, that no amateurs have ever produced works capable of standing side by side with those of professional painters; but when there is further the excessive difficulty caused by the almost instantaneous disappearance of the scene, so as to necessitate its being painted from the memory rather than the fact,—it is not to be wondered at that none of our scientific books yet contain a tolerable representation of the

effects of a total eclipse; and that only those few persons who have actually seen it, really know what it is like. The phenomenon therefore, when seen, has, by its unexpected novelty, such a power of enchantment, as to hold all observers spell-bound.

If astronomers, however, will only take the trouble, they may learn to give a good account of this most interesting subject. To no one who really tries to learn to draw, is the power wholly refused, and every one may by practice improve their memory, as applied to drawing, as well as to anything else. The test of the proper degree of skill having been arrived at, would be the taking of half-a-dozen views of the progress of a sunset, during a certain number of minutes; while to copy a picture after a one-minute view of it, would give the means of ascertaining afterwards what were the probable limits of that person's errors in light, shade, and form, without some estimation of which no astronomical drawing should be considered presentable. No drawing can be made perfect, any more than a numerical observation can. The one cannot be depended on to the minutest feature inserted on the paper, nor the other to the smallest fraction of a division read off from the instrument. The question in either case must be, what is the extent to which dependence can be placed? By knowing that the greatest probable error of Tycho Brahe's observations was 3′, Kepler proceeded safely to deduce the elliptic theory of the planets: and if theories are ever to be based on astronomical drawings, the possible limits of error in every way must be ascertained, and published as a necessary appendage to the pictorial representation.

I will not presume to say that I have arrived at the mark which is here proposed; but I have practised myself in drawing from memory, as well as in hasty sketches from nature. My part, however, at Bue Island, was with a telescope, and but for the unexpected clouding of the sky, I might have seen nothing of the general effects; the clouds, however, absolving me from my special duty, enabled me at least to look round, and I hastily made pencil sketches of what I saw. These were coloured as soon as possible afterwards, and form a series of views, shewing the varying effects, through the short period of the totality, and in various directions. One of these views has been engraved with the present paper (Plate XIV.), and as far as one only can serve, may perhaps tend to give something more of a local name and habitation in person's minds, to the verbal descriptions of which there have been many good ones from various of the observers of 1851.

I will only therefore add, that to understand the scene more fully, the reader must fancy himself on a small rocky island, on a mountainous coast, the weather calm, and the sky, at the beginning of the eclipse, $\frac{7}{10}$ covered with thin and bright cirro-strati clouds. As the eclipse approaches, the clouds gradually darken, the rays of the sun are no longer able to penetrate through and through, and drench them in living light as before; but, as with clouds on an evening sky, they become

darker than the background, on which they are projected. The air becomes sensibly colder, the clouds darker, and the whole atmosphere murkier. From moment to moment, as the totality approaches, the cold and the darkness advance apace; and there is something peculiarly awful and terribly convincing in the two different senses so entirely coinciding in their indications of an unprecedented fact being in course of accomplishment. Suddenly, and apparently without any warning, so immensely greater are its effects than those of anything else that had before occurred,—the totality supervenes, and darkness comes down. The shadow of the moon must evidently have a very well-defined termination; and those who have seen a large eclipse, or even an annular one, have no idea what a *total* eclipse is like. Then suddenly came into view lurid lights and forms, as, on the extinction of the candles, a phantasmagoric picture, before unnoticed, may be made to appear prominently imposing in a darkened room. This was the most striking point of the whole phenomenon, and was precisely that which made the Norse peasants about us fly with precipitation, and hide themselves for their lives. Darkness was everywhere, in heaven and in earth, except where along the north-eastern horizon a narrow strip of unclouded sky presented a low burning tone of colour, and where some distant snow-covered mountains, beyond the range of the moon's shadow, reflected the faint monochromatic light of the partially-eclipsed sun; and exhibited all the detail of their structure, the light and shade and markings on their precipitous sides, with an apparently supernatural distinctness. After a little time, the eyes seemed to get accustomed to the darkness, and the looming forms of objects close by could be discerned, all of them exhibiting a dull green hue; seeming to have exhaled their natural colours, and to have taken this particular one, merely by force of the red colour in the north. Life and animation seemed indeed to have now departed from everything around; and we could hardly but fear, against our reason, that if such a state of things were to last much longer, some dreadful calamity must happen to us also. While the lurid horizon northward, appeared so like the gleams of departing light in some of the grandest of the works of MARTIN and DANBY, that one could not at the time, and in that presence, but believe, in spite of their alleged extravagances, that nature has opened up to the constant contemplation of their mind's eyes, some of those magnificent revelations of power and glory, which others can only get a hasty glimpse of on occasions such as these.

To this part of the scene the plate refers, and may, perhaps, be considered a successful work on the part of the engraver, Mr JAMES FAED, in giving an idea in mere black and white, of the dark mysterious colouring of the scene. On other sides, rain clouds and falling rain, prevented any such striking effects as those just detailed, and within three minutes, the light of day was prevailing again.

So much, then, for the general effects of this total eclipse, and may the next

one meet with a better artist, and may some more perfect plan, than mezzotint engraving, be found for reproducing the drawings in all their colours, and cheap enough to admit of a whole series being published by any scientific society.

The clouds already mentioned, prevented anything very interesting being done in the way of exact measurement, and what little was accomplished, having already appeared in the *Memoirs of the Royal Astronomical Society of London*, 1852, need not be repeated here. The most important subjects which presented themselves for observation to those under clear skies, were the corona, and the red prominences. Both *may* be spurious effects, and both *may* be real forms of matter in the neighbourhood of the sun, but of such faint illumination as only to be visible during the darkness of a total eclipse.

Respecting the corona, Professor Baden Powell has produced such excellent imitations of it, by making dark bodies occult very bright points, and he has even shewn such a necessity for its existence to some extent, that what, with the excessive difference in the descriptions of different observers, and the absence of any crucial observations, we cannot consider that the corona has been proved to be anything real or material. On the other hand, we must not refuse the possibility of something of the sort, inasmuch as the best theory of the Zodiacal Light, represents it to be a nebulous mass, increasing in density towards the sun; but no part of the sky during the totality was dark enough to exhibit any such portion of the zodiacal light, as has ever been seen and recognised for it at night.

The red prominences, however, are much more precise phenomena in themselves, and have been better observed. Indeed, it may be considered, that they have been proved to lengthen on one side, and shorten on the other, during the eclipse, precisely in the *direction* in which they should do, on the supposition that they were true appendages of the sun, and that the moon was occulting them. This, however, is all, for other imaginable causes might produce such an appearance, and a difference of effect would only appear on comparing the accurately measured quantity and progress of the alteration of length, with the calculated motion of the moon.

This, however, has not been done; and it is not a little surprising, that so many astronomers should have observed the phenomenon, and been contented with merely gazing. A few of them measured approximately the angular position of the prominences on the sun's limb, but none measured the size and shape, and the rate of amount of increase or decrease. Indeed, the figures given by different observers, vary in the most incomprehensible way, and we can do no more than conclude, that something red was seen, and of a cosmical nature; but each person gives a different size and shape, and each person is quite certain that he is right. The observation doubtless may be, and indeed from this must be, very difficult; and a person who has not seen these bodies, ought not perhaps to form any judgment. But it must be apparent to every one, that almost every observer attempted

to do too much, and with insufficient means. He tried to give an account of all the prominences all round the sun's limb, as well as to observe the instants of beginning and ending of the totality, and judge of darkness over the landscape, &c., &c.;—his main instrument being, too, a small telescope, with generally some inferior style of altitude azimuth mounting.

Now, a little experience would shew, that a firm and clock-moved equatorial, with micrometer and lamp apparatus, is a *sine quâ non*; and that with this apparatus an observer should confine himself to a single red prominence, and get a numerous series of measures of it throughout its period of visibility. On the records of such measures a safe theory might be erected.

But, if we are never to see these red prominences, except during the very un frequent phenomenon of a total solar eclipse, ages may pass away before we know much, and if they be real, they must play some important part in the great mystery of the economy of the solar light and heat. Astronomers are bound, therefore, to exert themselves to the utmost, in contriving methods which shall make these prominences visible at all times; and Mr James Nasmyth, C.E., having suggested to me a method by which he hoped the end in view might be effected, I lost no time in putting it into execution. The method consisted in pointing a telescope to the sun from a dark room, and therein receiving the image of the field of view on the top of a box, painted black inside. A circular hole, a little larger than the sun's image, being then made in the lid, the solar light passes through, is completely absorbed on the sides of the box, and the picture of the annular portion of the field, between its boundaries and the sun's, *i.e.*, the blue sky adjacent to the sun, can be examined at leisure, and in comparative darkness, so that a faint light projecting from the solar orb, anything in the shape of a ray or red prominence, would have much greater chance of being seen.

Mr Nasmyth having no means at command to try his proposed experiment, I put it into execution myself in the Edinburgh Observatory. The shutters in the dome were furnished with screens and tubes, allowing no sunlight to enter the room, but what passed through the object-glass of the telescope; and this was 9 feet long, by 6 inches in diameter, was moved by clockwork, and carried near the eye-end, on an adjustable arm, a large light box lined with black velvet, and having a hole in the top. The image of the sky, in all but contact with the sun, was then received and examined on the surface of the lid round about the hole, into which the sun's rays passed and were lost. So far as the apparatus was concerned, everything answered to admiration, for when the sun's image was actually thrown into the dark box, the general illumination of the room was certainly much fainter than that of the air during the total eclipse.

But notwithstanding this, and though I have tried it carefully on all the finest days of the autumn of 1851, and the summer of 1852, I have seen nothing of any prominent matter beyond the photosphere of the sun. The same negative

result was obtained when a circular plate was made to eclipse the sun's image in the focus of the telescope, and I looked directly into the eyepiece. But hardly any other result could well be expected, as however dark the room might be kept by the apparatus employed, that in no wise checked the illumination of the atmosphere outside, in the apparent neighbourhood of the sun, the daylight, in fact; and this was always so bright, that no object of the reputed faintness of these red prominences could well appear on so luminous a background.

There is only one way of getting over this difficulty; *i. e.*, taking the telescope to the top of a high mountain, above all grosser parts of the atmosphere. Other circumstances have lately compelled me to request leave from Government to take the Edinburgh Equatorial temporarily to the top of the Peak of Teneriffe; and if allowed to do so, it shall be one of my first cares to repeat this experiment.

This mode undoubtedly would not be perfect, none would unless tried altogether above the limits of the atmosphere; but it would certainly be a great improvement on anything done on the surface of the earth at the level of the sea, and might perhaps be found sufficient for the object in view. All travellers who have ascended high mountains, combine in speaking of the greater blackness of the sky witnessed in those elevated regions, as well as of the sun becoming more luminous and more concentrated as to his rays, and of stars becoming visible to the naked eye by day. Captain Hodges, at the height of 15,000 feet on the Himalayas, saw, with a two-inch object-glass, stars of the fifth magnitude in the open sunshine: but on the Calton Hill, with the smoke of Edinburgh more or less diffused through the air, stars of the first magnitude are frequently invisible, in our pale blue sky, to a six-inch object-glass; thus making a difference in favour of the mountain station, of at least 100 to 1. I have not myself had experience of such great heights, but have observed for months at the altitude of 6000 feet, and from the improvement in the transparency of the atmosphere up to that point, can well believe what has been related of the higher station.

Thus far I have gone on the supposition that it was right and proper to attach great importance to the conclusions of the actual observers, that the red prominences were actual material bodies. This, however, has not been proved; and we cannot be too careful in guarding against the deceptive effects of objects close by. Now it is not difficult to suppose some partial diffractions of the sun's light amongst the craggy mountains of the moon, during the total eclipse, which might make some rays diverge, and become visible in an anomalous manner. Accordingly, I introduced into the focus of the object-glass a small sphere, which was made to pass before and so eclipse the sun's image, as in the natural phenomenon.

The results were, that light of a pink colour was thrown off from the edge of the sphere, and in greater quantity as the polish of the surface was higher; in a complete ring if the surface was smooth, and in detached portions if the surface

was crystallised. Balls of plaster of Paris, zinc, brass, transparent and opal glass, were tried: the best results were obtained with the last; when scratched with a diamond, there appeared only a little pink prominence here and there; often appearing exactly like those pictured by the eclipse observers.

This pink light was, however, always thrown off from some object out of focus, though the visibly bounding line of the sphere might be in focus; and again, the light belonged to the ball as a centre, and not to the sun, seeming, therefore, to be a different phenomenon to the eclipse prominences; though the parallel direction of the rays grazing the moon's edge, and the converging of those touching the ball's, should be taken into consideration.

A more similar experiment in this way, is to eclipse the sun behind a distant object; and for this purpose I placed a black tin screen on the top of Nelson's Monument, and observed it from below with the naked eye and a small telescope. When the sun was completely eclipsed by the disc, there was much light of a spectrum character, with a preponderance to orange and red, thrown off beyond the edge, and this light was most abundant on that part of the circumference of the tin disc, to which, at the time, the sun was closest: thus bearing some sort of relation to the observed fact of the lengthening of the red prominences on that side of the moon to which the sun was advancing. Anything transparent, as a bristle, on the edge of a disc, was particularly vivid, and some ropes in the neighbourhood were "glorified" over an extent of two degrees. This effect, too, was more marked the clearer and more transparent the atmosphere. With much haze in the air it vanished altogether; the disc and ropes then projecting themselves blackly on the bright sky behind. This would seem also to be in some measure in favour of the idea of a spurious origin at the moon's edge for the eclipse prominences. The evidence, however, is so very uncertain, that few things would be more productive of advantage in the present state of the subject, than the repetition of all the experiments with a better instrument, either in the rarified atmosphere of the Peak of Teneriffe, as just mentioned, or that of some higher mountain: such observations, too, made at once, might tend to save and to utilise much valuable time on the occasion of the next total eclipse of the sun.

XXXV.—*Observations on the Speculations of Dr Brown and other recent Metaphysicians, regarding the Exercise of the Senses.* By Professor W. P. Alison.

(Read 7th and 21st February 1853.)

In offering to this Society a few remarks which have occurred to me on this fundamental department of Mental Physiology, I beg in the first place to explain, that my reason for doing so is merely this, that in consequence of certain unguarded expressions, and, as I think, hasty reflections, the opinions of Dr Brown, and likewise of Sir James Mackintosh, and of Lord Jeffrey, and other more recent writers on this subject, have been supposed to be irreconcileably at variance with those of Dr Reid and Mr Stewart; *i. e.*, with those which are usually called the leading doctrines, or essential characteristics, of the Scotch School of Metaphysics, in this fundamental department of the science. And when such difference of opinion is believed to exist among men of generally acknowledged talent, who have studied this subject, and nothing like an *experimentum crucis* can be pointed out, to compel us to adopt one opinion and reject another,—the natural inference is, that there is something in the study itself, which renders it unfit for scientific inquiry,—that what is called the study of the Mental Faculties granted to our species is, in fact, only a record of the vacillations of human fancy and ingenuity, in the invention and demolition of hypotheses,—and that the subject is one on which it is in vain for our minds to dwell, with any hope of applying the principles of Inductive Science, and acquiring any insight into the laws of Nature, regulating the phenomena presented by the last and greatest of her works, similar to that which is the object and the reward of all other scientific inquiries.

When, for example, we find it stated by Dr Brown, that on the first and most fundamental of all inquiries regarding the human mind,—that into the belief which attends the exercise of the Senses,—the creed of the sceptic, and of the orthodox philosopher of Dr Reid's School, consists, in fact, of the same two propositions,—"and that what appeared to Dr Reid and Mr Stewart to be the overthrow of a great system of scepticism on this subject, was nothing more than the proof that certain phrases are metaphorical, which were intended by their authors to be understood only as metaphors;"—(*Lectures*, vol. i., p. 584); when we find this statement of Dr Brown's regarded by Sir James Mackintosh as so just and important, that he says, "the whole intellectual part of the philosophy of Brown is an open revolt against the authority of Reid;—Mr Stewart had dissented from the language of Reid, and departed from his opinions on several secondary theories; Dr Brown rejected them entirely,—*very justly* considering the claim of Reid to the merit of detecting the universal delusion, which had betrayed philosophers into the belief that Ideas, which were the sole object of knowledge, had a separate existence, as

a proof of his having mistaken their illustrative language for a metaphysical opinion;"—and when we remember the unparalleled popularity of Dr Brown's Lectures since his death, which has no doubt led many to suppose that he is now regarded as the first authority on these subjects in Scotland;—when we find, again, Lord Jeffrey admitting that "Dr Reid's subversion of the ideal system, or confutation of that hypothesis which represents the immediate objects of the mind in perception as certain images or pictures of external objects conveyed by the senses to the sensorium, had been performed with complete success;" but adding "that after considering the subject with some attention, he has not been able to perceive how the destruction of the Ideal Theory can be held as a confutation of those reasonings, which have brought into question the popular faith on this subject;" (*Edinburgh Review*, vol. iii., p. 281; or *Contributions to Edinburgh Review*, vol. ii., p. 604)—when, on the other hand, we find it stated by Sir William Hamilton, that Dr Brown is, from first to last, in one and all of his strictures on Reid's doctrine of Perception, wholly in error;" but, "that nevertheless there are ambiguities and inconsistencies of Reid himself, in this the most important part of his philosophy, which ought to be exposed, and so deprived of their evil influence;" (*Works of Reid*, p. 820)—"and, in particular, that so far from confuting Idealism, the doctrine of Reid and Stewart affords it the best of all possible foundations;" (*Works of Reid*, p. 820)—and, again, by Morell, that although Reid "performed an inestimable service to philosophy, by shewing that certain simple processes must be viewed as ultimate and primitive facts in our constitution,—the benefit of which is still to be developed in coming generations,—yet that the false, or at least inadequate view which he has taken of the *reflective method* in mental philosophy, has caused a want of comprehensiveness as to the legitimate objects or extent of philosophy at large;" (*Morell's Philosophy*, vol. ii., p. 51)—I think I have quoted enough to shew, that a general distrust of all speculations which led such men to such variety of opinion, and despondency as to the possibility of any fixed or useful principles being established, by scientific examination of the elementary mental processes to which they referred, is not unreasonable. And if, nevertheless, we hold, as I think we ought to do, by the maxim, "that when Reason and Philosophy have erred, it is by themselves alone that their error can be corrected,"—I trust it will not be deemed a useless or unprofitable discussion, to endeavour to shew that when the subject is calmly reviewed, and verbal ambiguities as far as possible avoided, there is really no such difference of opinion among these authors, as will justify the strong expressions of dissent which I have quoted; but that the differences of opinion are either verbal only, or relate to matters *ulterior to the main points of controversy*, which have interested the human mind, in all ages, on this subject; and, in particular, ulterior to those on which it was the object of Reid and Stewart to establish fixed and satisfactory principles; and that there are certain general truths

in regard to the mental part of the process concerned in the exercise of the senses,—probably admitting of much more subtle analysis, and more learned discussion than I can presume to offer,—but already sufficiently certain and precise, to constitute an important part of the science of Physiology; and remarkably in accordance with all that has since been ascertained, as to the physical part of that process.

As I was myself honoured in early life with the friendship both of Mr STEWART and Dr BROWN, and as I know well how much the former of these illustrious men was pained by finding that the latter, when succeeding him in the Chair of Moral Philosophy, had (as he afterwards expressed it) "given countenance to some doctrines, which, to more cautious and profound thinkers, appear to have a practical tendency quite at variance with his known principles and opinions;" (*Elements of the Philosophy of the Mind*, p. 502)—although I believe that the natural partiality of Mr STEWART to the studies to which he had devoted his life, had led him to exaggerate, in some degree, their practical importance,—still I feel much gratified at being able, as I think, in some measure to reconcile the apparently conflicting statements in their writings, and point out the misapprehensions —almost entirely on the part of Dr BROWN—to which they may be traced.

It will be generally admitted, that the first object of REID and STEWART was to ascertain, by strict induction, the existence, and establish the authority, of certain *Principles of Common Sense*, as they were termed by REID; Primary Elements of Human Reason, or *Fundamental Laws of Human Belief*, as they were termed by STEWART; *Principles of Intuitive Belief, or Truths learned by Intuition*, —perhaps the best name for them,—as they were since termed by BROWN; which must be regarded as *ultimate facts* in the constitution of the human Mind, equally essential to all reasoning, to all scientific inquiry, to the acquisition of all practical knowledge, and to the daily business of life.

Now the existence of such principles of Belief, and their authority, as ultimate facts in our mental constitution, are fully admitted by all the authors I have quoted; by no one are they more clearly and emphatically announced than by Dr BROWN. "Principles of intuitive belief," he says, "are essential to Philosophy in all its forms, as they are physically essential, indeed, to the very preservation of our animal existence." "The belief of *our identity* is not the result of any series of propositions; but arises immediately, in certain circumstances, from *a Principle of thought*, as essential to the very nature of the Mind, as its powers of Perception or Memory, or as the power of Reasoning itself; on the essential validity of which, and consequently on the intuitive belief of some *first truth* on which it is founded, every objection to the force of these very truths themselves must ultimately rest. To *object* is to *argue;* and to argue is to assert the *validity* of *argument*, and therefore of the primary evidence, from which the evidence of each succeeding proposition of the argument flows. To object to the authority of such primary

intuitive belief, would thus be to reason against reason,—to affirm and deny at the same moment,—and to own that the very arguments which we urge are unworthy of being received and credited.

" Without *some* principles of immediate belief, then, it is manifest that we could have no belief whatever; for we believe *one* proposition because we discover its relation to *some other proposition;* and we must ultimately come to some *primary* proposition, which we admit from the *evidence contained in itself*, or, to speak more accurately, which *we believe from the mere impossibility of disbelieving it.* All reasoning, then—the most sceptical, be it remarked, as well as the most dogmatical—must proceed on *some* principles which are taken for granted, not because we infer them by logical deduction, but because the admission of these first principles is a necessary part of our intellectual constitution.

" Every action of our lives is an exemplification of some one or other of these truths, as practically felt by us. Why do we believe that what we *remember* truly took place, and that the course of Nature will be in future such as we have already observed it? Without the belief of these physical truths, we could not exist a day, and yet there is no reasoning from which they can be inferred.

" These *principles of intuitive belief*, so necessary for our very existence, and too important, therefore, to be left to the casual discovery of Reason, are, as it were, an eternal, never-ceasing voice from the Creator and Preserver of our being. The reasonings of men, admitted by some and denied by others, have over us but a feeble power, which resembles the general frailty of man himself. These internal revelations from on high are omnipotent, like their Author. It is *impossible* for us to doubt them, because to disbelieve them would be to deny what our very constitution was formed to admit."—(Brown, p. 286.)

The principle thus stated by Dr Brown, and some of the illustrations of it which he has given, seem to me to be worthy of all acceptation; but I beg to ask, how do they differ from the fundamental proposition of Dr Reid's Philosophy of Common Sense; long previously set forth, for example, in the following passage? If there is no essential difference, then I think it clear that Dr Brown ought to have distinctly intimated his acquiescence in this, which Dr Reid regarded as the cardinal point of his doctrine; and so far, by limiting and defining the province of reasoning, and that of simple observation in such inquiries, endeavoured to prevent useless labour, and irksome uncertainty, in future students of the same science.

" All reasoning must be from First Principles; and for first principles no other reason can be given but this, that, by the constitution of our Nature, we are under a necessity of assenting to them. Such principles are parts of our constitution, no less than the power of thinking; Reason can neither make nor destroy them, nor can it do anything without them.

" How, or when, I got such first principles, upon which I build all my rea-

soning, I know not, for I had them before I can remember; but I am sure they are parts of my constitution, and that I cannot throw them off. That our thoughts and sensations must have a subject, which we call *ourself*, is not an opinion got by reasoning, but a natural principle. That our sensations of touch indicate something external, extended, figured, hard or soft, is not a deduction of reason, but a natural principle. The belief of it, and the very conception of it, are equally parts of our constitution. If we are deceived in it, we are deceived by Him that made us, and there is no remedy."—(*Works of* Reid, *by* Sir W. Hamilton, p. 130.)

"I beg," he says farther, "to have the honour of making an addition to the sceptical system, without which I conceive it cannot hang together. I affirm that the belief of the existence of Impressions and Ideas, is as little supported by reason, as that of the existence of Minds and Bodies. No man ever did, or ever could, offer any reason for this belief. A thorough and consistent sceptic will never therefore yield this point; and while he holds it, you can never oblige him to yield anything else.

"*To such a sceptic I have nothing to say;* but of the semi-sceptics, I should beg to know, why they believe the existence of their own impressions and ideas? The true reason I take to be, because they cannot help it; and the same reason will lead them to believe many other things."—(*Do.*, p. 130.)

In quoting this last passage from Dr Reid, I think it right to say, that notwithstanding his distinct assertion here made, and supported by Mr Stewart, that the evidence of *Consciousness* (by which we are informed of the acts of our own minds) stands on exactly the same footing as that of *Sense*, and is equally open to the objections of the sceptic, it seems to me that the objection to that statement, made by several more recent authors, is well founded; because what we mean by objects of consciousness are certain changes or events which we feel within ourselves, and we cannot, *without absurdity*, assert, both that such a change exists, *i. e.*, that we feel it, and that we doubt its existence, which implies that it may not exist. To doubt the evidence of consciousness, therefore, is not merely to do violence to our understandings, but is to assert a contradiction in terms.

This is thus stated by Lord Jeffrey: "Whatever we doubt, and whatever we prove, we must plainly begin with Consciousness. *That only is certain—all the rest is inference.* Our perceptions—not the existence of their objects—are what we cannot help believing."—(*Review*, vol. iii., p. 283.) And the same ground is taken by Sir Wm. Hamilton thus: "There is no scepticism possible touching the facts of consciousness *in themselves.* We cannot doubt that the phenomena of consciousness are real, in so far as we are conscious of them, because such doubt, being an act of consciousness, would contradict, and consequently annihilate itself: but all beyond the mere phenomena of which we are conscious, we may, without fear of self-contradiction at least, doubt."—(*Works of* Reid, &c., p. 129.)

But granting that this criticism is correct, the only alteration we need make on the passage last quoted from Reid is this, that, instead of asking the "semi-sceptics why they believe in the existence of their impressions and ideas," we should ask them, why they believe that the impressions and ideas of which they are *conscious*, are *their own*, or belong to the same persons as other mental changes which they *remember*. Here we become involved with the evidence of Memory and of Personal Identity, as to both of which Dr Brown expressly admits, in passages already quoted, that they are to be ranked among the principles of Intuitive Belief; and with that slight correction, this passage from Dr Reid—closely approximating, as it obviously does, to that previously quoted from Dr Brown,—must have commanded his entire acquiescence.

The reality and importance of these principles, regarded as ultimate facts in our mental constitution, is still more satisfactorily attested by Sir Wm. Hamilton, who has marshalled an array of authorities, such as any other man in this country might have in vain attempted, amounting to more than a hundred ancient and modern writers, all of whom, under certain varieties of expression, have announced and illustrated the same general proposition.

Farther, not only have we this nearly uniform agreement of these philosophers in regard to the general statement of Reid, that "there are various acts of our minds, of which, when we analyse them as far as we are able, we find Belief to be an essential ingredient;" but we have a special agreement, of all those whose opinion is thought of much weight, as to the fact of the exercise of the Senses being one of the occasions, in which evidence of this description, whether directly or indirectly, is at least uniformly and essentially concerned.

The shortest and simplest account of Dr Reid's doctrine on this subject is given by him in the following words: "The external senses have a double province, to make us *feel*, and to make us *perceive*. They furnish us with a variety of *sensations*, pleasant, painful, or indifferent; at the same time, they give us a *conception*, and an *invincible belief of the existence* of external objects. This conception of external objects is the work of Nature; the belief of their existence is the work of Nature; so also is the sensation that accompanies it. The *conception and belief*, which Nature produces by means of the senses, we call Perception."

He thus introduces the Intuitive Belief, simply as a part or accompaniment of the operation of the mind which results, in the healthy state, from an impression made on the senses, and a Sensation excited in the mind; and afterwards he enters on explanations in regard to the different qualities attributed to the material objects thus made known to us,—particularly as to the distinction of the Primary and Secondary qualities of matter, and the formation of the general notion or conception of Extension or Space; which, as he says, is no sooner formed, than it swells in the human mind to Infinity, as surely as the notion of Time to Eternity; and affords, therefore, the simplest illustration of the essential distinction between

Sensations felt in the mind, and Perceptions formed in the mind, in consequence of those sensations.

But although he stated that he could trace the formation of our notions in regard to the external world no farther than the mental operations thus described, he distinctly admitted, as a general principle, the possibility, and approved the attempt, of farther analysis, if made under due precautions. "It must," he says, "require great caution, and great application of mind, for a man that has grown up in all the prejudices of education, fashion, and philosophy, to unravel his notions and opinions, till he find out the simple and original principles of his constitution, of which no account can be given but the will of our Maker.

"This may be truly called an *analysis* of the human faculties; and till this is performed, it is in vain we expect any just system of the mind,—that is, an enumeration of the original powers and laws of our constitution, and an explication from them of the various phenomena of human nature. Success, in an inquiry of this kind, it is not in human power to command; but perhaps it is possible, by caution and humility, to avoid error and delusion. The labyrinth may be too intricate, and the thread too fine, to be traced through all its windings; but if we stop where we can trace it no farther, and secure the ground we have gained, *there is no harm done; a quicker eye may in time trace it farther.*"—(Reid, p. 99.)

It is plain, therefore, that it was quite in accordance with Dr Reid's views,—both with the principles which he thought he had established, and with his anticipations of the future progress of the science,—to attempt a farther and more minute analysis of the acts of Mind, attending the exercise of the Senses, by which we are assured of the existence, and informed of the properties of external things; and to endeavour to refer these, by a process of induction, to other and more general Laws of Mind. This, accordingly, has been attempted by several later writers. Mr Stewart maintained, and fortified himself by the opinion of Turgot, that in order to the formation of the notion of Externality, or independent existence, in any object of perception, a repetition of the same sensations, under the same conditions, is necessary, and that then the formation of that notion,—the conclusion thus drawn as to the existence of a cause for our sensations, independent of ourselves, might be referred to the general Law of Mind,—analogous to the first Law of Motion, or the Inertia of Matter,—our belief "that the course of Nature is uniform, or will be in future such as we have already observed it."

Dr Brown went a step farther. He explicitly admitted the accuracy of the distinction drawn by Dr Reid between Sensations and Perceptions, and the convenience of the term Perception, as denoting an act of the mind, distinguishable from all others, but, as he thought, resolvable into others. "I am far from wishing," he says, "to erase the term Perception from our metaphysical vocabulary. On the contrary, I conceive it to be a very convenient one, if the meaning attached to it be sufficiently explained, by an analysis of the complex state of mind which

it denotes."—(*Lectures*, vol. ii., p. 47.) And he made a very ingenious attempt (such as Dr Reid, from his expressions above quoted, I think, must have approved) to explain how the notion of the primary qualities of matter may be gradually formed, by the help of experience, in the mind.

"Perception," he says, "is only another name for certain associations and *inferences* which flow from other more general principles of the mind."—(Vol. i., p 569.) He then goes on to explain how, by means of certain sensations, and particularly of those *muscular sensations*, consequent on the excitement of instinctive and voluntary muscular actions, which he has so ingeniously illustrated, the notion of the qualities of matter may be gradually introduced into the human mind. He distinguishes the Primary Qualities of Matter, I think, more satisfactorily than Reid, or perhaps any other author has done, as the different modifications of Extension and Resistance; "the very notion of which combined," he says, "seems necessarily to indicate a material cause, or rather, is truly that which constitutes our very notion of Matter."—(Vol. i., p. 574.)

I am much inclined to think, although I would not state it as certain, that his very ingenious analysis of the mental acts suggesting this notion, *as it is often suggested*,—*i. e.*, regarding it as the natural result of *muscular sensations, repeatedly excited*, and *again obstructed*, in *different degrees*, at *different points*, and for *different periods* of time,—is correct; if so, it affords as good an example as can be given, of what his friend Mr Campbell called "the mysterious, and almost miraculous subtilty of his mind." But I maintain with confidence, that it does not in the slightest degree invalidate the statement of Reid, as to the Belief which accompanies this act of the mind being a case of that Intuitive Perception of Truth, which we have seen that Brown, equally as Reid, admitted as the foundation of all knowledge and all reasoning; and that for two reasons:—

First, Dr Brown expressly admits, that the perception of the primary qualities of matter *may take place without any such process of repeated muscular contraction and reasoning thereupon;* and that it does so in the lower animals, in whom the very first complex act of perception may often be observed to be *instantaneous*, and yet perfect, and its suggestions correct. "The calf and the lamb," he says, "newly dropt into the world, seem to measure forms and distances with their eyes almost as distinctly as the human reason measures them after all the acquisitions of his long and helpless infancy."—(Vol. ii., p. 70.)

The well-known observation of the chicken and the spider shews that, in other classes of the lower animals, this primitive instinct, or suggestion, as he calls it, is still more obvious. It is therefore, as he states it, only a question of observation and experiment, whether or not, in man as in other animals, Nature does communicate information by *intuitive* suggestion consequent on sensation,—which is neither contained in, nor logically deducible from, the sensation, but is, nevertheless, correct.

But *secondly*, the analysis which he offers of this act of mind, as usually performed by man, only professes to resolve the act, which REID called Perception, and regarded as an ultimate fact, into other principles or laws of thought, which Dr BROWN himself regards as ultimate facts; particularly into the principle that "we must *suppose a cause for all our feelings*" (vol. i., p. 565); the "*intuitive belief*" that what has been as an *antecedent*, will be followed by what *has* been as a *consequent*" (do., p. 514); the notion of *Time;* the belief in the Suggestions of Memory (do., p. 553); and the principle of Association or Suggestion (do., p. 565). "I do not conceive," says he, "that it is by any *peculiar* Intuition we are led to believe in the existence of things without. I consider this belief as the effect of that more *general Intuition* by which we consider a new consequent, in any series of accustomed events, as a sign of a new antecedent, and of that equally general principle of association, by which feelings that have frequently co-existed flow together, and constitute afterwards one complex whole."—(Vol. i., p. 518.)

The fact that *notions are formed* in the Mind of the properties of Matter, perfectly distinct from the sensations which excited them, and to be explained only by reference (sooner or later) to what we call *Intuition*, remains, therefore, as REID stated it; and is indeed strongly illustrated and confirmed by the elaborate analysis of the mode of their formation, attempted by Dr BROWN.

On the other hand, a fundamental part of the doctrine of KANT, as I understand it, and to which Sir WILLIAM HAMILTON is disposed to assent, is, that the notion of *Extension or Space*, which Mr STEWART thought it important to distinguish from the other primary qualities, as what he called one of the Mathematical affections of Matter, ought to be regarded as a necessary condition, or native element or *form of thought;* and that a belief in the existence of "an extended world, external to the mind and even to the organism, is not a faith blindly created, or instinctively determined, on occasion of a sensation; but exists in, or as a constituent of, Perception proper, as an act of Intuition or immediate knowledge."—(*Collected Works of* REID, p. 883.)

Whether this is really an improvement on the doctrine which he states, in connection with it, as that of Dr REID [viz., "that on occasion of a Sensation, along with a notion or conception, constituting the Perception proper, there is blindly created in us, or instinctively determined, an invincible belief in its existence"], or whether this distinction is really verbal, I do not presume to decide; but I think it must be admitted, that this opinion is truly *an addition* to the statement of REID, and does not *stand opposed* to it; inasmuch as REID says only, "that the conception and belief are the work of Nature;" and this, of course, does not exclude the evidence that may be adduced in favour of any particular mode, in which we may suppose that Nature accomplishes the work; as, indeed, we have already seen that both STEWART and BROWN supposed it to be performed

by help of the general law of belief in the continuance of the order of Nature, which it had not occurred to Reid to connect with it.

But if there be, as I maintain, this perfect *accordance* between the principles of Dr Reid and the elaborate attempt of Dr Brown, as of other later authors, to analyse those operations of mind to which the term Perception has been restricted by both, we may be pretty well assured that any *difference* of opinion among those authors, on this subject, can be of no great scientific importance; and may very probably resolve itself into one of those partial controversies, involving more or less of personal jealousy, which, we must admit, have disfigured and retarded most sciences.

We may next ask, then, how it should happen that Dr Brown should have thought himself justified in dwelling at great length on what he called an *extraordinary mistake* made both by Reid and his followers, as to the evidence of Sense, and the scepticism of Berkeley and Hume regarding it?—how he should have been led to infer, and been at such pains to prove, that there is no real difference between the creed of the sceptic and that of the orthodox philosopher of Dr Reid's school as to the evidence of sense; and how Sir James Mackintosh should have been led to assert the whole intellectual part of the philosophy of Brown to be, by reason of their *difference* on this very subject, an open revolt against the authority of Reid?

The reason of this is, that both these authors, and other recent writers, as it appears to me, certainly misconceived and misrepresented the controversy as it was carried on during last century, in several particulars. I do not say that there may not have been partial mistakes on the part of Dr Reid, particularly as to the exact meaning of previous authors,—and certainly there is in his writings a diffuseness of style, and frequent repetition of statements which might have been more impressive if more condensed;—but the chief misapprehensions affecting the principles which I have stated, were clearly on the side of Brown.

I. It was a palpable misconception on the part both of Dr Brown and Lord Jeffrey, to attribute to Dr Reid the attempt to prove, by reasoning, the existence of the material world, in opposition to the scepticism of previous authors.

Thus Dr Brown speaks of Reid's "*supposed proof of the existence of a material world*," as quite inadmissible (vol. ii., pp. 50, 51); and Lord Jeffrey speaks of his destruction of the Ideal Theory as "having been *held as a demonstration of the real existence of matter*."—(*Edinburgh Review*, vol. iii., p. 281.) Whereas they ought to have observed that Reid had, in a few simple but weighty words, disclaimed, as expressly as it is possible to conceive, any intention of attempting, or belief in the possibility of obtaining, such proof. He says, "Many eminent philosophers have laboured to furnish us with *reasons for believing our senses*; but their reasons are *very insufficient, and will not bear examination*."—(*Collected Works*, p. 328.) "Man's knowledge of what really exists, or ever did exist, *comes by a channel*

which is open to those who cannot reason. He is *led to it in the dark, and knows not how he comes by it.*" "The pride of philosophy has led some to invent vain theories to account for this knowledge; and others, *who see this to be impracticable*, to spurn at a knowledge which they cannot account for, and vainly endeavour to throw it off. But the wise and humble will take it as the gift of Heaven, and endeavour to make the best use of it."—(*Ibid*, p. 330.)

Consistently with this statement, it is plain that Dr REID'S object (as expressly avowed by Mr STEWART, *Phil. Essays*, p. 551, published in 1810, prior to Dr BROWN'S first course of Lectures on this subject), in this department of the science, could not be to prove by argument the existence of the material world, but only to *refute the argument against it;* and to put our belief in it on the footing of one of those Intuitive principles, the existence of which we have seen that Dr BROWN fully admitted and illustrated, as being essential to all knowledge and all reasoning, and tacitly admitted in all inquiries and all arguments; therefore, to put scepticism on this subject on the same footing as that of the "thorough and consistent sceptic, who will not believe in the suggestions of his own memory, or the identity of his own person," to whom Dr REID had explicitly avowed, that "he had nothing to say;" and whose scepticism, as we have seen, Dr BROWN regarded in precisely the same light.

II. It was quite a misconception to suppose that the creed of the sceptics of those days was merely, as Dr BROWN states it, the *negative proposition* that the independent existence of the material world cannot be proved by reasoning,—or, as he expresses it, "that no argument can be offered to shew, by mere reasoning, the existence of external causes for our feelings."—(*Sketch of a System, &c.*, p. 143.) If this had been their principle, the words above quoted prove, that it must have commanded the entire acquiescence of Dr REID. But their creed,—so plausibly supported, and so ingeniously deduced from the language of the most esteemed metaphysicians then generally known, as to have a practical bearing which we can hardly realise in this generation,—was *the positive proposition*, that Reasoning compels or necessitates our *disbelieving* that independent existence, as involving an absurdity.

The opinion of the ablest judges, says Dr REID (in his first work, published in 1764), when speaking of the reasoning of BERKELEY as to "the evidence of the senses, seems to be, that these arguments neither have been nor can be confuted, and that *he has proved by unanswerable arguments, what no man in his senses can believe.*"—(*Collected Works*, p. 101.)

The object of HUME, says Mr STEWART, obviously was, "to inculcate a universal scepticism; not, as some have supposed, to exalt reasoning, in preference to our instinctive principles of belief, but, by illustrating the *contradictory conclusions* to which our different faculties lead, to involve the whole subject in the

same suspicious darkness;—not to *interrogate* Nature, with a view to the discovery of truth, but, by a *cross-examination* of Nature, to involve her in such contradictions as might set aside the whole of her evidence, as good for nothing." (*Phil. Essays*, p. 56.)

The argument of Berkeley and Hume, although expressed in various terms, seems in substance to have been always this,—That we are made acquainted with any existence external to ourselves only by means of our own Sensations, *i. e.*, of certain acts or states of our own minds; or, as they usually expressed it, by *ideas in our own minds;* that any such external objects as exist must be the *exact images or prototypes of these ideas or mental states*, and that *it is absurd to assert* that an act or state of mind, whether called sensation or idea, can be the exact image or resemblance of any thing but another act of the same, or some other mind.

The following passage from Mr Hume is given by Dr Reid, as the shortest and clearest exposition of the argument which he had anywhere found:—

"The universal and primary opinion of all men, that we perceive external objects, is soon destroyed by the slightest Philosophy, which teaches us, that nothing can be present to the mind but an image or Perception;" (the distinction of which term from Sensation, was not recognised by Hume), "no man who reflects, ever doubted that the existences which we consider when we say *this house*, and *that tree*, are nothing but perceptions in the mind, and *fleeting copies and representations of other existences which remain uniform and independent.* So far, then, *we are necessitated by reasoning to depart from the primary instincts of nature*, and to embrace a new system with regard to the evidence of our senses." To the same purpose we have the explicit declaration of Berkeley, "that the existence of bodies, out of a mind perceiving them, is not only impossible, but a *contradiction of terms.*"

This is not, as Dr Brown stated it, "a mere negative assertion, that the existence of external things cannot be proved by argument" (vol. ii., p. 55), but as Dr Reid had said, a distinct *positive assertion*, that argument or reasoning does *compel*, or *necessitate*, our departing from the belief in that existence, as involving an absurdity or contradiction. It was these positive but puzzling, and even humiliating assertions, and these only, that Dr Reid undertook to confute.

III. It was quite a misconception to assert, as Dr Brown repeatedly and confidently did, that the term Ideas, in the language of Hume, or of any philosopher after Locke, was to be understood *only* metaphorically or figuratively, as an expression for acts or states of mind, and did not imply belief in the existence of anything intermediate between the mind and the external objects of sense.

He shewed, indeed, that the term had been used *occasionally* in that metaphorical sense by various authors; which Dr Reid knew, and regarded as a proof of its

being ambiguous, and therefore inconvenient. But we have already seen, that Mr HUME expressly asserted that the existences which we consider when we speak of objects of sense, are "*fleeting copies and representations of other existences* which remain uniform and independent;" and his notion as to the nature of these fleeting copies is farther shewn in another passage, as follows,—"No external object can make itself known to the mind *without the intervention of an image*, and of these images the most obvious of the qualities is *extension*."—(*Treatise on Human Nature*, vol. ii., p. 416.) Has not Mr LOCKE expressly told us, says Mr STEWART, "that *the ideas of primary qualities* of matter are *resemblances of them;* and that *their patterns do really exist in the bodies themselves;*" and did not Mr HUME understand this doctrine in the most strict and literal meaning of words when he stated, "as one of its necessary consequences, that the mind either is no substance, or is an extended and divisible substance, *because the idea of extension cannot be in a substance which is indivisible and unextended?*"—(*Phil. Essays*, p. 553.)

This is surely enough to shew that what LOCKE and HUME called Ideas, had, according to them, a physical (not merely metaphorical) existence, and were essentially distinct from the mere acts or states of the mind itself. And as to BERKELEY, we have the distinct admission of Dr BROWN himself, that he evidently considered ideas "*not* as states of the individual mind, but as separate things existing in it, and capable of existing in other minds, but in them alone."—(*Lect.* vol. i., p. 523.) On which he very justly afterwards observes, that "a mind containing, or capable of containing, *something foreign* within itself, and not only one foreign substance, but a multitude of foreign substances at the same minute, is no longer that simple indivisible existence which we term spirit."—(*Lect.*, vol. i., p. 525.) But these statements are obviously and irreconcileably inconsistent with Dr BROWN'S subsequent assertion, that the word Idea was used by *all* previous authors only metaphorically, and that in proving ideas not to be self-existent things, REID had merely assumed as real what was intended as metaphorical.

It is still more remarkable, that the notion which was taken up by Dr BROWN, of the language of HUME and BERKELEY having been only metaphorical or figurative, is the very same as had been previously hazarded by PRIESTLEY, and previously answered, and shewn to be inconsistent, both with the language of these and other philosophers, and with his own language, by Mr STEWART in his *Philosophical Essays.*

"The following strictures," says Mr STEWART, "on REID'S reasonings against the Ideal Theory, occur in a work published by Dr PRIESTLEY in 1774:—

"Before our author had rested so much upon this argument, it behoved him, I think, to have examined the strength of it a little more carefully than he seems to have done; for he appears to me to have suffered himself to be misled in the very foundation of it, merely by philosophers happening to call Ideas the *images* of external things; *as if this was not known* to be *a figurative expression*, denoting,

not that the actual shapes of things are delineated in the brain, or upon the mind, but only that impressions of some kind or other are conveyed to the mind by means of the organs of sense, and their corresponding nerves, and that between those impressions and the sensations existing in the mind, there is a real and necessary, though at present an unknown connection."

On this passage, Mr STEWART observes, "To those who have perused the metaphysical writings of BERKELEY and of HUME, the foregoing passage cannot fail to appear much too ludicrous to deserve a serious answer. Where did he learn that the philosophers who have happened to call ideas the images of external things, employed this term as a figurative expression?"

He then contrasts it with some of the expressions of LOCKE and of HUME, which I have already quoted, and afterwards proceeds to shew, that it is utterly inconsistent with the following passage in a subsequent work of Dr PRIESTLEY himself,—"Whatever ideas are in themselves, they are evidently produced by *external* objects, and *must therefore correspond to them;* and since many of the objects or archetypes of ideas are divisible, it necessarily follows, that the ideas themselves are divisible also. The idea of a *man*, for instance, could in no sense correspond to a man, which is the archetype of it, and *therefore could not be the idea of a man*, if it did not consist of the ideas of his head, arms, trunk, legs, &c. *It therefore consists of parts*, and is consequently divisible. And how is it possible that a thing (be the nature of it what it may) that is divisible, should be contained in a substance, be the nature of it likewise what it may, that is indivisible." If the "archetype of ideas have extension, the ideas *expressive* of them must have extension likewise; and therefore the mind in which they exist, whether it be material or immaterial, must have extension also."

"No form of words," says Mr STEWART, "could have conveyed a more unqualified sanction than he has here given to the old hypothesis concerning Ideas, —a hypothesis which he had before asserted to have been never considered by any philosopher but as a figurative mode of expression; and which, when viewed in the light of a theory, he had represented as an absurdity too palpable to deserve a serious refutation."—(*Phil. Essays*, p. 554.)

Mr STEWART afterwards refers, in the same work, to the passages which I shall presently quote from Dr REID, as containing the true statement of his reply to the sceptical argument of BERKELEY and HUME; founded, as he believed it to be, on the language of LOCKE, and of what have since been termed the Sensational School of Metaphysicians; and farther refers to several prior authors, particularly BAXTER in this country, and D'ALEMBERT in France, as having stated and pointed out the importance of the same principle that REID did, but without illustrating it sufficiently.—(See *Phil. Essays*, Notes and Illustrations, p. 55.)

I cannot conceive that Dr BROWN should have made the statements which I have quoted, and which Sir JAMES MACKINTOSH and others have approved, as to

the language of HUME and others having been merely metaphorical,—and should have pronounced, on that ground, the claim of Dr REID to a refutation of their scepticism to have been inadmissible, without making the least reference to Mr STEWART'S answer to the very same objection when made by PRIESTLEY, and without mentioning the passages in REID and other authors to which Mr STEWART had referred, as the true exposition of this argument,—if he had read or reflected on those passages in Mr STEWART'S writings; and yet they were published in his *Philosophical Essays* in the summer of 1810, *i. e.*, some months before the first course of lectures which Dr BROWN delivered as Professor of Moral Philosophy in Edinburgh. But those who are aware of the peculiar sensitiveness of Dr BROWN'S physical constitution, of the painful effort which he made to prepare his lectures for that first course, and of his unwillingness at any subsequent time to revert to that part of his subject, on which indeed his lectures subsequently underwent only verbal alterations, will feel no difficulty in understanding, that one of Mr STEWART'S essays (the second in the volume of *Philosophical Essays* published in 1810), and the notes to it, may either not have been read, or read so hastily as to have been speedily forgotten by Dr BROWN, and never recurred to his mind when he was either revising his lectures, or preparing the short abstract of this portion of them which was published only a few months before his death.

It is only doing justice to the candour and discernment of the late Dr WELSH to observe, that in stating, in his life of Dr BROWN, the argument drawn from what he considered to be only the metaphorical use of the term Idea, in opposition to Dr REID'S argument, he took notice of what he termed "the defence of REID'S views, contained, as if by anticipation, in Mr STEWART'S Philosophical Essays,"—*i. e.*, contained in a work published before Dr BROWN'S lectures containing that argument were delivered, if not before they were written. It was perhaps unfortunate that Dr WELSH merely referred to Mr STEWART'S argument, and to some of the extracts from former authors by which it was supported, without quoting them, or expressing any opinion of his own on the subject. (See *Life of* Dr BROWN, p. 259.) And it is still more unfortunate that Mr STEWART himself, in the essay in question, and the notes to it, although he refers to the passages in REID'S writings, which I shall presently quote, as containing the true statement of his argument, did not quote any of his words.

IV. But farther, keeping always in mind that Dr REID'S avowed object was, not to prove by reasoning the existence of the material world (which he expressly avowed to be impossible), but only to confute the argument which represented that belief as an absurdity, I would observe that it was quite a misconception to suppose, as both Dr BROWN and Lord JEFFREY did, that "the destruction of the Ideal Theory" was what constituted "the confutation of the reasoning of BERKELEY and HUME." Dr REID was perfectly aware that the word Idea, in that

argument, *might* be used only metaphorically, as asserted by Dr Brown; and his answer to the argument is expressly so stated as to be equally applicable, *whether the word is used in the literal* or *the metaphorical* sense. His main argument is directed, not necessarily against the supposition of intermediate existences, called Ideas, but against the supposition that the material world, if it exists, *must be the express image or representation* of the mental acts by which we are made acquainted with it.

It will be observed, that there is no *absurdity* in saying that a Sensation, or any other mental act, uniformly attends the impression on any of our organs, made by any particular external object or quality, that it indicates to us its existence, and suggests to us, or enables us to form, a notion of its nature. The absurdity lies only in supposing, that any mental act can be the exact image or representation of anything but another mental act, in the same or another mind; and Dr Reid was at pains to point out that his reply to this is independent of any particular meaning, and even of the use, of the word Idea.

He says,—"To prevent mistakes, the reader must be reminded, that if by Ideas are meant only the acts or operations of our minds in perceiving, remembering, or imagining objects, I am far from calling in question the existence of *those acts;* we are conscious of them every hour of life, and I believe no man of a sound mind ever doubted of their existence."—(*Intellectual Powers*, p. 197.)

This shews that he was aware that the term *Ideas might* be used *metaphorically*, "or as illustrative language" for acts or states of mind.

Then he says, in stating his argument against Bishop Berkeley,—"That we have many Sensations by means of our external senses, there can be no doubt; and if he is pleased to call these Ideas, there ought to be no dispute about the meaning of a word." "But," says Bishop Berkeley, "by our senses we have knowledge *only* of our Sensations or Ideas, call them which you will; and these, which are attributes of Mind, can have no resemblance to any qualities of a thing which is inanimate. *I allow him to call them which he will*, but I would have the word *only* in this sentence to be well weighed, because a great deal depends upon it. For if it be true that by our senses we have the knowledge of our sensations *only*, then his system must be admitted, and the existence of a material world must be given up as a dream."—(*Collected Works*, p. 290.)

Then he goes on to give the proof, that the mental act in question, however rapid, is more complex than it had been represented,—that our minds are so constituted as to form uniformly certain definite *notions on occasion of certain sensations being excited in us*,—to draw certain inferences, or pass certain judgments, as to the existence and certain qualities of things external to ourselves,—that it is to these *perceptions* that the intuitive belief of independent existence is attached, —and that these we at once perceive, when our attention is fixed on them, to be *essentially distinct from the sensations*, and *to resemble them in no particular*. This per-

ceived or felt *dissimilarity* of the Notions or Conceptions, as to external existences, which are formed in the mind, from the Sensations which suggest or introduce them into the mind, is what both REID and STEWART relied on, as the answer to the sceptical argument of HUME and BERKELEY; and is not once noticed either by Dr BROWN or Lord JEFFREY.

This argument is given at more length by REID as follows:—"It is true we have feelings of touch, which every moment present the notion of Extension or Space to the mind: but how they come to do so is the question; for those feelings do *no more resemble extension, than they resemble justice or courage;* nor can the existence of extended things be inferred from those feelings, by any rules of reasoning; so that the feelings we have by touch can neither explain how we get the notion, nor how we come by the belief, of extended things.

"What hath imposed upon philosophers in this matter is, that the feelings of touch, which suggest primary qualities, *have no names*, nor are they ever reflected upon. They pass through the mind instantaneously, and serve only to introduce the notion and belief of external things which, by our constitution are connected with them. They are natural signs, and the mind immediately passes to the thing signified, without making the least reflection upon the sign, or observing that there was any such thing."

"Let a man press his hand against the table, *he feels it hard.* But what is the meaning of this? The meaning undoubtedly is, that he hath a certain feeling of touch, from which he concludes, without any reasoning, or comparing ideas, that there is something external really existing, whose parts stick so firmly together, that they cannot be displaced without considerable force.

"There is here a feeling, and a conclusion drawn from it, or some way suggested by it. The hardness of the table is the conclusion, the feeling is the medium by which we are led to that conclusion. Let a man attend distinctly to this medium and to this conclusion, and he will perceive them to be *as unlike* as any two things in nature. The one is a sensation of the mind, which can have no existence but in a sentient being, nor can it exist one moment longer than it is felt; the other is in the table, and we conclude, without any difficulty, that it was in the table before it was felt, and continues there after the feeling is over. The one implies no kind of extension, nor parts, nor cohesion; the other implies all these. Both, indeed, admit of degrees, and the feeling, beyond a certain degree, is a species of pain, but adamantine hardness does not imply the least pain."—(*Collected Works*, p. 125.)

The substance of this argument is, that the external existences, or qualities of external objects, of which our knowledge is acquired by the senses, are *not* felt or apprehended by us as prototypes or patterns of the sensations, through which they are made known, *but perceived to differ from them in every particular;* as in the case of the notion of Extension or Space, formerly mentioned,—formed during the exercise of various senses, *i. e.*, in consequence of the excitement of various

sensations, but which is no sooner apprehended than it "swells in the human mind to Infinity," to which notion certainly no human sensation can bear any resemblance; and no one has rightly apprehended the argument, or can be aware of the importance ascribed to it by Mr STEWART, as opposed to what has been since called the Sensational School of Metaphysicians, who has not adverted to this absolute and essential *dissimilarity* of the sensations, from what Dr REID calls "the Perceptions," and Dr BROWN, the "Associations and Inferences," consequent on those sensations. Those who do advert to that dissimilarity must perceive that our conception of, and belief in, the external and independent existence of space and matter,—although a mental act, and a complex one, and involving one of those intuitive judgments, as to the existence and authority of which we have seen that REID, STEWART, and BROWN, are fully agreed,—is perfectly distinct from the sensation by which it is excited, and involves no such *absurdity* or contradiction in terms, as the assertion that a sensation or other mental act, can be the *exact image and representation* of anything that is not mental; and therefore, that the sceptical argument of BERKELEY and HUME, founded on that supposed absurdity, and *necessitating* our *departure*, as HUME expressed it, from the instincts of nature, as to the evidence of the senses, falls to the ground.

The same observation applies to the notice of this subject by MORELL, in his review of the Scottish Philosophy, who says, that Dr REID "does not appear to him to have dealt a complete and effective blow against HUME's argument respecting the material world;" because, he says, "the sceptic may urge, with no little force, that although we must admit the reality of our own personal or subjective ideas (*i. e.*, of the objects of consciousness), yet it still remains to be proved, that our perceptions, however clear, and our beliefs, however strong they may be, internally, have reference to any object out of, and distinct from ourselves." REID, he says, deprived himself of the "power of answering this final argument, by maintaining that Perception is altogether an act of Mind. So long as perception is regarded as only a subjective process (*i. e.*, an act of mind of which we are conscious), and an idea defined to be the act of the mind in making itself acquainted with external things, we are unable to point out to the sceptic what he demands, viz., a clear passage from this subjective activity of the mind to the outward and material reality."—(*Morell's Philosophy*, vol. i., p. 287.)

Now, if this author had rightly comprehended the argument of REID,—which I apprehend he must have known only from the account of the controversy given by Dr BROWN,—he would have known that REID considered the clear passage from the act of Perception in the mind to the material reality, to be precisely similar to the passage from our consciousness of to-day to our recollections of yesterday; *i. e.*, to rest on one of those principles of Intuitive belief, the *existence* and *authority* of which are admitted by himself and by BROWN, as well as by REID; and to be from its own nature incapable of any other proof.

But if he had rightly comprehended the argument of HUME and BERKELEY, he would have known, that they not only demanded a clear passage from the mind to the material object, but maintained that it is *absurd* to assert that any such passage exists; because, as we have seen, they said that by our senses we have the knowledge *only* of our Sensations or Ideas, call them which we will, and nothing can possibly resemble a sensation, except another sensation in the same or another mind; to which assertion and consequent imputation of absurdity it was that Dr REID opposed *the fact* in the natural history of the mind, that by our senses we have the act of Perception excited in our minds, involving, as all admit, an intuitive belief; and which, particularly in the case of the primary qualities of matter, is distinctly felt by us to be separate from the sensation by which it is excited, and utterly incapable of comparison with it.

But it is equally obvious, that this perception and belief, being regarded as an ultimate fact, or as containing in itself an ultimate fact in our mental constitution, like every other *ultimate fact, physical or moral,* involves *a mystery;* and one on which we must accustom our minds to dwell, if we would form to ourselves any clear notions as to the constitution of the human mind, or its connection with the Divine Mind. It is only by a kind of Instinct, as expressed by D'ALEMBERT, but it seems better to use the term Intuition,—"prior to Reason, and superior to reason, —that the human mind can overleap the gulf that separates the visible world, from the percipient soul."

I have already shewn that by the admission of Dr BROWN himself, in all departments of human knowledge, we meet with such ultimate facts and principles of intuitive belief, any farther explanation of which can be given us only by "the great teacher, Death;" and very little reflection is sufficient to shew that the only objects which we can propose to ourselves in any inquiry which lies on the confines of Matter and Mind,—in which both physical changes and mental acts are concerned,—are to ascertain the exact phenomena on each side of the line of demarcation, the precise conditions under which they take place, and the precise laws by which they are determined,—the mode of union being beyond our comprehension. But so restricting our objects of inquiry, we may confidently assert, that enough has been ascertained in regard to the mental operations consequent on the impressions on our senses, as well as to their physical conditions, to form an important body of science, and furnish conclusions of the highest interest.

I think myself justified by what has been stated, in affirming that in so far as Dr BROWN thought he had detected an essential error in the reasonings of Dr REID on this subject, he had deceived himself; and that in so far as he made a real advance, in our knowledge of the manner in which the notion of the primary qualities of matter is formed in the human mind, he proceeded strictly in accordance with the principles of REID and STEWART; and therefore, that it is only

retarding the progress of knowledge on the subject, to represent these authors as at variance with one another. In fact, it appears to me, that his doctrine on this subject, referring to the general Law of Belief in the permanence of the order of Nature, is substantially the same as that of STEWART and TURGOT, and that the only real addition which he made to our knowledge on the subject, consists in explaining the province of the *muscular sensations*, as distinguished from those sensations that result merely from *impressions on the cutaneous nerves*, with which they had generally been confounded under the name of Sensations of Touch; and in connection with them, the importance of *the idea of Time*, in communicating the information on which our notions of the Primary qualities of Matter are founded. This is the same distinction as is expressed by several French physiologists by the terms Tact and Toucher; and it appears from the learned researches of Sir WILLIAM HAMILTON, that it had been clearly pointed out by various other authors, ancient and modern; but I have no doubt that it was original on the part of Dr BROWN.

In concluding these remarks on this part of the Philosophy of Dr BROWN, I see no objection to my stating, what I am very certain was the case, that the repugnance which he felt towards the peculiar doctrines of Dr REID, was in reality not so much on the score of judgment as of *taste*. His own taste in literature was peculiar,— it was founded in a great measure on the classical models,—and he was even more ambitious than Mr STEWART, of combining the reputation of a scholar and elegant writer with that of an acute metaphysician. The perfect simplicity of the language, the total absence of fancy, and the homeliness of many of the illustrations, in the writings of Dr REID, were distasteful to him; and I cannot but consider, therefore, his objections to the doctrines there laid down, as an illustration of the truth of the observation on his own scientific character, which I have often heard from my Father; who had the highest admiration, both of the acuteness of his intellect, and of the purity and elevation of his moral principles, but used to speak of him as the man of the most fastidious taste that he had ever known.

It has been often observed, that the intellectual opinions, even of the men who take most pride in the exercise of their understandings, are very often more or less guided by their tastes and feelings; and in regarding the prejudice which may be detected in the writings of Dr BROWN, against the phraseology and the doctrines of REID, as an instance of the reaction of independent thought against mere authority, and of cultivated taste against the imputation of vulgarity, I do not think I do injustice to the memory of either of these illustrious men.

Sir WILLIAM HAMILTON, as I already mentioned, expresses himself strongly as to the doctrine of REID regarding the formation of the notion of the primary qualities of matter, as so far from "being a confutation of Idealism, affording it the best of all possible foundations;" but then he explains this by saying that he

means only "that simpler and more refined Idealism, which views in ideas only modifications of the mind itself," *i. e.*, only what Dr Brown, in one passage already quoted, regarded the ideas of Berkeley, viz., as a metaphorical way of expressing acts or states of the mind; in which sense Dr Reid, as we have already seen, said he did not object to the use of the term, although he preferred another phraseology; and using it in that sense, we have seen that his argument against Hume and Berkeley is independent of any objection to the term.

Dr Reid goes no farther in explaining the manner in which we acquire the knowledge of extension or space, than to say, that it is a Perception, or a notion suggested to the mind by certain of our sensations, distinctly formed in the mind, and in which, when we analyse it as minutely as we can, we find the belief of external independent existence to be an essential element. Sir William Hamilton considers the conception of Space to be a native form, or necessary condition of thought; but that we have an immediate perception of something extended, *i. e.*, invested with this quality, and which is independent of us. (See Notes to pages 126 and 324 of *Collected Works, &c.*)

I cannot perceive that there is anything more than a verbal distinction between these forms of expression; but if there be a real improvement in the latter form, it seems to me that it is sufficiently provided for by Dr Reid's admission, that a finer eye may trace the labyrinth farther than he has done; but that in the meantime "there is no harm done" in resting on the position of Reid as to that belief; and acquiescing in his reflection, that "if we are deceived in it, we are deceived by Him that made us, and there is no remedy."

It is stated by Morell, and I believe is the opinion of others who have made a study of recent German works on metaphysics, that the works of Dr Reid and all other Scotch metaphysicians, although accurate, so far as theygo, in "investigating and classifying the more obvious phenomena of the mind, as they appear in the individual, are deficient in not having gone a step farther, and discovered the very laws of our mental constitution, on which our primitive beliefs rest; that they might have sought the groundwork of our universal notions in the depths of our own being, and thus referred all the principles of common sense, all the primary laws of belief, back to their source in the subjective forms of the understanding and the reason (*Historical and Critical View, &c.*, vol. ii., p. 64); that in investigating the mental phenomena, our object should be to discover, not merely the reality of certain principles, but their necessity,—not merely the law of operation, but the reason of that law" (*Ditto*, p. 53); and that this is to be done, not by mere induction, but "by scanning the contents of our consciousness by the power of reflection, whereby we are enabled to catch the very forms of our inward activity."—(P. 52.)

In forming this opinion, I cannot help thinking that this very learned and estimable author has deceived himself; and that no such advance has been made

since the time of STEWART and BROWN, either in the mode of inquiry, or in the results of inquiry on the subject. But all that I wish to observe on that point is this, that those speculations avowedly relate to subjects *ulterior to those* on which REID and STEWART exerted their minds; that they do not *stand opposed* to the doctrines of REID or STEWART as to the exercise of the senses, and the mental acts thence resulting, but are regarded as an addition to these doctrines; and therefore, that, whether admitted or rejected, they ought not to interfere with our appreciation of the truth or importance of the principles regarding our mental constitution, which they had laid down, and which these authors substantially approve.

In particular, while I cannot but admire the sublimity of the Theological inferences which MORELL has stated as resulting from the study of the Mind as he directs it, I cannot think it necessary to go farther into the subject than REID and STEWART had done, in order to draw from it inferences as satisfactory to the intellect, and as consoling to the heart of man, as can be drawn from any unassisted human contemplation or reflection.

It is stated, indeed, by MORELL, that the great argument of Natural Theology, drawn from the observed adaptation of means to ends,—of which I may observe, that the principle of the adaptation of the construction of animals to the *conditions of their existence*, so well illustrated since their time by CUVIER, OWEN, and their followers, is distinctly an example,—has been well set forth by all the Scottish School of Metaphysicians, from REID to CHALMERS; but that two subjects connected with it ought to have been taken up more fully, viz., 1*st*, the origin of the idea of Absolute Power, or of the Divinity in the mind; and, 2*d*, the relation of the Divine Power, or Energy, to Man on the one hand, and to Nature on the other.—(*Modern Philosophy*, vol. ii., p. 71.) The first of these, I think, may really be regarded as a defect in the philosophy of Dr BROWN, who rested the great argument of Natural Theology *exclusively* on the observed adaptation of means to ends;—and did not admit as a part of that argument, the formation of the notion of Efficient Cause, as distinguished by REID and STEWART from Physical Cause;—and that it was a defect seems to me distinctly shewn by an observation of his own, which I cannot reconcile with the doctrine which he had laid down on this subject.

The passage to which I allude is that where he speculates, with his usual eloquence and fancy, on the emotions which would be excited in the human race if it were possible that they should come to maturity in a world of darkness, and the sun were then suddenly to arise on their sight. "The very atheists of such a world," he says, "would confess that there is a Power that can create." Now he surely could not have maintained that this instantaneous inference would imply a process of reasoning, by which the supposed atheists might satisfy themselves that some particular object was in view, which could only be attained by an influence of the sun, and therefore saw in this sudden and striking change an adaptation of

means to ends. If not, then this inference must be allowed to establish the fact of the observation of *sudden and striking change* introducing into the mind the notion of a "Power that can create;" and I cannot conceive that the notion arising in the mind from the contemplation of these circumstances, and which is here expressed by that term, excludes the idea of Arbitrary Will. If it includes that idea, it cannot be correctly expressed by the definition given by Dr Brown of Power, which he allowed to be a simple idea, formed by intuition, but was at great pains to prove to mean merely "Invariable Sequence, having reference not only to the past, but to every future case." (*Observations on Cause and Effect*, p. 101.) I cannot help thinking, therefore, that this illustration is sufficient to establish the reality of the idea of Absolute Power, or of Efficient Cause, as distinguished from Physical, which was maintained by Reid and Stewart, but contested by Brown. This criticism of Morell, therefore, I believe to be justly applicable to Brown, but certainly not to either of his predecessors.

The second principle stated by Morell as having been neglected by the Scottish School of Metaphysicians, is so beautifully expressed by himself, that I cannot help quoting his words. The principle in question, he says, should be a comment "on the scriptural doctrine, that in God we live and move and have our being. This is a truth which has more meaning in it than the cursory reading of it gives us; it evidently has a reference to the mysterious dependence of the human spirit upon the Divine, shewing us that we are all emanations from the Divine Essence, and although gifted with a distinct personality, yet that we are but waves in the great ocean of existence, ever rolling onwards to our eternal home."—(Morell, vol. ii., p. 72.)

Now if the doctrine of Reid and Stewart really excluded from the reflections of the metaphysician so elevating and consoling a train of thought as this, we might regard them as truly and lamentably defective; but I confidently maintain, that all that is necessary is to let the mind dwell for a little on the principle of Intuitive Perception of Truth, illustrated by them as well as by Brown, and in connection with it, on the facts regarding our mental constitution which they have explained, in order to be satisfied of the truth and justice of the sentiment which I have quoted, and which, indeed, in all ages has suggested itself to the most profound thinkers in this department of science.

"Intuition or Inspiration," says Victor Cousin, "is in all languages distinct from reflection or from Reasoning. It is the simple perception of Truth; I mean of essential and fundamental truths, without the intervention of any voluntary or personal act. This intuition *does not belong to us.* We are there, when the act is performed in our minds, simply as spectators, not as agents; all our action consists in having the consciousness of what is going on. Our perception of simple and primary truths may be separated, therefore, from the fallible reason of man, and referred to that Reason which is Universal, Absolute, Infallible, and Eter-

nal, beyond the limits of Space and Time, above all contact with error or disorder,—to that Intelligence of which ours, or that which makes its appearance in us, is but a fragment,—to that Mind, pure and incorruptible, of which ours is only the reflection."

These are sentiments which adorn and dignify Science, but I beg to ask, whether they are not in exact accordance with the doctrines of all our esteemed Scottish metaphysicians,—nay, whether they may not be regarded as commentaries on the simple text already quoted from Reid, that all our knowledge of what exists, or ever did exist, traced to its source, is found to come by a channel, which is open to those who cannot reason, *i. e.* (the word reason being ambiguous), who cannot exert the voluntary power of Reasoning, but only yield to the influence of the faculty of Intuition implanted in their nature,—" that we are led to it in the dark, and know not how we came by it,—and that the wise and humble will simply take it as the gift of Heaven, and try to make the best use of it." According to the doctrine of Reid, all those mental acts in which Intuitive Belief is involved, and on which all knowledge is directly or indirectly founded, although we call them *ours*, are ultimate facts in Nature, independent of our will, and beyond our comprehension; and this conclusion, so far from humbling the human mind, establishes a more intimate connexion between man and his Creator than can be inferred from any other facts in nature.

When we attend to the meaning, and trace the applications of this principle of Intuition, necessarily involved in the only account we can give of our perceptions, and of all our knowledge; when we observe the still more striking exercise of this power in animals, whose sensations suggest to them, prior to all experience, the true distance, direction, and size of external objects, certainly neither contained in, nor deducible by any process of reasoning from, the intimations of sense; when we reflect on the equally mysterious nature, and yet on the proved fidelity (in the healthy state) of the evidence of Memory, essential, not only to all reasoning, but to all definite voluntary action of men and animals; when we consider the nature and the tendency of those Instinctive propensities or Impulses, which are excited in us and in all animals during the exercise of the senses, and which are equally requisite and equally effective, in attaining objects essential to our existence, as are the vital properties of muscles and of nerves;—in all these cases, we shall perceive that truths are made known to us in a manner *absolutely mysterious*;—by means of impressions on our senses, but " no more contained in sense, than the explosion of a cannon in the spark that gave it fire."

And when we farther observe, that the *actions* which are prompted by the Instincts and Volitions both of animals and of men, consequent on the knowledge thus acquired, are all conducive to certain important ends, intelligible to us after observation and reflection, but scarcely ever in the contemplation of the agents at the moment, we can express these facts only by saying that both men and animals

are the depositaries or recipients of certain portions of the knowledge, and the instruments of certain of the designs, of the superior Mind to which they owe their existence. And the "creed of the sceptic," shewing that it is by no exertion of our own reason, and indeed by no process of which we can give any account, that so many truths are made known to us, and so many useful acts suggested to us, becomes an essential part of the short and simple train of reasoning by which that connection is inferred, and which may be thus stated.

Much of the knowledge which is part of the constitution of our minds, or which is awakened in us by the exercise of our senses, is not our knowledge; it is neither contained in our sensations, nor deducible by any reasoning from them, nor subject to our will, nor acquired by our experience or recollection; yet it is found to be accurate, and the possession of it to be useful and necessary to us.

So also, many of the actions which we perform, which are fitted to the attainment of ends important to us, and obviously performed in anticipation of those ends, are not prompted by any such anticipation of ours. The will which performs them is ours, but the knowledge of their consequences, with a view to which they are performed, is not ours. "Man," says GUIZOT, "is a workman, intelligent and free, but the work in which he is employed is not his; he sees the intention of it only when it has been so far accomplished, and even then, sees it only imperfectly." In so far, therefore, as the observation of these phenomena of our minds leads to an inference of Intelligence,—and if it does not, we have no grounds for ascribing intelligence to any of our friends or fellow-citizens,—it must be intelligence prior to ours, and superior to ours, and on which ours is dependent.

It seems to me, that it is quite unnecessary to make any additions to the doctrine, which we have seen was the common doctrine of REID, of STEWART, and of BROWN, as to the existence and authority of the Intuitive Principles of Belief,—and hardly necessary to illustrate this farther than the two former authors had done,—to justify the whole of this inference. But farther, it is precisely the same inference which we find, if not so fully illustrated, at least distinctly expressed as resulting from the contemplation of our mental constitution, by much earlier authors. It was the same idea that was expressed by the three memorable words of CICERO, "Homo Rationis Particeps" (not possessor); and by the positive assertion of PLATO,—that nothing is more certain than that a part of every man's mind existed before he did. Nay, in an earlier record than either of these, of the first metaphysical reflections of the human race, in those very words from the Book of Job which Dr REID took as the motto of his Work on the Intellectual Powers, there is, as we are assured by an eminent Hebrew scholar, a meaning more exactly in accordance with the leading principle of Dr REID's Philosophy, than, in selecting that motto, he was probably aware. The words are, "Who hath put wisdom in our inward parts?" but the more precise expression of the meaning, we are assured, is,

Who hath given to our inward parts, or to our thoughts, the *security of knowledge?* *i. e.*, What security have we of the truth or reality of knowledge, which we can trace no farther than certain impressions made on, and changes excited in, our own minds? and the only answer which the context will admit is, that we have no security but the will of our Maker, whereby our minds are so constituted, that Belief is an essential component part of the acts which they uniformly perform, or the states which they uniformly assume, under certain circumstances; which in this as in other departments of knowledge, we can go no farther than to specify and describe.

I may just add, that there are two questions in Physiology, which have attracted much attention of late years, and of which I think a just view cannot be taken, without a previous accurate discrimination of those mental phenomena which Dr Reid distinguished as Sensations, Perceptions, Recollections, and Voluntary Efforts. The first regards the appropriation of the larger masses of the nervous system to their specific uses; and *first*, to those muscular movements which are generally now described as depending on the Reflex action of the Spinal Cord, *e. g.*, those concerned in Respiration, Deglutition, and the various actions associated with those, and which have been ascertained, particularly by the experiments of Flourens, to have no dependence on the hemispheres of the Brain or Cerebellum; and, accordingly go on, even for months together, in animals of which both the brain and cerebellum have been extirpated; so that the term Reflex Spinal Action may be properly applied to them, instead of the older term Sympathetic Action, by which they were long previously distinguished. But it is equally certain, and was indeed established long ago, by Dr Whytt, that another principle is here concerned, which goes so far in explanation of the fact, not only that muscular contractions are excited by this reflex action in these circumstances, but that those muscles are *selected* for this purpose, which are capable of performing the motions, and successions of motion, requisite for the particular end to be attained in each case,—one set of motions, *e. g.*, for breathing, another for coughing, another for deglutition, another for vomiting, &c. That principle is the existence and the peculiarity of the Sensation, preceding and attending the performance of each of these motions. The proof of this is, that in many of these cases, the same sensation may be excited by impressions made on the sensitive nerves of different parts, in each of which the same reflex or sympathetic movement follows; while in others, different sensations result from varied impressions made on the sensitive nerves of the same parts, and in these different reflex actions are excited. It appears, therefore, that it is by the sensations preceding and attending them, that the nature and intensity of these reflex movements are determined, at least in the ordinary exercise of these functions; and that those parts of the nervous system, and those only, which are found to be essential to those movements, must be those which are concerned

in the mental act of Sensation; which term is now habitually used in Physiology, in exactly the same sense as Dr Reid understood it.*

Accordingly, I think it may be confidently asserted,—although many physiologists speak of reflex actions as not necessarily connected with sensation,—that the correct expression of these phenomena was truly given by Cuvier, in his Report to the Academy of Sciences on the Memoir of Flourens in 1822,—that an animal of which brain and cerebellum have been destroyed, and the medulla oblongata only remains in the cranium, is still capable of feeling Sensation, and of performing those acts which are immediately linked with sensation; and, indeed, is dependent on sensations for the preservation of its life, which, in these circumstances has been preserved for many months,—because it still breathes, and still swallows what is put into its mouth, &c.; but that, in these circumstances, it has *no recollection of past sensations*, shews none of its usual habits, cannot seek for food, or even avoid obstacles placed in its way; in short, is reduced to a state of stupor, more or less profound. In such an animal, of course, those judgments consequent on sensations, to which both Dr Reid and Dr Brown gave the name of Perceptions, and all more strictly Mental recollections and acts consequent on these, are manifestly suspended; and thus we acquire the certainty that the distinction of Sensations and Perceptions, which we have seen to be of so much importance when considered metaphysically, is fully confirmed by physiological inquiries, and, I may add, by researches in Comparative Anatomy; which have proved that the Cerebro-Spinal Axis is the part of the animal structure which furnishes the conditions, and supplies the instrument, of the ones et of mental phenomena; and the Brain and Cerebellum, superimposed on that structure within the skull, are those which minister in like manner to the other. This is, in fact, the only conclusion, as to the appropriation of these different parts of the larger masses of the nervous system to different acts or states of mind, which has ye tbeen satisfactorily established; and if we regard it, as I think we may, as an important guide to farther inquiries as to the use of the different portions of the physical instrument concerned in Thought, we ought also to regard it as an important indication of the value of the distinctions among the acts of thought, with which these different portions of the nervous system are connected.

The other question is, as to the degree of modification which the exercise of the Senses, as well as other mental acts may undergo, in several anomalous conditions of the living body, especially in that to which the term Somnambulism, Extâse, or Clairvoyance, has been applied. On this subject, which can only be elucidated by very carefully-conducted observations,—always likely to be impeded by peculiar

* See Observations on the Physiological Principles of Sympathy, by the present Author, in Edinburgh Medico-Chirurgical Transactions, vol. ii.

sources of fallacy, especially by that extraordinary propensity to deception which medical men so continually encounter in this part of their studies,—it would be wrong in me, not having had sufficient opportunities for making such observations, to pronounce any decided opinion; but I think it only due to the memory of Dr Reid to point out, that in one part of his writings he has distinctly asserted, —and indeed, consistently with his principles, could not fail to perceive,—the possibility of such a modification of the exercise of the senses, as has been expressed by the term Clairvoyance; and left it, therefore, as a question to be decided simply by experience, whether or not such modification may occur.

"Our power of perceiving external objects is limited in various ways, and particularly in this, that without the organs of the several senses, we perceive no external object. We cannot see without eyes, nor hear without ears; and it is not only necessary that we should have these organs, but that they should be in a sound and natural state.

"All this is so well known from experience, that it needs no proof; but it ought to be observed, that we know it *from experience only*. We can give no reason for it, but that such is the will of our Maker. No man can shew it to be impossible for the Supreme Being to have given us the power of perceiving external objects without such organs.

"If a man were shut up in a dark room, so that he could see nothing but through one small hole in the shutter of a window, would he conclude that the hole was the cause of his seeing, and that it is impossible to see in any other way? Perhaps, if he had never in his life seen but in this way, he might be apt to think so; but the conclusion is rash and groundless. He sees, because God has given him the power of seeing; and he sees only through this small hole, because his power of seeing is circumscribed by impediments on all other hands."—(Reid's *Collected Works*, p. 246.)

On this passage we have the following note by Sir William Hamilton:—"However astonishing, it is now proved beyond all rational doubt that, in certain abnormal states of the nervous organism, perceptions are possible through other than the ordinary channels of the senses."

This is expressing a decided opinion on the question, on which I have said that I do not think myself qualified to judge; but I beg to express my perfect concurrence with Sir William Hamilton in thinking, that, consistently with the principles of Dr Reid, it is a question on which no *à priori* opinion is admissible, and which observation and experiment alone can decide.

XXXIV.—*Summation of a Compound Series, and its Application to a Problem in Probabilities.* By Bishop Terrot.

(Read 21st February 1853.)

The series proposed for solution in the following paper is—

$$\left.\begin{array}{l}(\overline{m-q}\,.\,\overline{m-q-1}\,.\,.\,.\,.\,.\,m-\overline{q+p}+1)\times(1\,.\,2\,.\,3\,.\,.\,.\,.\,.\,q)\\ +(\overline{m-q-1}\,.\,\overline{m-q-2}\,.\,.\,.\,m-\overline{q+p})\quad\times(2\,.\,3\,.\,4\,.\,.\,.\,.\,.\,q+1)\\ \vdots\\ +(p\,.\,p-1\,.\,.\,.\,.\,.\,.\,.\,1\quad\times(\overline{m-p}\,.\,\overline{m-p+1}\,.\,.\,m-\overline{p+q}+1)\end{array}\right\}\quad .\qquad (A)$$

The law of this series is manifest. Each term is the product of two factorials, the first consisting of p, and the latter of q factors. And in each successive term, the factors of the first factorial are each diminished by one, and those of the latter increased by one.

Let there be a series, $X_n\,Y_1+X_{n-1}Y_2+\,.\,.\,.\,.\,.\,.\,.\,.\,.\,.\,.\,X_1\,Y_n$

where, $Y_2=Y_1+\Delta_1$, $Y_3=Y_2+\Delta_2=Y_1+\Delta_1+\Delta_2$, and so on.

Then the series $=X_n\times Y_1$

$$+X_{n-1}\times\overline{Y_1+\Delta_1}$$
$$+X_{n-2}\times\overline{Y_1+\Delta_1+\Delta_2},$$
$$\&c.$$
$$=\Sigma X_n\times Y_1+\Sigma X_{n-1}\times\Delta_1+\Sigma X_{n-2}\times\Delta_2+\&c.$$

where ΣX_n means the sum of all the terms of X from X_1 to X_n inclusive.

Let us then, in the first place, take the differences of the second factorials—

$$-(1\,.\,2\,.\,3\,.\,.\,.\,.\,.\,q)+(2\,.\,3\,.\,4\,.\,.\,.\,.\,.\,q+1)=(2\,.\,3\,.\,4\,.\,.\,.\,.\,.\,q)\,.\,q$$
$$-(2\,.\,3\,.\,4\,.\,.\,.\,q+1)+(3\,.\,4\,.\,5\,.\,.\,.\,.\,.\,q+2)=(3\,.\,4\,.\,5\,.\,.\,.\,.\,.\,q+1)\,.\,q$$
$$\&c.\qquad\qquad\qquad\&c.$$

Hence the sum of the whole series=

$$\left.\begin{array}{l}\Sigma\,(m-q\,.\,m-q-1\,.\,.\,.\,.\,.\,.\,m-\overline{p+q}+1)\,.\,1\,.\,2\,.\,3\,.\,.\,.\,.\,q-1\,.\,q\\ +\Sigma\,(m-q-1\,.\,\overline{m-q-2}\,.\,.\,.\,.\,.\,.\,m-\overline{p+q})\,.\,2\,.\,3\,.\,4\,.\,.\,.\,.\,.\,q\,.\,q\\ +\Sigma\,(m-q-2\,.\,m-q-3\,.\,.\,.\,.\,.\,m-\overline{p+q}-1)\,.\,3\,.\,4\,.\,5\,.\,.\,.\,.\,.\,q+1\,.\,q\\ \qquad\&c.\qquad\qquad\&c.\end{array}\right\}\qquad (B)$$

Integrating then each line separately, we have the sum

$$\left.\begin{aligned} &= \frac{q}{p+1} \cdot m-q+1 \,.\, m-q \,.\,.\,.\,.\, m-\overline{p+q}+1 \times 1.2.3.\,.\,.\,.\,.\,.\, q-1 \\ &+ \frac{q}{p+1} \cdot m-q \,.\, m-q-1 \,.\,.\,.\,.\,.\,.\, m-\overline{p+q} \times 2.3.4.\,.\,.\,.\,.\,.\, q \\ &+ \frac{q}{p+1} \cdot m-q-1 \,.\, m-q-2 \,.\,.\,.\, m-\overline{p+q}-1 \times 3.4.5.\,.\,.\,.\,.\,.\, q+1 \end{aligned}\right\} \quad . \quad (C)$$

&c. &c.

If again we treat this form as we have done the original, by taking the differences of the second factorials as they now stand, and again integrating, we reproduce the sum in the form

$$\left.\begin{aligned} &\frac{q \,.\, q-1}{p+1 \,.\, p+2} \cdot m-q+2 \,.\, m-q+1 \,.\,.\,.\,.\, m-\overline{p+q}+1 \times 1.2.3.\,.\,.\,.\, q-2 \\ &+ \frac{q \,.\, q-1}{p+1 \,.\, p+2} \cdot m-q+1 \,.\, m-q \,.\,.\,.\,.\,.\,.\, m-p+q \times 2.3.4.\,.\,.\,.\, q-1 \end{aligned}\right\} \quad . \quad (D)$$

&c. &c.

It appears, then, that we may continue this differentiation on the one side q times, and integration on the other $q+1$ times; and that at each succeeding operation, an additional next lower factor will be introduced into the numerator of the fractional coefficient, and an additional next highest into the denominator. And after q differentiations, the last factorials will all become unity; and, the middle factorial having acquired an additional higher factor at each of $q+1$ integrations, we have for the sum of the series—

$$\left.\begin{aligned} &\frac{q \,.\, q-1 \,.\, q-2 \,.\,.\,.\, 1}{p+1 \,.\, p+2 \,.\,.\,.\, p+q+1} \times m+1 \,.\, m \,.\,.\,.\,.\, m-\overline{q+p}+1 \end{aligned}\right\} \quad . \quad (E)$$

II.

The Problem in Probabilites to which the foregoing summation is applicable, is the following:—

Suppose an experiment concerning whose inherent probability of success we know nothing, has been made $\overline{p+q}$ times, and has succeeded p times, and failed q times, what is the probability of success on the $\overline{p+q+1}^{\text{th}}$ trial.

This Problem is interesting, because it tends to the discovery of a rational measure for those expectations of success which constitute the motive for a large portion of human actions. The force of such expectations commonly depends, not upon reason, but upon temperament; and, according, as a man is naturally sanguine or the reverse, so in all the contingencies of life, does he over-estimate or under-estimate the chances in his favour.

It would be going much too far to think, that we can give an algebraic formula, by the application of which a man may, in every practical case, correct his natural tendency to error, and arrive at a strictly rational amount of expectation.

All that we can say is, that experience has led dispassionate men to come to nearly the same conclusion as the mathematician: for while he asserts the probability of success to be $\frac{p+1}{p+q+2}$, they act upon the supposition that the probabilities of success and failure are proportioned to the number of experienced cases of success and failure: and when either p or q is a large number, that is, when the experience is great, the conclusion and the supposition coincide.

In order to realise the Problem, we shall use the ordinary illustration, and suppose that a bag contains m balls in unknown proportions of black and white, but all either black or white; that p white and q black balls have been drawn, and that it is required to find the probability of drawing a white at the $p+q+1^{\text{th}}$ drawing.

The Problem as thus stated, admits of four varieties.

1. m may be given, and the balls drawn may have been replaced in the bag.
2. m may be given, and the balls drawn not replaced.
3. m may be infinite or indefinite, and the balls replaced.
4. m may be infinite or indefinite, and the balls not replaced.

Of these, the 3*d* is the only case which I find solved in the treatises which I have consulted. I propose to solve the 2*d* case, and therein the 4*th*; and, in conclusion, to make an attempt at the solution of the 1*st* case.

To render the observed event, that is, the drawing of p white and q black balls (or E), possible, the original number of whites may have been any number from $m-q$ to p inclusive, and the number of blacks any number from q to $m-p$.

Let us call the hypothesis of $m-q$ white and q black, H_1

and $m-q-1$ white and $q+1$ black, H_2, &c.

Then H_1 gives for probability of E $\frac{m-q \,.\, m-q-1 \,.\, \ldots\ldots\, m-q-p+1 \times 1 \,.\, 2 \,.\, 3 \,.\, \ldots\ldots\, q^*}{m \,.\, m-1 \quad \ldots\ldots\, m-q-p+1}$

or, calling the denominator A,

$$\left.\begin{aligned} H_1 \text{ gives } \frac{1}{A} \cdot m-q \,.\, m-q-1 \,.\, \ldots\ldots\, m-q-p+1 \times 1 \,.\, 2 \,.\, 3 \,.\, \ldots\ldots\, q \quad (\alpha) \\ H_2 \text{ gives } \frac{1}{A} \cdot m-q-1 \,.\, m-q-2 \,.\, \ldots\ldots\, m-q-p \times 2 \,.\, 3 \,.\, 4 \,.\, \ldots\ldots\, q+1 \quad (\beta) \\ H_3 \text{ gives } \frac{1}{A} \cdot m-q-2 \,.\, m-q-3 \,.\, \ldots\ldots\, m-q-p-1 \times 3 \,.\, 4 \,.\, \ldots\ldots\, q+2 \quad (\gamma) \\ \text{\&c.} \qquad\qquad\qquad\qquad \text{\&c.} \end{aligned}\right\} \text{(F)}$$

Now, $\alpha+\beta+\gamma$, &c. by the former proposition (E)

$$= \frac{1}{A} \cdot \frac{q \,.\, q-1 \,.\, \ldots\ldots\, 1}{p+1 \,.\, p+2 \,.\, \ldots\, p+q+1} \times m+1 \,.\, m \,.\, \ldots\ldots\, m-p-q+1$$

$$\therefore \text{ probability of } H_1 = \frac{\alpha}{\alpha+\beta+\gamma, \text{ \&c.}}$$

* The coefficient (U of Galloway's Treatise), expressing the number of different ways in which p white and q black balls can be combined in $p+q$ trials, is here omitted. This is immaterial, as it disappears in the expression $\frac{\alpha}{\alpha+\beta+\gamma, \text{ \&c.}}$

$$= \frac{p+1 \,.\, p+2 \,.\, \ldots \,.\, p+q+1}{m+1 \,.\, m \,.\, \ldots \,.\, m-p-q+1 \times 1 \,.\, 2 \,.\, 3 \ldots q} \times (m-q \,.\, m-q-1 \ldots m-q-p) + (1 \times 1 \,.\, 2 \,.\, 3 \ldots q)$$

But the probability of a white at $\overline{p+q+1}^{\text{th}}$ drawing on H_1 is $\frac{m-p-q}{m-p-q}$

$\therefore$ probability of white derived from H_1 is

$$\frac{p+1 \,.\, p+2 \,.\, \ldots \,.\, p+q+1}{m+1 \,.\, m \,.\, \ldots \,.\, m-p-q \times 1 \,.\, 2 \,.\, 3 \,.\, \ldots \,.\, q} \times (m-q \,.\, m-q-1 \ldots m-q-p) \times (1 \,.\, 2 \,.\, 3 \ldots q) \quad \text{(G)}$$

So probability from H_2

$$= \frac{p+1 \,.\, \ldots \,.\, p+q+1}{m+1 \,.\, m \,.\, \ldots \,.\, m-p-q \times 1 \,.\, 2 \,.\, 3 \ldots q} \times (m-q-1 \,.\, m-q-2 \ldots m-q-p-1) \times (2 \,.\, 3 \cdots \overline{q+1})$$

And so for all the other hypotheses in succession.

Now this series, omitting for the present the consideration of the fraction which is a factor common to them all, is a series of the same form as that summed in the last proposition, only that now $p+1$ must be substituted for p.

We have therefore the whole probability of a white at $\overline{p+q+1}^{\text{th}}$ drawing

$$= \left.\begin{aligned} &\frac{p+1 \,.\, p+2 \,.\, \ldots \,.\, p+q+1}{m+1 \,.\, m \,.\, \ldots \,.\, m-p-q \times 1 \,.\, 2 \,.\, \ldots \,.\, q} \times \frac{1 \,.\, 2 \,.\, \ldots \,.\, q}{p+2 \,.\, \ldots \,.\, p+q+2} \times \\ &m+1 \,.\, m \,.\, \ldots \,.\, m-p-q = \frac{p+1}{p+q+2} \end{aligned}\right\} \quad \ldots \quad \text{(H)}$$

Note.—It may be worth observing, that, had we summed the original series in Prop. 1. upwards instead of downwards, we should have got for a first factor $\frac{1 \,.\, 2 \,.\, 3 \,.\, \ldots \,.\, p}{q+1 \,.\, q+2 \,.\, \ldots \,.\, p+q+1}$, which must therefore $= \frac{1 \,.\, 2 \,.\, 3 \,.\, \ldots \,.\, q}{p+1 \,.\, p+2 \,.\, \ldots \,.\, p+q+1}$. And that these fractions are equal may be proved independently, for if we divide each by $1 \,.\, 2 \,.\, 3 \,.\, \ldots \,.\, p \times 1 \,.\, 2 \,.\, 3 \,.\, \ldots \,.\, q$, we have on both sides the same quotient $\frac{1}{1 \,.\, 2 \,.\, 3 \,.\, \ldots \,.\, p+q+1}$.

There now remains for solution only the first case of the problem in chances, that is, to find the probability of drawing a white ball, when m the number of balls is given, and p white and q black have already been drawn and returned.

The main object in this case is to sum the series

$$\overline{m-1}^{|p} \times 1^q + \overline{m-2}^{|p} \times 2^q \,.\, \ldots \,.\, 1^p \,.\, \overline{m-1}^{|q} \quad \ldots \quad \text{(I)}$$

This may be done much as in the preceding case, by taking the successive differences of the right-hand factors till the differences vanish, and multiplying the successive terms of the last or $\overline{q+1}^{\text{th}}$ row of differences into the $\overline{q+1}^{\text{th}}$ summation of the successive terms of the series $(1+2^p \,.\, \ldots \,.\, +\overline{m-1}^{|p}) + (1+2^p \,.\, \ldots \,.\, +\overline{m-2}^{|p})$, &c.

This may be sufficiently explained by going through the operation in a low particular case. Let $p=2$, $q=3$.

Then the series written perpendicularly is

$$\begin{array}{lllll}
\overline{m-1}^{|2} \times 1 & \Sigma_1\overline{m-1}^{|2} \times 1 & \Sigma_2\overline{m-1}^{|2} \times 1 & \Sigma_3\overline{m-1}^{|2} \times 1 & \Sigma_4\overline{m-1}^{|2} \times 1 \\
\overline{m-2}^{|2} \times 8 & \Sigma_1\overline{m-2}^{|2} \times 7 & \Sigma_2\overline{m-2}^{|2} \times 6 & \Sigma_3\overline{m-2}^{|2} \times 5 & \Sigma_4\overline{m-2}^{|2} \times 4 \\
\overline{m-3}^{|2} \times 27 = & \Sigma_1\overline{m-3}^{|2} \times 19 = & \Sigma_2\overline{m-3}^{|2} \times 12 = & \Sigma_3\overline{m-3}^{|2} \times 6 = & \Sigma_4\overline{m-3}^{|2} \times 1 \\
\overline{m-4}^{|2} \times 64 & \Sigma_1\overline{m-4}^{|2} \times 37 & \Sigma_2\overline{m-4}^{|2} \times 18 & \Sigma_3\overline{m-4}^{|2} \times 6 & \\
\overline{m-5}^{|2} \times 125 & \Sigma_1\overline{m-5}^{|2} \times 61 & \Sigma_2\overline{m-5}^{|2} \times 20 & \Sigma_3\overline{m-5}^{|2} \times 6 & \\
\text{\&c.} & \text{\&c.} & \text{\&c.} & \text{\&c.} &
\end{array}$$

The value of the different sigmas is easily found by the method of finite differences.

Generally, since the differences of 1^q, 2^q, 3^q, &c., always vanish in the $\overline{q+1}^{|\text{th}}$ line and after the q^{th} term of it, the general expression is

$$\Sigma_{q+1}\overline{m-1}^{|p} + d_2\,\Sigma_{q+1}\overline{m-2}^{|p} \ldots\ldots d_q\,\Sigma_{q+1}\overline{m-q}^{|p};$$

d_1, d_2, d_3, &c., signifying the 1st, 2d, 3d, &c., terms of the $\overline{q+1}^{|\text{th}}$ row of differences.

This summation may be applied to find the probability in the case now under consideration, for it expresses the $\alpha+\beta+\gamma$, &c., of the preceding case. Applying it as we did the value of $\alpha+\beta+\gamma$, &c., there found, we shall find the probability of a white ball at the $\overline{p+q+1}^{|\text{th}}$ trial to be

$$\frac{\Sigma_{q+1}\overline{m-1}^{|p+1} + d_2\,\Sigma_{q+1}\overline{m-2}^{|p+1} \ldots\ldots d_q\,\Sigma_{q+1}\overline{m-q}^{|p+1}}{m\,(\Sigma_{q+1}\overline{m-1}^{|p} + d_2\,\Sigma_{q+1}\overline{m-2}^{|p} \ldots\ldots d_q\,\Sigma_{q+1}\overline{m-q}^{|p})} \quad \ldots\ldots \quad \text{(K)}$$

If m be infinite, the expression becomes

$$\frac{(1+d_2+\ldots\ldots d_q)\,.\,\Sigma_{q+1}m^{p+1}}{m\,(1+d_2\ldots\ldots d_q)\,.\,\Sigma_{q+1}m^p} = \frac{\Sigma_{q+1}m^{p+1}}{m\Sigma_{q+1}m^p}$$

But if x be a quantity varying between the limits 0, x,

$$\frac{\Sigma_1 m^{p+1}}{m\,\Sigma_1 m^p} = \frac{\int_x^0 x^{p+1}\,dx}{x\int_x^0 x^p\,dx} = \frac{p+1}{p+1}\cdot\frac{x^{p+2}}{x\,.\,x^{p+1}}$$

And by continuation

$$\frac{\Sigma_{q+1}m^{p+1}}{m\,\Sigma_{q+1}m^p} = \frac{p+1\,.\,p+2\ldots\ldots p+q+1}{p+2\,.\,p+3\ldots\ldots p+q+2} = \frac{p+1}{p+p+2} \quad \ldots \quad \text{(L)}$$

We have thus found the probability in every case of the problem; the 2d and 4th at H, for the result, being independent of m, must be true for an infinite as well as for a finite number. The 1st case is solved at K, and the 3d at L.

XXXVII.—*On the Optical Phenomena and Crystallisation of Tourmaline, Titanium, and Quartz, within Mica, Amethyst, and Topaz.* By Sir David Brewster, K.H., D.C.L., F.R.S., and V.P.R.S. Edin. (With a Plate.)

(Read 4th January 1853.)

The existence of certain minerals imbedded in others,—the optical phenomena which they exhibit,—their form and mode of distribution, and the mechanical influence which has been exerted during their formation on the mineral that contains them, are among the most curious and instructive facts in physical science.

The dissemination of perfectly-formed crystals of titanium, both in the form of *titanite* and *anatase*, in Brazilian crystals of quartz, is a fact so well known that I shall take no farther notice of it, but shall proceed to give an account of a series of facts of a much more general and interesting character, which I have had occasion to observe, during an extensive examination of minerals, undertaken with a different object.

1. *On the Distribution of Tourmaline in Mica.*

When fluids and condensed gases are imprisoned in the cavities of *topaz* and other hard minerals, they retain their place till some powerful agent releases them from confinement, or till heat gives them such an expansive force as to burst the mineral. In *mica*, however, where the laminæ of which it is composed are held together by a very feeble cohesive force, the fluids in their cavities, and the extraneous materials which were present at their formation, have experienced no difficulty in quitting their place, and spreading themselves between the plates of the mineral.

Tourmaline and *quartz*, though thus distributed between the laminæ of mica subsequent to its crystallisation, have yet found a place in it contemporaneously with the crystallisation of the mica itself. In this case they are large crystals, equivalent in thickness to many laminæ, and may be taken out and subjected to examination. Some of the crystals of tourmaline are so large, indeed, that I have used them with their own natural faces as analysing prisms; and the quartz crystals, which are amorphous, and very irregularly formed, occupy a still greater space. In both cases, however, the tourmaline and the quartz, when taken out, leave large openings in the laminæ, and have greatly disturbed the structure of the mica around them.

The crystals of tourmaline thus formed in the mica, have almost always the faces of the flattened hexagonal prism parallel to the laminæ of the mica. I have found, however, a few cases in which the flat summit of the hexagonal prism is parallel to the laminæ. The crystallisations of quartz have also the axis of the prism, or its hexagonal faces parallel to the laminæ.

The other crystals of tourmaline which I have discovered in mica have a very different character: They have been formed subsequently to the crystallisation of the mica, and exist only between its laminæ. I have not been able to discover any cavities in mica containing fluids or gases, but I have found thousands from which the fluids and gases have escaped,—the one crystallising into hexagonal plates of tourmaline, and the other separating the laminæ, or running between them, and carrying along with it minute portions of crystallisable matter.

The hexagonal crystals thus formed have their faces perpendicular to the axis of double refraction, which is the axis of the prism; and what is peculiarly interesting, the fluid from which they were formed has insinuated itself between several of the laminæ, and the different plates of tourmaline which they formed have, of course, the sides of the hexagon incoincident. Sometimes these crystals extend to different distances from the centre of the original cavity, and are occasionally formed round it in a circular group. See Plate XV., Fig. 1.

The centre of the cavity from which these crystals have been projected is occupied by a spherical group of granular or capillary crystals, which is generally very *opaque*, though such groups sometimes exhibit, in particular spots, double refraction, and a speck of light is occasionally seen through the centre of the group. In some cases I have observed these very thin hexagonal plates without this opaque centre; and they have probably been formed by a portion of the fluid projected to a distance between faces of easy cleavage. The black spherical group already mentioned has its outward surface bristled with points, which are the extremities of the crystals radiating from its centre; and in one fine specimen to be farther described, it is surrounded with a ring of less opacity than the nucleus, and analogous to what is common in circular crystals. See Fig. 1.

The thin plates thus formed between the laminæ, whether hexagonal or prismatic, are always of a faint *brownish yellow*, which at an increased thickness becomes *green;* and so exceedingly thin are these plates, especially those farthest from the nucleus, that with a power of 400, it is often very difficult to see their terminal lines.

In order to convey an idea of these phenomena, I have given a drawing in Fig. 1 of a very interesting one, where the prismatic crystal nearest the black central group is a bright *green* in all azimuths with polarised light, surrounded with three or four larger prismatic yellowish plates, growing fainter both in tint and outline as they recede. In some cases the crystals are brown, and in others beautifully dichroitic, being *bright green* and *pink* in the different azimuths of polarised light.

As considerable forces must have been in operation during the production of these phenomena, we may expect to see the effects of them upon the surrounding mica. We accordingly observe the polarisation produced by pressure round almost all of these crystalline groups. Rents and other marks of violence are dis-

Fig. 1

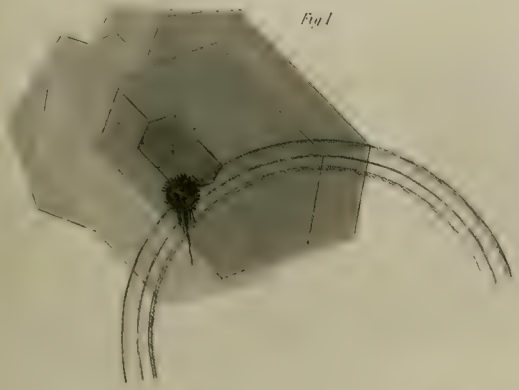


Fig. 2.

Fig. 4.

Fig. 3

Fig. 5.

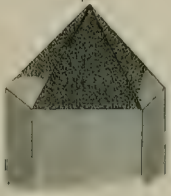


Fig. 6.

Fig. 8.

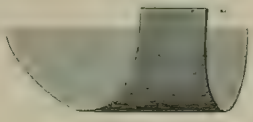
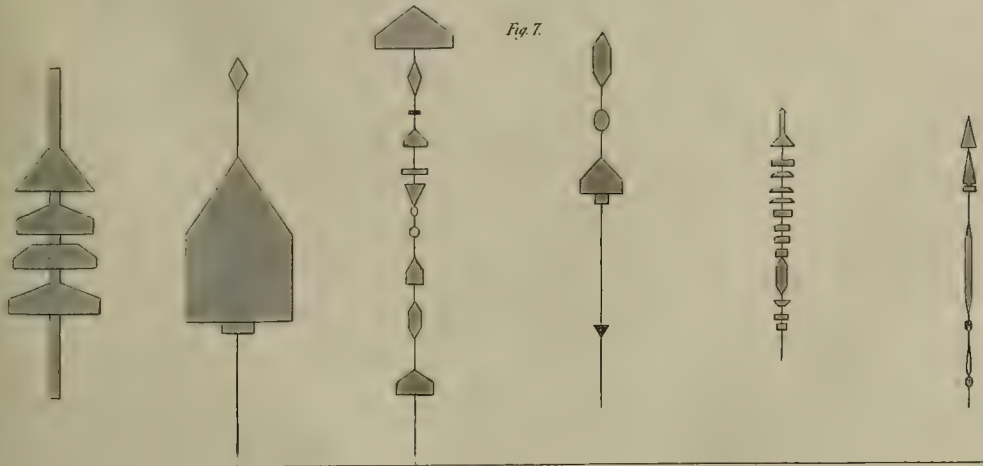


Fig. 7.



BRITISH
MUSEUM
NATURAL HISTORY

tinctly seen in the mica, and cracks or luminous streaks often occur in the tourmaline plates themselves. I have observed, too, in portions of the mica where I cannot find any cavities or crystals, distinct luminous sectors of polarised light, which could only be produced by a force emanating from their centre. This force may have been that of gas discharged from some neighbouring cavity, and driven by change of temperature to some other part of the mica plate; and in the following remarkable phenomenon we may perhaps find some evidence in favour of this opinion.

Plates of mica contain many beautiful systems of Newton's rings, occupying a circular space where the laminæ have been separated by some cause or other, and where, of course, there must be either air or some gaseous body. The colours of the first order are at the circumference of the circular space where the laminæ are in optical contact, and the higher orders of colour extend towards, and often to the centre of the space. Now it is a curious fact, that wherever there is a cavity which has projected its *fluid* and probably *gaseous* contents, *it is situated in the circumference of one of these circular spaces.* When two cavities have been near each other, the circular spaces unite and lose their form, and when the cavities have been more numerous, the circular spaces unite into very irregular shapes. That these circular hollows or spaces between the laminæ have been produced by something which has issued from the cavity to which they are so constantly related, cannot admit of a doubt. That it has not been a fluid is evident, and therefore it must have been a gas, which is either there still, or has escaped through some minute openings between the laminæ, where optical contact has been restored.*

There are some specimens of mica in which the crystals of tourmaline are large and opaque, and exhibit phenomena which I believe have not been recognised in any other mineral. The most interesting specimen of this kind I owe to Professor Fleming, who pointed out to me one of the peculiarities which it contains. This specimen is accurately represented, of the natural size, in Fig. 2. The largest of the five crystals is 0·28 of an inch broad, and the smallest 0·08 of an inch. Their thickness cannot greatly exceed the thousandth of an inch, and yet it is with difficulty that the strongest sun-light can be seen through them. The form of the smallest is a perfect hexagon, and in the rest the same form is more or less distinct. In the oval crystal there are numerous holes, and in all of them there are numbers of rectilineal cracks parallel to the sides of the hexagon, and some of them so narrow that light can scarcely pass through them. When we look at the sun through one of these crystals, a curious optical phenomenon is seen, a luminous hexagonal surface, composed of lines of light, parallel to the

* A fluid even may have thus escaped, and the circular hollow remained as before. In support of this opinion, see *Edinburgh Transactions*, vol. x., p. 11; but especially vol. xvi., p. 13; or *Phil. Mag.*, vol. xxxi., p. 101, August 1847.

sides of the hexagon, and six beautiful radiations, like those of the Asterial Sapphire, perpendicular to the sides of the hexagon.

The existence of these rectilineal fissures is an important fact in crystallography. It proves that the crystals were in a soft state after they had attained their present form; and that, in the process of induration, the fissures were produced by the shrinking of the tourmaline, in the same manner as similar fissures are produced during the induration of clay. In the mica which surrounds some of the crystals, there is the appearance of considerable disturbance; but I can find no trace of any cavity from which the tourmaline may have been ejected in a fluid state. The faces of these crystals are not everywhere in optical contact with the mica, and it is very probable that they could be removed without any adhering mica, as I have occasionally found crystals of tourmaline that were moveable between the laminæ.

In the same specimen which contains these tourmalines, and in others, I have found, what I believe has never before been observed, the woolly filaments of the *Penicillum glaucum* of Link, with its sporules scattered about between the laminæ, and sometimes beautifully moniliform, as in the *Penicillum glaucum* obtained from milk by M. Turpin.*

2. *On the Distribution of Titanium in Mica.*

In examining a remarkable specimen of mica from Irkutsk, in Siberia, I found *titanium* between the laminæ in various forms, sometimes in amorphous plates, sometimes in a powdery state adhering to the mica, and most frequently in beautiful dendritic forms, of various degrees of thickness. At a thickness of about the hundredth of an inch, the titanium, in all these forms is opaque; but at less thicknesses, it has a brownish transparency, becoming almost perfectly transparent at thicknesses which do not seem to exceed the 2000th part of an inch. In Fig. 3 I have given a drawing of an opaque group executed for me with minute accuracy by my celebrated friend Mr Haidinger of Vienna, during his residence in Edinburgh. The transparent groups are much more beautiful than the opaque ones, the crystalline ramifications having the most diversified forms, resembling often regular organisations.

When the *mica* is removed from above the titanium, so that only an exceedingly thin film of it is left, the reflected light is extremely brilliant, and consists of the most splendid colours. These colours, which have always the form of the titanium, are those which are produced by the thin film of mica which covers the titanium, and are not produced, as has been supposed, by a vacuity in the mica.

In some specimens of mica from Bengal, the imbedded titanium is spread out

* See *Comptes Rendus*, tom. v., p. 822, 1837, Dec. 11.

in a very irregular manner from a nucleus, sometimes having the form of a thin film; sometimes of oriental characters; and sometimes it is disseminated in grains so extremely minute, that the flame of a candle seen through it is surrounded with a halo of five or six perfectly-formed coloured rings.

3. *Distribution of Quartz in Mica.*

In mica from various localities, I have found large crystallisations of quartz, the quartz replacing the mica. I have never even once met with a regular crystal of quartz; and what is curious, all the crystalline masses of it which I have examined have their axis of double refraction in the plane of the laminæ of mica. In some very large specimens of Bengal mica given to me by Mr SWINTON, I have found layers of quartz, several inches in area, and about the 200th of an inch thick. The two surfaces of the plates are exceedingly inequal and corrugated, owing to the circumstances under which they were formed, but they possessed regular double refraction, and gave the colours of polarised light.

4. *Distribution of Titanium in Amethyst.*

While examining, many years ago, along with the late MARQUIS of NORTHAMPTON, several bags of amethyst which had been imported into Scotland from the Brazils, we were surprised to observe a number of fine pyramidal crystals, which seemed to have a powdery matter distributed through their mass. Upon more narrowly examining these crystals, I found that this dust formed an inner pyramid, all the faces of which were parallel to the faces of the pyramid of amethyst. When two parallel faces were ground upon the pyramid, and perpendicular to its axis, the particles of dust were seen by the microscope to consist each of several spicular crystals of titanium, crossing one another at angles of 60° and 30°, and forming distinct groups. In one crystal there were two interior pyramids composed of these groups; and it will be seen, from the explanation which I shall presently give of this phenomenon, that there may be any number of such pyramids.

As the crystals of amethyst are supposed to have been produced by the gradual enlargement of a small crystal placed in an amethystine solution, we have only to assume that a solution containing titanium has been introduced into the amethystine solution at different times during the growth of the crystal. The small crystals of titanium will deposit themselves on each of the surfaces of the pyramid; and when the whole of the introduced titanium has been thus deposited, the enlargement of the amethyst will go on, leaving a pyramid of titanium crystals in its interior. If a second solution of titanium is introduced, a second pyramid of its particles will be formed in the same manner; and this process may be repeated any number of times.

If we now suppose that the amethystine solution is exhausted, just when the

titanium solution has deposited all its crystals, the completed crystal of amethyst will have its outer surfaces covered with spicular crystals of titanium, or the pyramid of titanium groups will be on the very outside of the pyramid of amethyst. I had the good fortune to find such a crystal, in which the coat containing the titanium is laid like varnish on all the faces of the pyramid, but only on the upper end of three of them; the lower end of these three faces having lain on the solution protected from the deposition of the titanium. This crystal is, I believe, unique, and possesses the great interest of exhibiting the very process by which it was formed.

The two phenomena which I have just described are shewn in Figs. 4 and 5.

5. *Distribution of Titanium in Brazil Topaz.*

In examining a great number of very imperfect crystals of Brazil topaz, I found many which contained crystals of titanium of a brilliant scarlet colour, with a tinge of yellow. These crystals were perfectly transparent, and occurred in *seven* different forms.

1. In flat amorphous plates, which were highly transparent.
2. In hexagonal plates, lying in different planes.
3. In transparent lines running in different directions, and, though continuous, lying in different planes.
4. In lines running inwards from the margin of the specimen, and terminating in small flat plates. See Fig. 6.
5. In the most remarkable symmetrical forms like sceptres or maces, resembling some of those symmetrical cavities which I had previously found in the white topazes of New Holland.* See Fig. 7.
6. In some specimens the plates of titanium are actually bent, as in Fig. 8.
7. In little groups of transparent circular plates of a scarlet colour, and having concentric rings.

When light is reflected from the separating faces of the titanium and topaz, it is almost completely polarised; and at greater angles than that of maximum polarisation, colours of singular brilliancy cross the reflected images. These colours are doubtless connected with the fact, that at some of these faces there are *three* images of a luminous object seen by reflexion, one of the two outer ones being polarised oppositely to one of the double middle images, as in the case of the multiplication of images in composite crystals of calcareous spar.†

6. *On the Crystals and Cavities in Garnet.*

In the greater number of the crystals of garnet which I have had occasion to examine, I have found many crystals and cavities, and much amorphous matter. In

* See *Edinburgh Transactions*, 1826, vol. x., Plate XX.
† See *Phil. Trans.*, 1815, Plate XV., Fig. 2.

one specimen, in particular, the included crystals form a larger mass than the garnet, which is merely a cement for holding them together. These crystals have various crystalline forms, while some are amorphous, though regularly crystallised in their interior. All these crystals are doubly refracting, and give the colours of polarised light from their small size.

In another specimen, many of the crystals, in the form of hexagons and rhombic plates, are opaque, and exhibit by polarised light the remarkable phenomenon, which I had never before seen, of having luminous edges, so that when the rest of the crystal and all the field of view is dark, we observe hexagons and rhombs, and other geometrical figures, depicted in lines of red light. It is not easy to ascertain the cause of this singular appearance, because we cannot see the form of the crystals where the light exists; but I have no doubt that the luminous lines consist of light depolarised by reflexion from the sides of the hexagonal and rhombic plates, because the illuminating pencil is much larger than the crystals, and the crystals much smaller than the pupil of the eye, so that light must be reflected from the prismatic faces of the hexagons and rhombic plates, if they have sufficiently broad faces, and that light so reflected must enter the pupil of the eye.

In this specimen and in others there are many spherical cavities, surrounded with sectors of polarised light, and also several amorphous masses of matter, round which there is also polarised light, indicating, as all the phenomena of the crystals do, that the matter of the garnet must have been in a soft state, and compressed by some force emanating from these cavities.

In another specimen of garnet, a large fissure in its interior is occupied with granular matter, which must have issued either from a burst cavity containing a fluid or a gas, or both; but what is very interesting, and what I have never observed in any other mineral, the matter has, in several places, formed circular crystals of singular beauty, some being very simple, and others very composite.

St Leonard's College, St Andrews,
December 11, 1852.

XXXVIII.—*On the Production of Crystalline Structure in Crystallised Powders, by Compression and Traction.* By Sir David Brewster, K.H., D.C.L., F.R.S., V.P.R.S. Edin., and Associate of the Institute of France.

(Read 7th March 1853.)

The influence of compression and dilatation in producing the doubly refracting structure in solids of all kinds, whether crystallised or uncrystallised, which do not possess it, and in modifying that structure in all crystals which do possess it, has been long known; but with this class of phenomena, those which I am about to describe have no connection whatever.

In the course of experiments on the *double reflexion* and polarisation of light which I discovered in the *chrysammates of potash* and *magnesia*, *murexide*, and other crystals, I was surprised to find that these substances could be spread out upon glass by hard pressure, like grease or soft wax, and that in the case of chrysammate of potash and other bodies, when the powder could scarcely be distinguished from snuff, I obtained a transparent film, exhibiting the phenomena of double reflexion and polarisation from its surface, as perfectly as if I had been using a large crystal.

In subsequently repeating these experiments, and examining, under polarised light, the film thus produced by compression and traction, I was surprised to observe that the streaks and separate lines of the film, as well as the film itself, had regular axes of double refraction, as if they were regularly crystallised portions of the substance under examination. These streaks and capillary lines, which were often of extreme minuteness, did not appear to consist of insulated particles merely dragged into a line, but when the substance possessed the new property in perfection, the lines of polarised light were continuous, and the crystallographic as well as the optical axes of the particles were placed in that line. In other cases, where the experiment was less successful, the insulation of the particles was easily recognised, though the general mass of them was crystallographically arranged.

In making these experiments, the natural crystalline powder, or the particles of the crushed crystal, may be placed, either upon a polished glass surface, or upon a piece of glass ground on one side. In those cases where the substance is soft, the polished surface is preferable, but when the powder is hard and considerable pressure necessary, it is better to place it upon the ground surface of a piece of glass,

as the particles are detained between its minute elevations, and submit more readily to the combined force of pressure and traction. When the powder is thus placed, I take a polished and elastic knife, and with its broad point I compress and drag the powder in a given direction, till there is the appearance of a polished surface on the compressed substance. In general, I have used both the smooth and the rough glass, and have frequently obtained results with the one, which were not given by the other.

If we now place the plate of glass in a polarising microscope, with the field dark, we shall find that the streaks and lines produced by traction have, in certain substances, regular neutral and depolarising axes, as if they were prismatic crystals of the substance under examination. With the *chrysammate of magnesia*, a red powder with specks of yellow reflected light, the phenomena are peculiarly splendid; the natural colours of the substance, which vary greatly with the thickness of the streaks and films, being combined with the different tints which they polarise. As the crystals of this substance possess unusual reflexion, this property is displayed in the crystallised streaks produced by traction; and the superficial colours which they reflect, vary with the azimuth which the plane of incidence forms with the plane passing through the axis of the prism.

The remarkable property which I have now described, I have found, in a greater or a less degree, in the following crystals:—

Chrysammate of magnesia.
... of potash.
Hydro-chrysammid.
Murexide.
Aloetinate of potash.
Aloetinic acid.
Oxamide.
Palmine.
Palmic acid.
Amygdaline.
Tannin, pure.
Quinine, pure.
... acetate of.
... sulphate of.
... muriate of.
... phosphate of.
... citrate of.
Cacao butter.
Veratric acid.
Esculine.
Theine.
Silver, cyanide of.
... acetate of.
Platina and magnesia, cyanuret of.
... and barytes, cyanuret of.
... potassium, cyanuret of.
... ammonia, chloride of.
Potash, oxymuriate of.
... chromate of.
Urea, nitrate of.
Sulphur.
Camphor.
Cinchonine.
... sulphate of.
Meconic acid.
Brucine, sulphate of.
Morphia, acetate of.
Tin, iodide of.
Cerium, oxide of.
Parmeline.
Lecanorine.
Indigo, red.
Ammonia, oxalate of.
... sulphate of.
Soda, chromate of.
Lead, iodide of.

Strychnine, sulphate of.	Mercury, oxymuriate of.
... acetate of.	Isatine.
Soda, native nitrate of.	Alizarine.
Berberine.	Manganese, sesquioxide of.
Mucic acid.	Lead, protoxide of.
Solanine.	Tungstic acid.
Asparagine.	Chromo-oxalate of potash.

In submitting other crystals to the influence of compression and traction, I have found great numbers which do not exhibit the least trace of transparent streaks and lines, the separate particles being merely dragged into lines, and exhibiting only a quaquaversus polarisation. On the other hand, there is another class of crystals, whose powders or particles are forced into distinct and transparent streaks and lines in which the individual particles have a quaquaversus polarisation, and no trace of a prismatic arrangement. As these crystals have a peculiar relation to those in the preceding list, I shall enumerate the most important of them in the following table; that is, those in which the powder has been dragged into transparent and continuous streaks and lines, resembling externally portions of a solid body; for it is only by a comparison of the physical, or perhaps the chemical qualities of the two classes of bodies, that we can expect to explain the new property which is possessed only by one of them.

Hydrate of potash, pure.	Soda, acetate of.
Indigotic acid.	Mercury, prussiate of.
Urea.	... muriate of.
Citric acid.	... sulphuret of.
Silver, nitrate of.	Barytes, acetate of.
Meconine.	Zinc, chromate of.
Napthaline.	... sulphate of.
Soda, nitrate of.	Cobalt, sulphate of.
Potash and copper, sulphate of.	Magnesia and soda, sulphate of.
Soda, phosphate of.	Borax.

As both compression and traction are necessary in producing the transparent streaks and lines in both classes of the substances I have enumerated, it became interesting to ascertain what effect was produced by each of these forces acting separately, and which of them was chiefly influential in developing the doubly refracting arrangement exhibited by the substances that possessed it.

The force of compression was undoubtedly the agent in forcing the separate particles into optical contact, while that of traction drew them into a line, and tended to dilate the film in the direction of that line, and to draw its particles from each other; or overcome their attraction of aggregation in that direction. It is quite possible, too, that these forces may have exercised some influence in modifying the doubly refracting structure of the substance under examination; but as such a question has no bearing upon our present subject, I have not attempted its solution.

Without expecting any very interesting result, I submitted to examination several of the soft solids which possess double refraction, such as *bees' wax, oil of mace, tallow, and almond soap*. The last of these substances, though in common use, is a very remarkable one. Owing to its particles not being in optical contact, it has a fine pearly lustre, and may be drawn out into long and slender strings. Upon laying a portion of it on glass, it has a quaquaversus polarising structure, with a tendency to form circular crystals, but when it is drawn out into strings, and laid upon glass, these strings have neutral and depolarising axes, like the streaks formed by compression and traction. In the present case, it is by traction alone, that this crystalline arrangement of the particles is produced.

In *oil of mace* and *tallow*, a similar effect is produced by compression and traction. With *bees' wax*, the depolarising lines are still better displayed, and the effect is considerably increased by mixing the bees' wax with a small quantity of rosin.

As the preceding experiments place it beyond a doubt, that the optical or crystallographic axes of a number of minute particles are dragged by pressure and traction into the same direction, so as to act upon light like regular crystals, it became interesting to discover the cause of phenomena which certainly could not have been anticipated from any theoretical principle with which we are acquainted. The primary force, and indeed the only apparent one exerted in these experiments, is a mechanical force; but it is not improbable that a secondary force, namely, that of electricity, may be generated by the friction which accompanies the forces of pressure and traction. That such a force is excited with certain crystals will not admit of a doubt; but even if it were developed in every case, this would not prove that electricity was the agent in producing the phenomena under consideration. In subjecting asparagine to compression and traction, I observed, upon placing it in the polarising microscope, that its particles were moving about under an electrical influence, but in no other case did the same phenomenon present itself to me.

The experiments with soft solids, but especially those made with the almond soap, exclude the supposition that the electricity of friction is the cause of the crystalline arrangement of its particles; though it is not improbable that the sliding of the particles upon one another, as produced by traction, and their mutual separation, as in the case of tearing asunder mica or paper, may produce enough of electricity to have some share in giving the same direction to the axes of the particles.

When a portion of almond soap is placed upon glass, the axes of its particles lie in every direction, and have no tendency to assume the crystalline arrangement. The forces of aggregation emanating from three rectangular axes, are not strong enough to overcome the inertia, as we may call it, arising from the natural *quaquaversus* adhesiveness of the substance, and from the water interposed be-

tween its particles; but when the portion of soap is drawn out into a thread, these resistances to crystalline arrangement are diminished; elementary prisms, or crystals whose length is greater than their breadth, will have a tendency to place their greatest length in the line of traction, and all lateral obstruction to the play of its natural polarities being to a great extent removed, when the substance is drawn into a capillary thread the molecules will have free scope to assume their natural crystalline arrangement.

The application of these views to the powders and particles of hard crystals, is not so readily apprehended; but when we consider that the pressure brings the molecules of the substance within the sphere of their polarities, and that the force of traction reduces the compressed film into separate streaks and lines, like the threads of the almond soap, we have reason to conclude, that even in hard substances the atoms, when released from their lateral adhesions, and brought into narrow lines, will assume the crystalline arrangement.

In the course of these experiments, I have observed, in some cases where the crystalline arrangement was very imperfectly effected, a tendency in the atoms to quit their position, as if they were in a state of unnatural constraint, like the particles of silex and manganese in certain kinds of glass which experience a slow decomposition. If this should prove to be the case, either partially or generally, which time only can shew, it will doubtless arise from the non-homologous sides of the elementary atoms having come into contact, a condition of the crystalline lines perfectly compatible with the existence of neutral and depolarising axes, and of the colours of polarised light, provided that the non-homologous sides in contact deviate from their proper position, either 90° or 180°. If we cut a plate of mica, for example, into two pieces, and combine them by turning one of them round 90° or 180°, polarised light transmitted through them perpendicularly, will exhibit the same colours as when they were in their natural position, and also the same neutral and depolarising axes. If the polarised light is transmitted obliquely, the *hemitropism* of the combination, as we may call it, will be instantly discovered by the difference of colour of the two plates.

St Leonard's College, St Andrews,
February 25, 1853.

XXXIX.—*On the Absolute Zero of the Perfect Gas Thermometer; being a Note to a Paper on the Mechanical Action of Heat.* By WILLIAM JOHN MACQUORN RANKINE, C.E., F.R.S.E., F.R.S.S.A., &c.

(Read January 4, 1853.)

Temperature being measured by the pressure of a perfect gas at constant density, the absolute zero of temperature is that point on the thermometric scale at which, if it were possible to maintain a perfect gas at so low a temperature, the pressure would be null.

The position of this point is of great importance, both theoretically and practically; for by reckoning temperatures from it, the laws of phenomena depending on heat are reduced to a more simple form than they are when any other zero is adopted.

As we cannot obtain any substance in the perfectly gaseous condition (that is to say, entirely devoid of cohesion), we cannot determine the position of the absolute thermometric zero by direct experiment, which furnishes us with approximate positions only. Those approximate positions are always too high; because the effect of cohesion is to make the pressure of a gas diminish more rapidly with a diminution of temperature, than if it were devoid of cohesion.

As a gas is rarefied, the cohesion of its particles diminishes, not only in absolute amount, but also in the proportion which it bears to the pressure due to heat. The gas, therefore, approaches more and more nearly to the stateo f a perfect gas as its density diminishes; and from a series of experiments on the rate of increase of its elasticity with temperature, at progressively diminishing densities, may be calculated the positions of a series of points on the thermometric scale, approaching more and more nearly to the true absolute zero.

By observing the law which those successive approximations follow, the true position of the absolute zero can be determined.

Having performed this operation by means of a graphic process, soon after the publication of the experiments of M. REGNAULT on the elasticity and expansion of gases, I stated the result in a paper on the Elasticity of Vapours (*Edinburgh New Philosophical Journal*, July 1849), and also in a paper on the Mechanical Action of Heat (*Trans. Royal Soc. Edin.* vol. xx., Part 1), viz., that the absolute zero is

274·6 centigrade degrees,
or 494·28 degrees of FAHRENHEIT, } below the temperature of melting ice;
or 462·28 degrees below the ordinary zero of FAHRENHEIT'S scale.

To enable others to judge of the accuracy of this result, I shall now explain the method by which it was obtained, annexing a copy of the diagram used.

Let E denote the mean rate of increase, per degree, between the freezing and boiling points, of the pressure of a gas whose volume is maintained constant. Then the reciprocal of this coefficient, $\frac{1}{E}$, is an approximation to the number of degrees below the freezing point, at which the absolute zero is situated.

The experimental data in the following table were copied from the memoirs of M. Regnault on the Expansion of Gases. The numbers in the first column designate the series of experiments. The second column contains the pressures of the gases at the freezing point. The third column contains the mean coefficients of increase of pressure per centigrade degree, between 0° and 100 centigrade. The fourth column contains the reciprocals of those coefficients, with the negative sign, being approximate positions of the absolute zero, in centigrade degrees, below the temperature of melting ice. The gases employed were atmospheric air and carbonic acid.

No.	Pressure at 0° Centigrade in Atmospheres.	Coefficient of increase of Elasticity with Temperature = E.	Approximate positions of the absolute zero in Centigrade degrees $= \frac{1}{E}$
		CARBONIC ACID.	
1.	0·9980	0·0036856	−271°·33
2.	1·1857	0·0036943	−270·63
3.	2·2931	0·0037523	−266·50
4.	4·7225	0·0038598	−259·08
		ATMOSPHERIC AIR.	
1.	0·1444	0·0036482	−274·11
2.	0·2294	0·0036513	−273·88
3.	0·3501	0·0036542	−273·66
4.	0·4930	0·0036587	−273·32
5.	0·4937	0·0036572	−273·43
6.	1·0000	0·0036650	−272·85
7.	2·2084	0·0036760	−272·03
8.	2·2270	0·0036800	−271·74
9.	2·8213	0·0036894	−271·05
10.	4·8100	0·0037091	−269·61

The approximate positions of the absolutezero contained in this table were laid down on the diagram, in which they are marked by crosses. The longitudinal divisions represent centigrade degrees divided into tenths; the transverse divisions, atmospheres of pressure at 0° centigrade, also divided into tenths. The positions of the crosses indicate at once the pressures in the second column of the table, and the approximate zeros in the fourth; and the numbers affixed to them correspond with those in the first column.

As the effect of cohesion is greater, and more easily eliminated, in carbonic acid gas than in atmospheric air, the determination of the true absolute zero was made from the experiments on the former gas. It will be observed that the approximate positions of the absolute zero for carbonic acid lie nearly in a straight line. A straight line (dotted in the diagram) having been drawn so that it should as nearly as possible traverse them, was found to intersect the line corresponding to the zero of pressure, that is, to the state of perfect gas, at a point on the scale of temperatures 274·6 centigrade degrees below the temperature of melting ice; which point was accordingly taken as the true absolute zero of the perfect gas thermometer.

So far as their irregularity permits, the experiments on atmospheric air confirm this result, for the approximate positions of the absolute zero deduced from them, evidently tend towards the very same point on the diagram with those deduced from the experiments on carbonic acid.

The values of the coefficient of dilatation and of increase of pressure, of a perfect gas, per degree, in fractions of its volume and pressure, at the temperature of melting ice, are accordingly,—

For the Centigrade Scale $\frac{1}{274{\cdot}6} = 0{\cdot}00364166$

For Fahrenheit's Scale $\frac{1}{494{\cdot}28} = 0{\cdot}00202314$

XL.—*On the Mechanical Action of Heat.* By William John Macquorn Rankine, Civil Engineer, F.R.S.E., F.R.S.S.A., &c.

(Read January 17, 1853.)

Section VI.—A Review of the Fundamental Principles of the Mechanical Theory of Heat; with Remarks on the Thermic Phenomena of Currents of Elastic Fluids, as illustrating those Principles.

(Article 46.) I have been induced to write this Section, in continuation of a paper on the Mechanical Action of Heat, by the publication (in the *Philosophical Magazine* for December 1852, Supplementary Number) of a series of experiments by Mr Joule and Professor William Thomson, on the Thermal Effects experienced by Air in rushing through small Apertures. Although those authors express an intention to continue the experiments on a large scale, so as to obtain more precise results; yet the results already obtained are sufficient to constitute the first step towards the experimental determination of that most important function in the theory of the mechanical action of heat, which has received the name of Carnot's Function.

By the theoretical investigations of Messrs Clausius and Thomson,—which are based simply on the fact of the convertibility of heat and mechanical power, the determination of their relative value by Mr Joule, and the properties of the function called *temperature*, without any definite supposition as to the nature of heat,—Carnot's function is left wholly indeterminate.

By the investigations contained in the previous sections of this paper, and in a paper on the Centrifugal Theory of Elasticity,—in which the supposition is made, that heat consists in the revolutions of what are called Molecular Vortices, so that the elasticity arising from heat is in fact centrifugal force,—a *form* is assigned to Carnot's function; but its numerical values are left to be ascertained by experiment.

The recent experiments of Messrs Joule and Thomson serve (so far as the degree of precision of their results permits) at once to determine numerical values of Carnot's function for use in practice, and to test the accuracy with which the phenomena of heat are represented by the consequences of the hypothesis of molecular vortices, from which the investigation in this paper sets out.

SUB-SECTION 1.—*Properties of Expansive Heat.*

(47.) To shew more clearly the nature of the questions, towards the decision of which these experiments are a step, I shall now briefly review the fundamental principles of the theory of heat, and the reasoning on which they are based; and the object of this being illustration rather than research, I shall use algebraical symbols no farther than is absolutely necessary to brevity and clearness, and shall follow an order of investigation, which, though the same in its results with that pursued in the previous sections of this paper, is different in arrangement.

By a mind which admits as an axiom, that, in the present order of things, physical power cannot be annihilated, nor produced out of nothing, the law of the mutual convertibility of heat and motive power must be viewed as a necessary corollary from this axiom, and Mr JOULE's experiments, as the means of determining the relative numerical value of those two forms of power. By a mind which does not admit the necessity of the axiom, these experiments must be viewed also as the proof of the law.

This law was virtually, though not expressly, admitted by those who introduced the term Latent Heat into scientific language; for when divested of ideas connected with the hypothesis of a subtle fluid of caloric, and regarded simply as the expression of a fact, this term denotes heat which has disappeared during the appearance of expansive power in a mass of matter, and which may be made to reappear by the expenditure of an equal amount of compressive power.

(48.) Without for the present framing any mechanical hypothesis as to the nature of heat, let us conceive that unity of weight of any substance, occupying the bulk V under the pressure P, and possessing the absolute quantity of thermometric heat whose mechanical equivalent is Q, undergoes the indefinitely small increase of volume d V; and let us investigate how much heat becomes latent, or is converted into expansive power, during this process; the thermometric heat being maintained constant, so that the heat which disappears must be supplied from some external source.

During the expansion d V, the body, by its elastic pressure P, exerts the mechanical power P d V. Part of this power is produced by molecular attractions and repulsions; and although this part may be modified by the influence of heat upon the distribution of the particles of the body, it is not the direct effect of heat. The remainder must be considered as directly caused by the heat possessed by the body, of which the pressure P is a function; and to this portion of the power developed, the heat which disappears during the expansion must be equivalent.

To determine the portion of the mechanical power P d V which is the effect of heat, let the total heat of the body, Q, be now supposed to vary by an indefinitely

small quantity dQ. Then the mechanical power of expansion $P\,dV$ will vary by the indefinitely small quantity

$$dQ \times \frac{dP}{dQ} dV$$

This is the development of power for the expansion dV, caused by each indefinitely small portion dQ of the total heat possessed by the body; and consequently, the whole mechanical power for the expansion dV due to the whole heat possessed by the body Q, is expressed as follows:—

$$Q \frac{dP}{dQ} . dV \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (67.)$$

and this is the equivalent of the heat transformed into mechanical power, or the latent heat of expansion of unity of weight, for the small increment of volume dV, at the volume V and total heat Q.

Now a part only of this power, viz.—

$$P\,dV$$

is visible mechanical energy, expended in producing velocity in the expanding body itself, or in overcoming the resistance of the bodies which enclose it. The remainder

$$\left(Q \frac{dP}{dQ} - P\right) dV \quad . \quad . \quad . \quad . \quad . \quad . \quad (68.)$$

is therefore expended in overcoming molecular attraction.

Molecular attraction depends on the density and distribution of the particles of the body; and is consequently a function of the volume and total heat of unity of weight. It is therefore possible to find a potential S, being a function of V and Q, of such a nature, that the difference between its two values

$$S_2 - S_1$$

corresponding respectively to two sets of values of the volume and total heat (V_1, Q_1 and V_2, Q_2), shall represent the power which is the equivalent of the heat consumed in overcoming molecular attraction, during the passage of the body from the volume V_1 and heat Q_1 to the volume V_2 and heat Q_2. The form of the expression (68) shews that this potential has the following property:—

$$\frac{dS}{dV} = Q \frac{dP}{dQ} - P \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (69.)$$

The integration of which partial differential equation gives the following value for the potential of molecular action:—

$$S = \int \left(Q \frac{dP}{dQ} - P\right) dV + \phi(Q) \quad . \quad . \quad . \quad . \quad . \quad (70.)$$

$\phi(Q)$ being some unknown function of the heat only, and the integral being taken as if the heat Q were constant.

The heat which disappears in overcoming molecular action, during a small

increase of total heat dQ, while the volume remains constant, is expressed as follows:—

$$\frac{dS}{dQ}dQ = \left\{ Q\int\frac{d^2P}{dQ^2}dV + \phi'(Q)\right\}dQ \quad . \quad . \quad . \quad . \quad (71.)$$

the heat Q being treated as a constant in the integration.

If we now investigate the entire quantity of heat, both sensible and latent, which is consumed by a body during a simultaneous small change of total heat dQ and volume dV, we find the following results:—

Sensible heat (which retains its condition) . . . . . $= dQ$

Latent heat, or heat which disappears in overcoming molecular action . . . . . $\frac{dS}{dQ}dQ + \frac{dS}{dV}dV$

Latent heat equivalent to the visible mechanical effect . . PdV

The amount being

$$\left.\begin{aligned} dQ + d\,.\,S + PdV = \left(1+\frac{dS}{dQ}\right)dQ + \left(\frac{dS}{dV}+P\right)dV = \\ \left(1 + Q\int\frac{d^2P}{dQ^2}dV + \phi'Q\right)dQ + Q\,.\,\frac{dP}{dQ}\,.\,dV \end{aligned}\right\} \quad . \quad . \quad . \quad (72.)$$

This formula expresses completely the relations between heat, molecular action, and expansion, in all those cases in which the expansive power developed, PdV, is entirely communicated to the bodies enclosing the substance which expands.

(49.) The following coefficients are contained in, or deducible from it.

The ratio of the specific heat at constant volume to the real specific heat:—

$$\frac{K_V}{k} = 1 + \frac{dS}{dQ} = 1 + Q\int\frac{d^2P}{dQ^2}\,.\,dV + \phi'(Q) \quad . \quad . \quad . \quad . \quad (73.)$$

The coefficient of latent heat of expansion at constant heat:—

$$\frac{dS}{dV} + P = Q\frac{dP}{dQ} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (74.)$$

The ratio of the specific heat at constant pressure to the real specific heat is found as follows. To have the pressure constant, we must have

$$\frac{dP}{dQ}dQ + \frac{dP}{dV}dV = 0; \quad \text{or} \quad \frac{dV}{dQ} = -\frac{\frac{dP}{dQ}}{\frac{dP}{dV}}$$

consequently the ratio in question is

$$\left.\begin{aligned} \frac{K_P}{k} = 1 + \frac{dS}{dQ} - \left(\frac{dS}{dV}+P\right)\frac{\frac{dP}{dQ}}{\frac{dP}{dV}} = 1 + Q\int\frac{d^2P}{dQ^2}\,.\,dV + \phi'(Q) \\ - Q\frac{\left(\frac{dP}{dQ}\right)^2}{\frac{dP}{dV}} \quad . \quad . \quad . \quad . \end{aligned}\right\} \quad . \quad . \quad . \quad (75.)$$

(50.) In order to investigate the laws according to which heat is converted into mechanical power, in a machine working by the expansion of an elastic body, it will be convenient to use a function

$$F=\int \frac{dP}{dQ} dV \quad (Q=\text{const.})$$

of such a nature that the difference between two of its values, corresponding to different volumes of the body at the same total heat, represents the ratio of the heat converted into power by expansion between those volumes, to the given constant total heat. I shall call this function a *heat-potential.*

Introducing this function into Equation 72, we find, for the total heat consumed by a body during the increase of total heat dQ, and the expansion dV,

$$dQ+d.S+P\,dV=\left(1+\phi'.(Q)\right) dQ+Q\,d.F \quad . \quad . \quad . \quad . \quad (76.)$$

$$\left(\text{observing that } d.F=\frac{dF}{dQ}dQ+\frac{dF}{dV}dV=\left(\int\frac{d^2P}{dQ^2}dV.\right)dQ+\frac{dP}{dQ}dV.\right)$$

Let us now suppose that the body changes its volume without either losing or gaining heat by conduction. This condition is expressed by the equation

$$0=(1+\phi'.Q)\,dQ+Q\,d.F$$

from which we deduce the following,

$$-d.F=\frac{1+\phi'.(Q)}{Q}.dQ \quad . \quad . \quad . \quad . \quad (77.)$$

which expresses the following theorem :—

When the quantity of heat in a body is varied by variation of volume only, the variation of the heat-potential depends on the heat only, and is independent of the volume.

In order that a machine working by the expansive power of heat may produce its greatest effect, all the heat communicated from external bodies should be employed in producing expansive power, and none in producing variations of the quantity of heat in the body; for heat employed for the latter purpose would be wasted, so far as the production of visible motion is concerned. To effect this, the body must receive heat by conduction, and convert it into expansive power, while containing a certain constant quantity of heat Q_1; give out by conduction heat produced by compression, while containing a smaller constant quantity of heat Q_2; and change between those two quantities of thermometric heat by means of changes of volume only, without conduction. For this purpose a cycle of operations must be performed similar to that described by CARNOT; as follows:—

(I.) Let F_A be the initial value of the heat-potential; let the body expand at the constant heat Q_1, till the heat-potential becomes F_B. Then the heat received and converted into expansive power is

$$H_1=Q_1\,(F_B-F_A)$$

(II.) Let the body further expand without receiving or emitting heat, till the quantity of heat in it falls to Q_2; the heat-potential varying according to Equation 77, and becoming at length F_C. The heat converted into expansive power in this operation is

$$Q_1 - Q_2$$

(III.) Let the body be compressed, at the constant heat Q_2, till the heat-potential becomes F_D; a quantity differing from the initial heat-potential F_A by as much as F_C differs from F_B. In this operation the following amount of power is reconverted into heat, and given out by conduction:—

$$H_2 = Q_2 (F_C - F_D)$$

(IV.) Let the body be further compressed, till the heat-potential returns to F_A, its original value. Then, by the power expended in this compression alone, without the aid of conduction, the total heat of the body will be restored to its original amount, exactly reversing the operation II.

At the end of this cycle of operations, the following quantity of heat will have been converted into mechanical power:—

$$H_1 - H_2 = Q_1 (F_B - F_A) - Q_2 (F_C - F_D)$$

but it is obvious that the difference between the heat-potentials is the same in the first and third operations; therefore, the useful effect is simply

$$H_1 - H_2 = (Q_1 - Q_2)(F_B - F_A) \quad . \quad . \quad .$$

while the whole heat expended is,

$$H_1 = Q_1 (F_B - F_A) \quad . \quad . \quad . \quad .$$

. . (78.)

Hence, *the ratio of the heat converted into mechanical effect, in an expansive machine working to the greatest advantage, to the whole heat expended, is the same with that which the difference between the quantities of heat possessed by the expansive body during the operations of receiving and emitting heat, respectively, bears to the quantity of heat possessed by it during the operation of receiving heat; and is independent of the nature and condition of the body.*

This theorem is thus expressed symbolically,—

$$\frac{H_1 - H_2}{H_1} = \frac{\text{Effect}}{\text{Heat Expended}} = \frac{Q_1 - Q_2}{Q_1} \quad . \quad . \quad . \quad . \quad . \quad . \quad (79.)$$

(51.) When a body expands without meeting with resistance, so that all its expansive power is expended in giving velocity to its own particles, and when that velocity is ultimately extinguished by friction, then a quantity of heat equivalent to the expansive power is reproduced.

The heat consumed is expressed by taking away the term representing the expansive power, $P\,d\,V$, from the expression 72, the remainder of which consists merely of the variation of actual heat, and the heat expended in overcoming molecular attraction, viz.:—

$$dQ+d.S=\left(1+\frac{dS}{dQ}\right)dQ+\frac{dS}{dV}dV=\left(1+Q\int\frac{d^2P}{dQ^2}dV+\phi'.(Q)\right)dQ$$
$$+\left(Q\frac{dP}{dQ}-P\right)dV.$$

This expression is a complete differential, and may be written thus:—

$$d(Q+S)=d\left\{Q+\phi(Q)+\left(Q\frac{d}{dQ}-1\right)\int P\,dV\right\} \quad . \quad . \quad (80.)$$

(Q being treated as a constant in performing the integration $\int P\,dV$).

Its integral, Q+S, the sum of the heat of the body, and of the potential of its molecular actions, is the same quantity which I have denoted by the symbol Ψ in the 10th article of a paper on the Centrifugal Theory of Elasticity, and whose differences are there stated to represent the total amount of power which must be exercised on a body, whether in the form of expansive or compressive power, or in that of heat, to make it pass from one volume and temperature to another. This integral corresponds also to the function treated of by Professor WILLIAM THOMSON in the fifth part of his paper on the Dynamical Theory of Heat, under the name of "Total Mechanical Energy."

(52.) We have now obtained a system of formulæ, expressing all the relations between heat and expansive power, analogous to those deduced from a consideration of the properties of temperature, by Messrs CLAUSIUS and THOMSON, and from the Hypothesis of Molecular Vortices in the previous sections of this paper; but, in the present section, both the theorems and the investigations are distinguished from former researches by this circumstance;—that they are independent, not only of any hypothesis respecting the constitution of matter, but of the properties, and even of the existence, of such a function as Temperature; being, in fact, simply the necessary consequences of the following

DEFINITION OF EXPANSIVE HEAT.

Let the term EXPANSIVE HEAT *be used to denote a kind of Physical Energy convertible with, and measurable by, equivalent quantities of Mechanical Power, and augmenting the Expansive Elasticity of matter, in which it is present.*

(52 A.) It is further to be remarked, that the theorems and formulæ in the preceding articles of this section are applicable, not only to heat and expansive power, but to any two directly convertible forms of physical energy, one of which is actual, and the other potential. They are, in fact, the principles of the conversion of energy in the abstract, when interpreted according to the following definitions of the symbols.

Let Q denote the quantity of a form of actual physical energy present in a given body;

V, a measurable state, condition, or mode of existence of the body, whose tendency to increase is represented by

P, a force, depending on the condition V, the energy Q, and permanent properties of the body; so that

P d V is the increment of a form of potential energy, corresponding to a small increment d V of the condition V.

Let d S be the quantity whereby the increment of potential energy P d V falls short of the quantity of actual energy of the form Q, which is converted into the potential form, by the change of condition d V.

Then, as in Equation 69

$$\frac{d\,\mathrm{S}}{d\,\mathrm{V}} = \mathrm{Q}\frac{d\,\mathrm{P}}{d\,\mathrm{Q}} - \mathrm{P}$$

an equation from which all those in the previous articles are deducible, and which comprehends the whole theory of the mutual conversion of the actual form of energy Q, and the potential form $\int \mathrm{P}\, d\,\mathrm{V}$, whatsoever those forms may be, when no other form of energy interferes. The application of these principles to any form or any number of forms of actual and potential energy, is the subject of a paper read to the Philosophical Society of Glasgow, on the 5th January 1853, and published in the Philosophical Magazine for February 1853.

Sub-Section 2.—*Properties of Temperature.*

(53.) Still abstaining from the assumption of any mechanical hypothesis, let us proceed a step beyond the investigation of the foregoing articles, and introduce the consideration of *temperature*; that is to say, of an arbitrary function increasing with heat, and having the following properties.

Definition of Equal Temperatures.

Two portions of matter are said to have equal temperatures, when neither tends to communicate heat to the other.

Corollary.

All bodies absolutely destitute of heat have equal temperatures.

The ratio of the real specific heats of two substances, is that of the quantities of heat which equal weights of them possess at the same temperature.

Theorem.

The ratio of the real specific heats of any pair of substances, is the same at all temperatures.

For, suppose equal weights of a pair of homogeneous substances to be in contact, containing heat in such proportions as to be in equilibrio. Then, let additional

portions of each substance, of equal weight, and destitute of heat, be added to the original masses; so that the quantities of heat in unity of weight may be diminished in each substance, but may continue to be in the same ratio. Then, if the equality of temperature do not continue, portions of heat which were in equilibrio must have lost that equilibrium, merely by being transferred to other particles of a pair of homogeneous substances, which is absurd. Therefore, the temperatures continue equal.

It follows, that the quantity of heat in unity of weight of a substance at a given temperature, may be expressed by the product of a quantity depending on the nature of the substance, and independent of the temperature, multiplied by a function of the temperature, which is the same for all substances.

Let τ denote the temperature of a body according to the scale adopted; κ, the position, on the same scale, of the temperature corresponding to absolute privation of heat; $\mathfrak{k}$, a quantity depending on the nature of the substance, and independent of temperature. Then the quantity of heat in unity of weight may be expressed as follows:—

$$Q=\mathfrak{k}\,(\psi\,.\,\tau-\psi\,.\,\kappa) \quad . \quad . \quad . \quad . \quad . \quad . \quad (81.)$$

(54.) If we introduce this notation into the formula (79) which expresses the proportion of the total heat expended, which is converted into useful power by an expansive machine working to the best advantage, the quantity $\mathfrak{k}$, peculiar to the substance employed, disappears, and we obtain CARNOT'S THEOREM, as modified by Messrs CLAUSIUS and THOMSON, viz.,—that this ratio is a function solely of the temperatures at which heat is received and emitted respectively, and is independent of the nature of the substance; or symbolically,

$$\frac{\text{Effect}}{\text{Heat Expended}}=\frac{Q_1-Q_2}{Q_1}=\frac{\psi\,.\,\tau_1-\psi\,.\,\tau_2}{\psi\,.\,\tau_1-\psi\,.\,\kappa} \quad . \quad . \quad . \quad . \quad (82.)$$

(55.) Let us now apply the same notation to the formula (67) for the latent heat of a small expansion $d\,\mathrm{V}$ at constant heat, viz:—

$$Q\frac{d\,P}{d\,Q}\cdot d\,V$$

we have evidently

$$\frac{d\,P}{d\,Q}=\frac{1}{\frac{d\,Q}{d\,\tau}}\cdot\frac{d\,P}{d\,\tau}=\frac{1}{\mathfrak{k}\,\psi\,.\,\tau}\cdot\frac{d\,P}{d\,\tau}$$

and consequently, the heat which disappears by the expansion $d\,\mathrm{V}$ is

$$Q\frac{d\,P}{d\,Q}\cdot d\,V=\frac{\psi\,.\,\tau-\psi\,.\,\kappa}{\psi\,.\,\tau}\cdot\frac{d\,P}{d\,\tau}\cdot d\,V \quad . \quad . \quad . \quad . \quad . \quad (83.)$$

from which formula the specific quantity $\mathfrak{k}$ has disappeared.

Now, in the notation of Professor THOMSON we have

$$\frac{\psi\,.\,\tau-\psi\,.\,\kappa}{\psi\,.\,\tau}=\frac{J}{\mu}$$

where J is JOULE's equivalent, and μ a function of the temperature, the same for all substances, to be determined empirically; and consequently,

$$\text{hyp. log.}\,(\psi\,.\,\tau-\psi\,.\,\kappa)=\frac{1}{\mathrm{J}}\int^{\tau}\mu\,d\tau$$

or,

$$\psi\,.\,\tau-\psi\,.\,\kappa=\epsilon^{\frac{1}{\mathrm{J}}\int^{\tau}\mu\,d\tau}$$

and

$$\mathrm{Q}=\mathrm{K}\,(\psi\,.\,\tau-\psi\,.\,\kappa)=\mathrm{K}\,.\,\epsilon^{\frac{1}{\mathrm{J}}\int^{\tau}\mu\,d\tau} \qquad . \quad (84.)$$

These expressions will be recognised by those who have studied Professor THOMSON's papers on the Dynamical Theory of Heat. By introducing the value given above of the quantity of heat in unity of weight, into the formulæ of the preceding articles of this section, they are at once transformed to those of Professor THOMSON, and in particular, the formulæ 79 and 82 become the following:—

$$\frac{\text{* Effect of Machine}}{\text{Heat Expended}}=\frac{\epsilon^{\frac{1}{\mathrm{J}}\int^{\tau_1}\mu\,d\tau}-\epsilon^{\frac{1}{\mathrm{J}}\int^{\tau_2}\mu\,d\tau}}{\epsilon^{\frac{1}{\mathrm{J}}\int^{\tau_1}\mu\,d\tau}}=1-\epsilon^{-\frac{1}{\mathrm{J}}\int_{\tau_2}^{\tau_1}\mu\,d\tau} \qquad . \quad (85.)$$

SUB-SECTION 3.—*On the Hypothesis of Molecular Vortices.*

(56.) The use of a Mechanical Hypothesis in the Theory of Heat, as in other branches of physics, is to render it a branch of Mechanics, the only complete physical science; and to deduce its principles from the laws of Force and Motion, which are better understood than those of any other phenomena.

The results of the investigations in the preceding part of this section are consistent alike with all conceivable hypotheses which ascribe the phenomena of heat to invisible motions amongst the particles of bodies.

Those investigations, however, leave undetermined the relation between temperature and quantity of heat, except in so far as they shew that it must follow the same law of variation in all substances.

By adopting a definite hypothesis, we are conducted to a definite relation between temperature and quantity of heat; which, being introduced into the formulæ, leads to specific results respecting the phenomena of the mutual transformation of heat and visible mechanical power; and those results, being compared with experiment, furnish a test of the soundness of the hypothesis.

Thus the hypothesis of Molecular Vortices, which forms the basis of the investigations in the first five sections of this paper, and in a paper on the Centrifugal Theory of Elasticity, leads to the conclusion, that, if temperature be mea-

* It is to be observed, that in Professor THOMSON's notation, heat is supposed to be measured by an arbitrary unit, whose ratio to a unit of mechanical power is denoted by J; while in this paper, the same unit is employed in expressing quantities of heat and of mechanical power.

sured by the expansion of a perfect gas, the total quantity of heat in a body is simply proportional to the elevation of its temperature above the temperature of absolute privation of heat; or, in the notation of the preceding article,

$$\psi \cdot \tau = \tau, \ \psi' \cdot \tau = 1, \text{ and}$$
$$Q = \mathfrak{k} (\tau - \kappa) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (86.)$$

$\mathfrak{k}$ being the real specific heat of the body.

If this value be substituted for the quantity of heat Q, in all the formulæ, from 67 to 80 inclusive, which are founded simply on the definition of expansive heat, it reproduces all the formulæ which, in this and the other paper referred to, have been deduced directly from the hypothesis. In the sequel I shall apply one of these formulæ to the calculation, from the experiments of Professor Thomson and Mr Joule on the heating of currents of air by friction, of approximate values of the absolute temperature corresponding to total privation of heat, that the mutual consistency of those values may serve as a test of the soundness of the hypothesis, and the accuracy of the formulæ deduced from it.

(57.) Before proceeding further, it may be desirable to point out how far this hypothesis agrees with, and how far it differs from, that proposed by Mr Herapath and Mr Waterston, which supposes bodies to consist of extremely small and perfectly elastic particles, which fly about in all directions with a velocity whose half-square is the mechanical equivalent of the heat possessed by unity of weight, and are prevented from dispersing by their collisions with each other and with the particles of surrounding bodies. Let v be the velocity of motion, then

$$\frac{v^2}{2g} = Q$$

represents the heat possessed by unity of weight, expressed in terms of the force of gravity.

The expansive pressure due to such motions is found by conceiving a hard, perfectly elastic plane of the area unity to be opposed to the collision of the particles, and calculating the pressure which would be required to maintain its position against them. If all the particles were to strike and rebound from such a plane at right angles, the pressure would be represented thus:

$$\frac{v^2}{g} \cdot \frac{1}{V}$$

where V is the volume which contains so many particles as amount to unity of weight. But the particles are supposed to fly in equal numbers in all directions. Then if θ denote the angle of incidence on the plane

$$\frac{\sin\theta \, d\theta}{\int_0^{\frac{\pi}{2}} \sin\theta \, d\theta} = \sin\theta \, d\theta$$

represents the proportion of the whole particles which fly in those directions which make the angle θ with the normal to the plane. Of this proportion, again,

the fraction $\cos\theta$ only strikes the plane; while the force of the blow also is less than that of a normal blow in the ratio $\cos\theta : 1$. Hence the mean force of collision is

$$\int_0^{\frac{\pi}{2}} \cos^2\theta \sin\theta \, d\theta = \frac{1}{3}$$

of the force of a perpendicular collision; so that the expansive pressure is represented by

$$\frac{1}{3} \cdot \frac{v^2}{g} \cdot \frac{1}{V} = \frac{2}{3} \cdot \frac{Q}{V} = P$$

Hence, according to this hypothesis, we should have for a perfect gas

$$PV = \frac{2}{3} Q$$

or the product of the pressure and volume of a mass of a perfect gas equal to two-thirds of the mechanical equivalent of its total heat.

It is known, however, that the product of the pressure and volume of a mass of sensibly perfect gas is only about four-tenths of the equivalent of its total heat. The hypothesis, therefore, requires modification.

By supposing the particles to attract each other, or to be of appreciable bulk compared with the distances between them, the ratio in question is diminished; but either of these suppositions is inconsistent with the perfectly gaseous condition.

It appears to me, that, besides this difficulty connected with the gaseous condition, there exists also great difficulty in conceiving how the hypothesis can be applied to the solid condition, in which the particles preserve definite arrangements. The limited amount of time and attention, however, which I have hitherto bestowed on this hypothesis, is not sufficient to entitle me to pronounce whether these difficulties admit of a solution.

(58.) The idea of ascribing expansive elasticity to the centrifugal force of vortices or eddies in elastic atmospheres surrounding nuclei of atoms, originated with Sir Humphry Davy. The peculiarity of the view of the hypothesis taken in this paper consists in the function ascribed to the nuclei or central physical points of the atoms, which, besides retaining the atmospheres round them by their attraction, are supposed, by their actions on each other, to constitute the medium which transmits radiant heat and light; so that heat is radiant or thermometric, according as it affects the nuclei or their atmospheres.

In this form the hypothesis of Molecular Vortices is not a mere special supposition, to elucidate the theory of expansive heat, but becomes connected with the theory of the elasticity of matter in all conditions, from solid to gaseous, and with that of the transmission of radiations.

I have already investigated mathematically the consequences of this hypothesis by two different processes, which are necessarily somewhat complicated.

When the question, however, is confined to the relations between temperatures and quantities of heat, a more simple process may be followed, analogous to that which has been applied in the preceding article to the hypothesis of Molecular Collisions.

If a mass of elastic fluid, so much rarefied that the effect of molecular attraction is insensible, be entirely filled with vortices, eddies, or circulating currents of any size and figure, so that every particle moves with the common velocity w, then, if the planes of revolution of these eddies be uniformly distributed in all possible positions, it follows, from reasoning precisely similar to that employed in the preceding article, that the pressure exerted by the fluid against a plane, in consequence of the centrifugal force of the eddies, has the following value in terms of gravity:—

$$\frac{1}{3} \cdot \frac{w^2}{g} \cdot \frac{1}{\mathrm{V}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (87.)$$

or two-thirds of the hydrostatic pressure due to the velocity of the eddies w; V being, as before, the volume occupied by unity of weight.

It is, however, reasonable to suppose, that the motion of the particles of atomic atmospheres does not consist merely in circulating currents; but that those currents are accompanied with a certain proportionate amount of vibration,—a kind of motion which does not produce centrifugal force. To these we have to add the oscillations of the atomic nuclei, in order to obtain the mechanical equivalent of the whole molecular motions; which is thus found to be expressed for unity of weight by

$$k \frac{w^2}{2g} \cdot = \mathrm{Q} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (88.)$$

k being a specific coefficient. Hence it follows (denoting $\frac{2k}{3}$ by N), that the expansive pressure due to molecular motions in a perfect gas, is equal to the mechanical equivalent of those motions in unity of volume multiplied by a specific constant

$$\mathrm{N} \cdot \frac{\mathrm{Q}}{\mathrm{V}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (89.)$$

The coefficient N has to be determined by experiment; its value for atmospheric air is known to be between 0·4 and 0·41.

In order to account for the transmission of pressure throughout the molecular atmospheres, it is necessary to suppose them possessed of a certain amount of inherent elasticity, however small, varying proportionally to density, and independent of heat. Let this be represented by

$$\frac{h}{\mathrm{V}}$$

then

$$\mathrm{P} = (\mathrm{N}\,\mathrm{Q} + h) \frac{1}{\mathrm{V}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (90.)$$

is the total pressure of a perfect gas.

Equilibrium of heat and pressure between portions of two different perfect gases in contact requires that the pressures independent of heat, and the pressures caused by heat, shall separately be in equilibrio. Let the suffixes a and b be used to distinguish quantities relative to two different substances in the perfectly gaseous condition. Then the first condition of equilibrium is expressed as follows:—

$$\left(\frac{h}{V}\right)(a) = \left(\frac{h}{V}\right)(b) \quad . \quad . \quad . \quad . \quad . \quad . \quad (91.)$$

that is to say, *the densities of two perfect gases in equilibrio are inversely proportional to the coefficients of elasticity of their atomic atmospheres.*

The second condition is expressed as follows:—

$$\left(\frac{NQ}{V}\right)(a) = \left(\frac{NQ}{V}\right)(b)$$

which, being taken in connection with the first condition, gives

$$\left(\frac{N}{h}Q\right)(a) = \left(\frac{N}{h}Q\right)(b) \quad . \quad . \quad . \quad . \quad . \quad (92.)$$

Now by Equation 90, we have

$$\frac{N}{h}Q = \frac{PV}{h} - 1$$

Hence the condition of equilibrium of heat between two perfect gases is

$$\left(\frac{PV}{h}\right)(a) = \left(\frac{PV}{h}\right)(b) \quad . \quad . \quad . \quad . \quad . \quad (93.)$$

consequently, *temperature may be measured by the product of the pressure and volume of a perfect gas, divided by a coefficient, which is proportional to the volume of the gas at a standard pressure and temperature.*

Temperatures thus measured are reckoned from the point known as the *zero of gaseous tension*, or *absolute zero of a perfect gas thermometer*, 274°·6 centigrade below the temperature of melting ice.

Let V_0 denote the volume of unity of weight of a perfect gas, at a standard pressure P_0, and absolute temperature τ_0; then any other absolute temperature has the following value:—

$$\tau = \tau_0 \frac{PV}{P_0 V_0} = \frac{\tau_0}{P_0 V_0}(NQ + h) \quad . \quad . \quad . \quad . \quad . \quad (94.)$$

while the absolute temperature of total privation of heat is

$$\kappa = \tau_0 \frac{h}{P_0 V_0} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (94\,A.)$$

Hence it appears that quantity of heat in unity of weight bears the following relation to temperature,—

$$Q = \frac{1}{N}(PV - h) = \frac{P_0 V_0}{N \tau_0}(\tau - \kappa) \quad . \quad . \quad . \quad . \quad . \quad (95.)$$

in which, if we substitute the symbol of real specific heat,

$$\mathfrak{k} = \frac{P_0 V_0}{N \tau_0} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (96.)$$

we obtain the formula already given (86) for the relation between heat and temperature.*

(59.) The introduction of the value given above of the quantity of heat in terms of temperature, into the formula 67, gives for the latent heat of a small expansion $d\,V$ at constant temperature

$$(\tau - \kappa)\frac{d\,P}{d\,\tau} \cdot d\,V \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (97.)$$

The formulæ 79 and 82, for the proportion of heat rendered available by an expansive engine working to the greatest advantage, becomes

$$\frac{\tau_1 - \tau_2}{\tau_1 - \kappa} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (98.)$$

or the ratio of the difference between the temperatures of receiving and emitting heat, to the elevation of the former temperature above that of total privation of heat. This is the law already arrived at by a different process in Section V. of this paper.

When the same substitution is made in Equation 80, which represents the total energy, whether as heat or as compressive power, which must be applied to unity of weight of a substance to produce given changes of heat and volume, the following result is obtained:—

$$\begin{aligned} d\,.\,\Upsilon = d\,Q + d\,.\,S &= \left\{ \mathfrak{k} + f'(\tau) + (\tau - \kappa)\int \frac{d^2 P}{d\,\tau_2}\, d\,V \right\} d\,\tau \\ &\quad + \left\{ (\tau - \kappa)\frac{d\,P}{d\,\tau} - P \right\} d\,V \\ &= d\,.\,\left\{ \mathfrak{k}\,\tau + f(\tau) + \left((\tau - \kappa)\frac{d}{d\,\tau} - 1 \right) \int P\,d\,V \right\} \quad . \quad . \quad . \quad (99.) \end{aligned}$$

As it cannot be simplified, it is unnecessary here to recapitulate the investigation, which leads to the conclusion that the functions $f(\tau)$ and $f'(\tau)$ have the following values:—

$$f(\tau) = \mathfrak{k}\,N\left(\kappa\,\text{hyp. log.}\,\tau + \frac{\kappa^2}{\tau}\right) ; f'(\tau) = \mathfrak{k}\,N\left(\frac{\kappa}{\tau} - \frac{\kappa^2}{\tau^2}\right) \quad . \quad . \quad (99\text{ A.})$$

We have thus reproduced Equation 26 of the paper formerly referred to, on the Centrifugal Theory of Elasticity.

The coefficient of the variation of temperature in the first form of Equation 99 is the specific heat of the substance at constant volume. Denoting this by K_v, the formula becomes

$$d\,.\,\Upsilon = K_v\,.\,d\,\tau + \left\{ (\tau - \kappa)\frac{d\,P}{d\,\tau} - P \right\} d\,V \quad . \quad . \quad . \quad (100.)$$

* See Appendix, Note A.

SUB-SECTION 4. *Thermic Phenomena of Currents of Elastic Fluids.*

(60.) When a gas previously compressed is allowed to escape through small apertures, as in the experiments of Mr JOULE and Professor THOMSON, and has its velocity destroyed entirely by the mutual friction of its particles, without impediment from any other substance, and without conduction of heat to or from any other substance; then its condition is expressed by making

$$d\Psi = 0$$

that is to say,

$$-d\tau = \frac{1}{K_V} \cdot \left\{ \tau \cdot \left(\frac{dP}{d\tau} - \frac{P}{\tau} \right) - \kappa \frac{dP}{d\tau} \right) dV \quad . \quad . \quad . \quad (101.)$$

If we assume (as is really the case in the experiments) that the specific heat of the gas at constant volume does not sensibly vary within the limits of the experiments as to temperature and volume, so that K_V is sensibly constant, and also that the variation of temperature is very small as compared with the absolute temperatures, then we have the following approximate integral:—

$$-\Delta\tau = \frac{1}{K_V} \cdot \left\{ \tau \int_{V_1}^{V_2} \left(\frac{dP}{d\tau} - \frac{P}{\tau} \right) dV - \kappa \int_{V_1}^{V_2} \frac{dP}{d\tau} \cdot dV \right\} \quad . \quad . \quad (102.)$$

which represents the cooling effect of an expansion from the volume V_1 to the volume V_2.

If it were possible to obtain any substance in the state of perfect gas to be used in experiments of this kind, the first integral in the above expression would disappear, because, for a perfect gas,

$$\frac{dP}{d\tau} = \frac{P}{\tau}$$

and as the other term is negative, the result would be a slight heating effect. As no gas, however, is perfect, and as $\frac{dP}{d\tau}$ always exceeds $\frac{P}{\tau}$, the mode of reducing the experimental data is to calculate the value of the first term, which represents the effect of cohesion, from the known properties of the gas, to subtract from it the actual cooling, and from the remainder to compute values of κ, the temperature of absolute privation of heat, according to the following formula:—

$$\kappa = \frac{\frac{1}{K_V}\tau \int_{V_1}^{V_2} \left(\frac{dP}{d\tau} - \frac{P}{\tau} \right) dV - (-\Delta\tau)}{\frac{1}{K_V} \int_{V_1}^{V_2} \frac{dP}{d\tau} dV} \quad . \quad . \quad . \quad . \quad . \quad (103.)$$

When the gas is nearly perfect, as in the case of atmospheric air, it is unnecessary to take into consideration its deviation from the perfect condition in computing the integral in the denominator; whose approximate value is found to be

$$\frac{P_0 V_0}{K_V \tau_0} \cdot \text{hyp. log} \frac{V_2}{V_1} = N \cdot \text{hyp. log} \frac{P_1}{P_2} \text{ nearly } (\tau \text{ being nearly constant}),$$

and K_V nearly $= k$.

The value of the integral in the numerator is found as follows:—

The Centrifugal Theory of Elasticity indicates that the pressure of an imperfect gas may be represented by the following formula:—

$$P = P_0 \frac{V_0}{V} \left\{ \frac{\tau}{\tau_0} + A_0 - \frac{A_1}{\tau} - \frac{A_2}{\tau^2} - \&c. \right\} \quad . \quad . \quad . \quad . \quad . \quad (104.)$$

where V_0 is the volume in the perfectly gaseous state, at a standard pressure P_0, and absolute temperature τ_0, and A_0, A_1, &c., are a series of functions of the density, to be determined empirically. From this formula it is easily seen that

$$\frac{dP}{d\tau} - \frac{P}{\tau} = P_0 \frac{V_0}{V} \left\{ -\frac{A_0}{\tau} + \frac{2A_1}{\tau^2} + \&c. \right\} \quad . \quad . \quad . \quad . \quad . \quad (105.)$$

so that the first term in the numerator of the expression (103) has the following value:—

$$\frac{\tau}{K_V} \int_{V_1}^{V_2} \left(\frac{dP}{d\tau} - \frac{P}{\tau} \right) dV = \frac{P_0 V_0}{K_V} \left\{ -\int_{V_1}^{V_2} \frac{A_0}{V} dV + \frac{2}{\tau} \int_{V_1}^{V_2} \frac{A}{\tau} dV + \&c. \right\} \quad . \quad . \quad (106.)$$

in which $\frac{P_0 V_0}{K_V} = N \tau_0$ nearly.

In order to represent correctly the result of M. Regnault's experiments on the elasticity and expansion of gases, it was found sufficient to use, in the formula for the pressure (104), the first three terms; and the functions of the density which occur in these terms, as determined empirically from the experiments, were found to have the following values, in which the unit of volume is the theoretical volume of unity of weight of air under the pressure of one atmosphere, at the temperature of melting ice,* and the values of the constants are given for the centigrade scale.

$$\frac{A_0}{V} = b \left(\frac{1}{V} \right)^{\frac{3}{2}} ; \quad \frac{A_1}{V} = a \left(\frac{1}{V} \right)^{\frac{8}{5}} \quad . \quad . \quad . \quad . \quad (107.)$$

Com. log $b = \bar{3}{\cdot}8181545$; Com. log $a = 0{\cdot}3176168$.

Hence it appears that the integrals in the formula (106) have the following values:—

$$\int_{V_1}^{V_2} \frac{A_0}{V} dV = 2b \cdot \Delta \cdot \left(\frac{1}{V} \right)^{\frac{1}{2}} ; \quad \frac{2}{\tau} \int_{V_1}^{V_2} \frac{A_1}{V} dV = \frac{10}{3} \cdot \frac{a}{\tau_0} \cdot \frac{\tau_0}{\tau} \cdot \Delta \cdot \left(\frac{1}{V} \right)^{\frac{4}{5}} \quad . \quad (107\,A.)$$

in which the common logarithms of the constants are

* This unit of volume is greater than the actual volume of air, under the circumstances described, in the ratio of 1·00085 to 1.

$$\text{Com. log } 2\,b = \bar{2}{\cdot}1101845\ ;\ \log \frac{10}{3} \cdot \frac{a}{\tau_0} = \bar{2}{\cdot}4017950\ ;$$

and these values suit any scale of temperatures.

In calculating, for use in these formulæ, the densities $\frac{1}{V}$ from the observed pressures, it is sufficiently near the truth, in the case of air, to use the approximate equation

$$\frac{1}{V} = \frac{\tau_0}{\tau} \cdot P \text{ (in atmospheres).}$$

The common logarithm of τ_0, the absolute temperature of melting ice, for the centigrade scale, is 2·4387005.

The constant N for atmospheric air is 0·4 nearly; therefore

$$\text{Com. log } (N \times \text{hyp. log } 10) = \bar{1}{\cdot}9642757.$$

The following, therefore, is the approximate value of the formula (103), to be used (with the numerical constants already given) in reducing the experiments of Mr JOULE and Professor THOMSON on atmospheric air, so as to obtain approximate values of the absolute temperature of total privation of heat:—

$$\kappa = \left\{ N\,\tau_0 \left(\frac{10\,a}{3\,\tau_0} \cdot \left(\frac{\tau_0}{\tau}\right)^{\frac{1}{8}} \Delta \cdot (P^{\frac{7}{8}}) - 2\,b \left(\frac{\tau_0}{\tau}\right)^{\frac{1}{2}} \Delta \cdot (P^{\frac{1}{2}}) \right) - (-\Delta\,\tau) \right\}$$
$$\div N \text{ hyp. log } 10 \times \Delta \cdot \text{com. log } \frac{1}{P} \quad . \quad . \quad . \quad . \quad . \quad (108.)$$

In using this formula, the mean absolute temperature should be taken as the value of τ.

The following table shews the values of the quantity κ, computed from ten mean experimental data, taken respectively from the first ten series of experiments described in the recent paper of Messrs JOULE and THOMSON, in the supplementary number of the *Philosophical Magazine* for December 1852. The temperatures in the table, for the sake of convenience, are reduced to the centigrade scale, because that scale has been used throughout the previous sections of this paper.

The final pressure in each case was that of the atmosphere.

Series and Number of Experiments.	Quantity of Air used in cubic inches per second.	Initial Temperature. — Centigrade.	Mean Absolute Temperature τ.	Initial Pressure in Atmospheres.	Actual Cooling. — Centigrade.	Calculated Cooling by Cohesion. — Centigrade.	Difference.	Approximate Values of z, the Absolute Temperature of Total Privation of Heat. — Centigrade.
Series I. Mean of 3 Experiments,	Not noted.	16°·11	290°·4	3·61	0°·76	1°·70	0°·94	1°·83
Series II. Mean of 5 Experiments,	Not noted.	71·11	345·6	2·64	0·28	0·70	0·42	1·08
Series III. Mean of 7 Experiments,	6·4	77·11	351·5	4·28	0·37	1·25	0·88	1·51
Series IV. Mean of 3 Experiments,	5·6	17·39	281·7	3·04	0·56	1·49	0·93	2·09
Series V. Mean of 2 Experiments,	8·4	3·89	278·0	4·11	1·00	2·18	1·18	2·087
Series VI. Mean of 4 Experiments.	11·2	5·21	279·4	4·40	0·92	2·31	1·39	2·345
Series VII. Mean of 5 Experiments,	1·4	17·87	292·3	1·894	0·23	0·66	0·43	1·683
Series VIII. Mean of 5 Experiments,	2·8	18·04	292·5	2·41	0·31	0·93	0·62	1·762
Series IX. Mean of 4 Experiments,	5·6	18·21	292·6	2·92	0·43	1·385	0·955	2·228
Series X. Mean of 3 Experiments,	11·2	18·37	292·6	4·11	0·72	1·93	1·21	2·14

Professor THOMSON and Mr JOULE have expressed the opinion, which is undoubtedly correct, that those experiments in which the largest quantities of air were used were the least liable to error from disturbing causes, such as the conduction of heat.

Now it may be observed in the preceding table, that the calculated values of κ are generally greatest, and the discrepancies amongst them least, for the experiments in which most air was used. To illustrate this, the results of the last eight series are arranged below in the order of the quantities of air employed.

Cubic inches per second,	1·4	2·8	5·6	5·6	6·4	8·4	11·2	11·2
Values of κ, ...	1·683	1·762	2·09	2·228	1·51	2·087	2·345	2·14

It is further to be remarked, that the discrepancy between the highest and the lowest of the values of κ is

$$2°{\cdot}345 - 1°{\cdot}08 = 1°{\cdot}265 \text{ centigrade:}$$

a quantity which corresponds to a difference of less than *one three-hundredth part* in computing the proportion of heat converted into mechanical power by any ordinary expansive engine, according to the formula (98), which has been deduced from the hypothesis of Molecular Vortices.

The experiments, therefore, may be considered as tending to prove, that the formulæ deduced from this hypothesis are sufficiently correct for practical purposes; and also as affording a strong probability that the principles to which it leads are theoretically exact, and that the temperature of absolute privation of heat is a real fixed point on the scale, somewhat more than two centigrade degrees above the absolute zero of a perfect gas-thermometer (which is, of course, an imaginary point); that is to say, about $272\frac{1}{2}$ centigrade degrees, or $490\frac{1}{2}$ degrees of FAHRENHEIT, below the temperature of melting ice.

If these conclusions be correct, it follows, that when the temperatures T_1 and T_2, between which an expansive engine works, are measured from the ordinary zero points of the centigrade and of FAHRENHEIT's scales respectively, the following are the utmost proportions of the total heat expended which it can be made to convert into mechanical power:—

$$\left.\begin{array}{ll} \text{For the centigrade scale,} & \dfrac{T_1 - T_2}{T_1 + 272\frac{1}{2}} \\ \text{For FAHRENHEIT's scale,} & \dfrac{T_1 - T_2}{T_1 + 458\frac{1}{2}} \end{array}\right\} \quad . \quad . \quad . \quad . \quad (109.)$$

In the fifth section of this paper, where a comparison is made between the actual duty of theCornish engine at Old Ford, as determined by Mr WICKSTEED, and the greatest possible duty which could be obtained from a given quantity of heat by a theoretically perfect engine working between the same temperatures, the constant κ is treated as being so small that it may be neglected in practice.

If the value of κ is really 2°·1 centigrade, as computed above, the calculated maximum theoretical duty in Section V. is too small by about one one-hundred-and-ninetieth part of its amount,—a quantity of no practical importance in such calculations.

(61.) It may be anticipated, that when Mr JOULE and Professor THOMSON shall have performed experiments on the thermic phenomena exhibited by air in more copious currents, and by gases of more definite composition, and more simple laws of elasticity, much more precise results will be obtained.

When a gas deviating considerably from the perfectly gaseous condition, or a vapour near the point of saturation, is employed, it will no longer be sufficiently accurate to treat the specific heat at constant volume as a constant quantity, nor the cooling effect as very small. It will therefore be necessary to employ, for the reduction of the experiments, the integral form of equation (99); that is to say,

$$0 = \Delta\,\Psi = \Delta\left\{ \mathfrak{k}\,\tau + \mathfrak{k}\,\mathrm{N}\,\kappa\left(\text{hyp. log}\,\tau + \frac{\kappa}{\tau}\right) + \left((\tau-\kappa)\frac{d}{d\tau} - 1\right)\int \mathrm{P}\,d\,\mathrm{V}\right\}$$

$$= \mathfrak{k}\,(\tau_2 - \tau_1) + \Delta\int\left(\tau\frac{d\,\mathrm{P}}{d\,\tau} - \mathrm{P}\right)d\,\mathrm{V}$$

$$- \kappa\left\{\Delta\int\frac{d\,\mathrm{P}}{d\,\tau}d\,\mathrm{V} - \mathfrak{k}\,\mathrm{N}\left(\Delta\cdot\frac{\kappa}{\tau} + \Delta\,\text{hyp. log}\,\tau\right)\right\} \quad . \quad . \quad (110.)$$

(62.) Preliminary to the application of this equation, it is necessary to determine the mechanical value of the real specific heat $\mathfrak{k}$. Supposing the law which connects the pressure, density, and temperature of the gas to be known, it is sufficient for this purpose to have an accurate experimental determination, either of the apparent specific heat at constant pressure for a given temperature, or the velocity of sound in the gas under given circumstances.

First, let us suppose that the apparent specific heat at constant pressure is known.

The value of this coefficient (Centrifugal Theory of Elasticity, art. 12) is

$$\mathrm{K_P} = \mathfrak{k} + (\tau-\kappa)\left\{\frac{\mathrm{P_0\,V_0}}{\tau_0}\,\frac{\kappa}{\tau^2} + \int\frac{d^2\,\mathrm{P}}{d\,\tau^2}\,d\,\mathrm{V} + \frac{\left(\frac{d\,\mathrm{P}}{d\,\tau}\right)^2}{-\frac{d\,\mathrm{P}}{d\,\mathrm{V}}}\right\} \quad . \quad . \quad . \quad . \quad . \quad (111.)$$

In order that the lower limit of the integral may correspond with the condition of perfect gas, it is convenient to transform it into one in terms of the density. Let D be the weight of unity of volume, then

$$\int\frac{d^2\,\mathrm{P}}{d\,\tau^2}\,d\,\mathrm{V} = -\int_0^{\mathrm{D}}\frac{1}{\mathrm{D}^2}\cdot\frac{d^2\,\mathrm{P}}{d\,\tau^2}\,d\,\mathrm{D} \quad . \quad . \quad . \quad . \quad (111\,\mathrm{A.})$$

If, then, we have the pressure of the gas under consideration expressed by the the following approximate formula:—

$$P = \frac{P_0 V_0}{V}\left\{\frac{\tau}{\tau_0} + A_0 - \frac{A_1}{\tau}\right\}$$

The following will be the values of the functions of the pressure which enter into the above equation:—

$$\left.\begin{aligned}
&\frac{dP}{d\tau} = \frac{P_0 V_0}{V}\left\{\frac{1}{\tau_0} + \frac{A_1}{\tau^2}\right\};\quad \frac{d^2 P}{d\tau^2} = -2\frac{P_0 V_0}{V}\cdot\frac{A_1}{\tau^3} = -2 P_0 V_0 \frac{A_1 D}{\tau^3}\\
&\int \frac{d^2 P}{d\tau^2} dV = -\int_0^D \frac{1}{D^2}\cdot\frac{d^2 P}{d\tau^2} dD = +2\frac{P_0 V_0}{\tau^3}\cdot\int_0^D \frac{A_1}{D} dD;\\
&-\frac{dP}{dV} = D^2\frac{dP}{dD} = P_0 V_0 D^2\left\{\frac{\tau}{\tau_0} + \frac{d.A_0 D}{dD} - \frac{1}{\tau}\cdot\frac{d.A_1 D}{dD}\right\}\\
&\therefore \frac{\left(\frac{dP}{d\tau}\right)^2}{-\frac{dP}{dV}} = P_0 V_0 \cdot \frac{\left(\frac{1}{\tau_0} + \frac{A_1}{\tau^2}\right)^2}{\frac{\tau}{\tau_0} + \frac{d.A_0 D}{dD} - \frac{1}{\tau}\cdot\frac{d.A_1 D}{dD}}
\end{aligned}\right\} \quad . \qquad (111\text{ B.})$$

To illustrate the application of these formulæ, let us calculate the difference between the real specific heat, and the apparent specific heat, at constant pressure, of carbonic acid gas, at the temperature of melting ice, and at the density which, if the gas were perfect, would correspond to a pressure of one atmosphere at the temperature of melting ice. Let this density be denoted by D_0, and its reciprocal by V_0. As the constants have been deduced from M. REGNAULT'S experiments, the calculations will be made in French measures and for the latitude of Paris.

The actual density of carbonic acid at 0° centigrade, and under one atmosphere of pressure, exceeds the theoretical density, in the perfectly gaseous state, in the ratio of 1·0065 to 1 nearly. Hence the height of a homogeneous atmosphere of actual carbonic acid at 0° centigrade being . . . . 5225·5 metres,
the corresponding height in the state of perfect gas is $P_0 V_0$ = 5259·5 „

and $\frac{P_0 V_0}{\tau} = 19{\cdot}53$ metres per centigrade degree = 62·84 feet.

The functions which express the influence of density on the deviation of carbonic acid gas from the perfectly gaseous state, have the following values:—

$$\left.\begin{aligned}
&A_0 = b\cdot\frac{D}{D_0};\quad A_1 = a\cdot\frac{D}{D_0};\ \text{when}\\
&\text{Com. log } b = \bar{3}{\cdot}1083932;\ \text{Com. log } a = 0{\cdot}3344538\\
&b = 0{\cdot}00128349 \qquad a = 2{\cdot}16;\\
&\int_0^D \frac{A_1}{D} dD = \int_0^{\frac{D}{D_0}} A_1 \frac{D_0}{D}\cdot\frac{d.D}{D_0} = a\cdot\frac{D}{D_0};\ \frac{d}{dD}\cdot A_0 D\\
&= 2b\cdot\frac{D}{D_0};\ \frac{d}{dD}\cdot A_1 D = 2a\frac{D}{D_0}
\end{aligned}\right\} \quad . \qquad (111\text{ C.})$$

For the purposes of a first approximation, we may assume that the value of κ

already found is sufficiently near the truth, viz., 2°·1 centigrade; so that, in the present instance $\tau - \kappa = 272°{\cdot}5$ centigrade.

Then we find the following results when $\tau = \tau_0$, and $D = D_0$;

		Metres.	Feet.
$(\tau - \kappa)\frac{P_0 V_0}{\tau_0} \cdot \frac{\kappa}{\tau^2}$	= per centigrade degree, ...	0·145	0·48
$(\tau - \kappa)\int \frac{d^2 P}{d\tau^2} dV$	= „ „ „	0·150	0·49
$\text{Sum} = K_V - k$	= excess of apparent specific heat at constant volume above real specific heat, ...	0·295	0·97
$(\tau - \kappa)\frac{\left(\frac{dP}{d\tau}\right)^2}{-\frac{dP}{dV}}$	= difference between apparent specific heats at constant volume and at constant pressure, .	19·565	64·19
$K_P - k$	= excess of apparent specific heat at constant pressure above real specific heat, . . .	19·860	65·16

$\frac{5}{9}$ of the above quantities are of course the corresponding quantities for FAHRENHEIT'S scale.

Secondly, If the velocity of sound in the gas is given, let this $= u$. Then we know that

$$u^2 = g \cdot \frac{dP}{dD} \cdot \frac{K_P}{K_V} \quad . \quad . \quad . \quad . \quad . \quad . \quad (112.)$$

in which

$$\frac{dP}{dD} = P_0 V_0 \left\{ \frac{\tau}{\tau_0} + \frac{d \cdot A_0 D}{dD} - \frac{1}{\tau} \frac{d \cdot A_1 D}{dD} \right\} \quad . \quad . \quad . \quad (112\text{ A.})$$

So that from the velocity of sound we can calculate the ratio of the specific heats at constant pressure and at constant volume. Let this ratio be denoted by γ, and let

$$K_V = k + c; \quad K_P = k + c'; \text{ then}$$

$$\gamma = \frac{k + c'}{k + c}; \text{ and } k = \frac{c' - \gamma c}{\gamma - 1} \quad . \quad . \quad . \quad . \quad (112\text{ B.})$$

in which c' and c are to be calculated as above.*

(63.) In using the formula (110) for a gas whose pressure is represented by the formula,

$$P = \frac{P_0 V_0}{V} \left\{ \frac{\tau}{\tau_0} + A_0 - \frac{A_1}{\tau} \right\}$$

the integrals may be transformed so as to be taken, with respect to the density, as in the preceding article. Thus we obtain

* See Appendix, Note B.

$$\Delta\int\left(\tau\frac{dP}{d\tau}-P\right)dV=-\Delta\int\frac{1}{D^2}\left(\tau\frac{dP}{d\tau}-P\right)dD=-P_0V_0\Delta\left\{\frac{2}{\tau}\int_0^D\frac{A_1}{D}dD-\int\frac{A_0}{D}dD\right\}$$

$$\int\frac{dP}{d\tau}dV=-\int\frac{1}{D^2}\cdot\frac{dP}{d\tau}dD=-P_0V_0\Delta\left(\frac{1}{\tau_0}\text{ hyp. log }D+\frac{1}{\tau^2}\int\frac{A_1}{D}dD\right) \quad . \quad (113.)$$

For carbonic acid, the first of these formulæ becomes simply

$$+P_0V_0\left\{\frac{2a}{D_0}\left(\frac{D_1}{\tau_1}-\frac{D_2}{\tau_2}\right)-\frac{b}{D_0}\left(D_1-D_2\right)\right\}$$

and the second,

$$+P_0V_0\left\{\frac{1}{\tau_0}\text{ hyp. log }\frac{D_1}{D_2}-\frac{a}{D_0}\left(\frac{D_1}{\tau_1^2}-\frac{D_2}{\tau_2^2}\right)\right\} \quad . \quad (113\text{ A.})$$

Glasgow, 27*th December* 1852.

APPENDIX.

Note A (to Article 58). Since this section was read, the theoretical views relative to the relation between heat and temperature contained in it and the previous sections of this paper, have received a strong confirmation by the publication by M. Regnault of the fact, that he has found the specific heat of air to be sensibly constant at all temperatures from $-30°$ centigrade to $+225°$, and at all pressures from one to ten atmospheres (*Comptes Rendus*, 18th April 1853); so that equal lengths on the scale of the air thermometer represent equal quantities of heat.

Note B (to Article 62). Until very recently, there existed no exact experimental determination of the specific heat of any gas. The specific heat of air at constant pressure, as compared with that of water, was calculated theoretically in the previous part of this paper, from Joule's equivalent and the velocity of sound, and found to be 0·24. This value has since been confirmed very closely by Mr Joule's experiments, whose mean result was 0·23, and still more exactly by M. Regnault's experiments, already referred to, which give the value 0·2379. The following table shews the results of the application of the formulæ of this paper to the specific heats of five different gases at constant pressure, selected from M. Regnault's table (*Comptes Rendus*, 18th April), as being those in which the velocity of sound can be computed, and has been determined experimentally. The table shews also a comparison of the calculated and observed velocities of sound. This table appeared originally, in French measures, in the *Philosophical Magazine* for June 1853: the metres are here reduced to feet. K_P, K_V, and K_W, are expressed in feet of fall per centigrade degree. K_W (Joule's equivalent) $=1389·6$.

Gases.	Experimental Data.			Theoretical Results.			Velocity of Sound at 0° Cent.		
	PV at 0° C. Regnault.	$K_P \div K_W$ Regn.	K_P	$K_P - K_V$	K_V	$K_P \div K_V$	By Theory.	By Observation.	Observers.
	Feet.		Feet.	Feet.	Feet.		Ft. per sec.	Ft. per sec.	
Air, . .	26214·4	0·2379	330·6	96·0	234·6	1·4094	1090·4	1090·5	Bravais & Martins.
								1090·1	Moll & Van Beek.
Oxygen, . .	23710·4	0·2182	303·2	86·8	216·4	1·4014	1036·4	1042·3	Dulong.
Hydrogen, .	378819·0	3·4046	4731·0	1388·0	3343·0	1·4150	4153·3	4165·1	Dulong.
Carbonic oxide,	27097·8	0·2479	344·5	99·25	245·25	1·4047	1106·8	1107·0	Dulong.
Carbonic acid,	17144·7	0·2169	300·7	64·2	236·5	1·2714	837·55	858·28	Dulong.

The real specific heat of carbonic acid gas is 235·5 feet of fall per centigrade degree. That of the other gases does not differ from the apparent specific heat at constant volume by an amount appreciable in practice.

NATURAL HISTORY

XLI.—*On Nitric Acid as a Source of the Nitrogen found in Plants.*

By George Wilson, M.D.

(Read 4th April 1853.)

The source from which plants obtain nitrogen, which is now recognised as one of their most important elements, has, from the first recognition of its importance, been matter of dispute. Latterly, however, chemists and physiologists have pretty unanimously come to the conclusion, that a large (perhaps the largest) part of the nitrogen of vegetables is derived from ammonia; whilst much discussion has been carried on as to the question, Is any part of their nitrogen yielded by nitric acid?

The most able advocate in this country of the claims of ammonia is Dr Gregory. The most able advocate of the claims of nitric acid is Professor Johnston of Durham, and the opposite conclusions to which accomplished chemists like these have come in reference to the point in dispute, have perplexed botanists, who know not which view to prefer. The extent to which they are pressed by this dilemma, has been so strongly represented to me by Dr Balfour, that I have engaged to bring the subject, as I now do, before this Society. I shall sedulously avoid discussing the question in a polemical spirit; and, as the shortest and most satisfactory way of doing justice to the rival views, I shall select Dr Gregory's clear and concise statement, as representing the opinions of those who deny that nitric acid is part of the food of plants; and then proceed to state what appear to me conclusive proofs that nitric acid does supply plants with nitrogen.

Dr Gregory writes thus:—"Let us now attend to the nitrogen of plants. This, as already stated, is supplied to wild plants entirely by the air, and, so far as we know, only in the form of ammonia. Some authors have held that nitric acid furnishes nitrogen to plants, and that this acid is formed in the air by thunder-storms, and carried down by the rain. And they point to the occurrence of nitric acid in springs in proof of this. Now it is true that nitric acid is formed in thunder-storms, but in very minute quantity, whereas ammonia is, and must be, present in the air at all times. Indeed there is reason to believe that the nitric acid of storms is produced by the oxidation of the ammonia of the air, as in nitrification, where ammonia is oxidised into nitric acid and water $NH_3 + O_8 = NO_5, 3\,HO$; so that even if nitric acid did yield nitrogen to plants, that nitrogen would be derived from ammonia. This would account, too, for the small amount of nitric acid formed. For if it were produced by the action of electricity on the nitrogen and oxygen of the air, there seems to be no reason why it

should not be formed in very large quantity; while ammonia forms less than one-10,000th of the air, perhaps much less. Nitric acid is only found in springs where decaying organic matter is near them, as in towns, and is formed from the ammonia produced in their decay, by the same process as in nitrification. Besides, while we have no proof that plants decompose nitric acid, which it is certainly possible they may do, we know that many plants, such as tobacco and sunflower, actually produce nitric acid, or, at least, do not destroy that which enters them."*

Thus far Dr Gregory. I at once concede to him that plants are largely indebted to ammonia for the nitrogen found in them; and in support of the belief that they are also indebted to nitric acid for their nitrogen, I adduce the following proofs.

Firstly, The production of nitric acid in the atmosphere during thunderstorms, is a certain, not a questionable fact; and the scale on which it is produced is such as to necessitate its recognition as a portion of the azotised food of plants. That this should have been questioned is perhaps not strange, for the newly-discovered truth that ammonia is generally present in the air, could scarcely fail to throw into temporary oblivion the equally important truth that nitric acid is generally present there also. The name of the great living chemist Liebig is identified with the one discovery, and the name of the great dead chemist Cavendish with the other; and we must not grudge that greater interest should be felt by most in the doings of the living philosopher. But assuredly it is not necessary to set the two truths against each other, as if they were mutually incompatible, or in any respect contradictory. On the other hand, I believe that they are complementary, and form an essential and manifest part of that harmonious adjustment which we everywhere perceive guarding plants and animals against imperfect nourishment or decay.

In the year 1781, Cavendish addressed himself to the task of answering this question, among others, "Why does the passage of an electric spark through a confined portion of air, cause a diminution in its volume?"† He did not give a categorical reply to this question till 1785, when he published his discovery that a mixture of two measures of nitrogen and five measures of oxygen can be *entirely* converted into nitric acid, by sending a succession of electric sparks through it.‡ He had observed the fact, however, in 1781, in the course of the famous experiments which led to the discovery of the composition of water,—a truth to which I refer, because an impression is prevalent, that the conversion of a mixture of nitrogen and oxygen into nitric acid by the electric spark, can only be effected with great difficulty, whereas the undesired and unintended production of this acid, in trials instituted with a totally different object in view, was the chief

* Gregory's *Organic Chemistry*. Third Edition, p. 466.
† *Phil. Trans.*, 1784, p. 119. ‡ *Ibid*, 1785, p. 372.

cause of the delay which attended the announcement that water is not a simple body. CAVENDISH's later experiments were repeated by a Committee of the Royal Society at his own request, and with entire success;* and if any one is slow to repose faith in chemical experiments made in 1785, let me remind him that FARADAY has shewn that every time a friction electric machine is in action, the truth of CAVENDISH's observations may be proved, by no more complex device than the stretching of a piece of paper wetted with solution of potass, across the interval separating two surfaces, between which electric sparks are passing. The potass is quickly changed into nitrate of potass.†

Resting upon these observations of CAVENDISH and FARADAY, I urge the conclusion, that every lightning flash *must* convert a portion of the air into nitric acid; and that in tropical regions where thunder-storms prevail, this acid must be produced largely and almost constantly.

Secondly, As for the proposition that the ammonia of the atmosphere is converted by simple oxidation, as in the process of nitrification at the surface of the earth, into nitric acid, I might leave it unconsidered, for my concern is simply with nitric acid, not with its source. I am quite prepared to admit the probability of atmospheric ammonia undergoing conversion into nitric acid; for although one condition essential to nitrification in the soil, namely, the presence of an alkali or alkaline earth is wanting, yet, from what is known of the intense oxidising power of ozone, we may well believe that when it is developed in the air, as it so certainly and frequently is, it will compel the conversion of ammonia into nitric acid. It will presently, indeed, appear, that, from the recent researches of BARRAL, it is probable that nitric acid is generated in the atmosphere at the expense of ammonia. If this, however, be the case, then we must acknowledge that, in addition to thunder-storms, a force is constantly at work in the air producing nitric acid; and further, that this force is constantly removing from the atmosphere the ammonia on which plants are supposed to be solely dependent for nitrogen.

No data exist from which we can compute, with even an approximation to accuracy, the amount of nitric acid produced by thunder-storms all the world over. It is certainly, however, considerable, as compared with the amount of ammonia in air, and with the amount of nitrogen required by plants.

It is further uncertain how far temperate regions profit by the nitric acid developed by the storms of tropical latitudes; but from the known effects of the winds, and of the diffusive force of gases, in spreading through the atmosphere substances added to one part of it, we cannot doubt that when the heavy rains which so frequently follow thunder-storms, do not at once transfer to the earth the nitric acid which they have produced, it will be conveyed, either free or combined, to immense distances from the spot where it was developed. But upon

* *Phil. Trans.*, 1788, p. 261. † *Electrical Researches*, vol. i., pp. 90, 91.

this point I do not dwell; for I am content with the alternative conclusion, that the nitric acid of thunder-storms either descends at once to the earth, and feeds the most luxuriant vegetation known to us; or that it is diffused through the entire atmosphere, and is available for the nutrition of the plants of all lands.

Thirdly, Rain-water is often found to contain nitric acid in combination with different bases. The most recent observations on this point with which I am acquainted, are those of M. Barral, communicated to the French Academy, and approved by a committee of that body. If Barral's results are confirmed, and are not found to be exceptional, they will compel us to acknowledge a much larger proportion of nitric acid, as normally present in the atmosphere, than is generally imagined. His researches were made on the water collected in the rain-gauges of the Observatory of Paris in 1851 and 1852.

The following are his general conclusions:—

"1°. During one year, reckoning from July 1, 1851, to June 30, 1852, there fell at Paris a quantity of nitrogen in combination, equal to 20·04 lbs. avoirdupois to the English imperial acre; namely, 11·13 lbs. in the condition of nitric acid, and 8·91 lbs. in the condition of ammonia.*

"2°. The quantity of ammonia which fell during that period amounted to 12·29 lbs. to the acre.

"3°. The quantity of anhydrous nitric acid which fell during the same period amounted to 41·24 lbs. to the acre.

"4°. The quantity of ammonia diminished in the months during which the quantity of nitric acid increased.

"5°. The quantity of nitric acid increased whenever the weather became stormy.

"6°. During the months only of February, March, April, and June, the quantity of nitrogen in the form of nitric acid, was a little less than the quantity of nitrogen in the form of ammonia."†

These observations apply to rain-water collected in the neighbourhood of a great city, and do not admit of direct comparison with the purer rain-water of the open country; but they are very remarkable, not merely as shewing that rain brings down nitric acid as well as ammonia, but that, in certain places at least, it contains more nitric acid than ammonia. And although a given weight of ammonia contains three times the amount of nitrogen which the same weight

* There is some mistake in Barral's numbers, for the statements in the first paragraph do not agree with those in the second and third; as the numbers, however, for nitrogen are *calculated* from the *observed* quantities of nitric acid and ammonia, the figures representing these are assumed as the correct ones.

If so, the quantity of "nitrogen in combination" is equal to 20·81 lbs. per acre; that of the nitrogen in nitric acid is 10·69 lbs.; and that of the nitrogen in ammonia, 10·12 lbs. These, accordingly, are the numbers which should appear in the first paragraph of Barral's conclusions.

† *Comptes Rendus* pour 27 Septembre 1852, p. 431.

of nitric acid does; yet, as in the numbers I have quoted, the weight of acid exceeds that of alkali three and a-half times, it appears that, so far as we have quantitative observations on the matter to refer to, a larger amount of nitrogen is offered to plants in rain-water, in the form of nitric acid, than in that of ammonia.

I have separated the question of the occurrence of nitric acid in rain-water, from that of its development in the atmosphere by oxidation, and by electricity; because it is not certain that the whole of the nitrates found in rain-water have been produced by a process like that of nitrification, or by the action of lightning-discharges on the air. Since rain-water is found to contain common salt, lime, magnesia, and the like, which have been raised into the atmosphere from the earth, or from the bodies of water at its surface, we cannot refuse to credit that nitre may be elevated in the same way. I might go further, for attention has long been directed to the fact, that there is a marked loss during the evaporation of solutions of common nitre, in consequence of that salt, although not volatile in the dry state, undergoing volatilisation along with the vapour of water. This is a secondary point, but it is important, as shewing that, apart altogether from oxidation, and from thunder-storms, there is a source from which the atmosphere everywhere may receive compounds of nitric acid.

Fourthly, It has been known for more than a century, that many springs contain nitrates.

Fifthly, It is now universally admitted, that wherever nitrogenous vegetable or animal matter is exposed to the air along with alkaline bases, ammonia is developed, and then oxidised into nitric acid, which combines with the bases. Now, those conditions are extensively realised all over the globe, both in cultivated and uncultivated tracts of land; and in the warmer regions of the earth, where decomposition proceeds with greatest rapidity, the production of nitre in the soil is constant and immense. India alone furnishes Great Britain with all the nitre needed for her gunpowder.

Sixthly, The most marked nitrous districts of India are celebrated for their fertility, provided a due supply of water is furnished to them.

Seventhly, The alkaline nitrates dissolved in water, and not employed in too strong solutions, have been found greatly to quicken the growth of plants; and the nitrate of soda which, from its cheapness, is the most accessible, is daily coming into greater use among our farmers. In the current number of the Journal of the Royal Agricultural Society,* will be found the last of a series of papers on this subject, in which the virtues of nitrate of soda in increasing the amount of wheat yielded by a field manured with it, are placed by Mr Pusey above those of ammonia.

It has been asserted, indeed, that alkaline nitrates are serviceable to plants only by furnishing them with alkalies; but I know not by what arguments it is proposed to defend this opinion. It is at variance with the experience of farmers, who find nitrate of soda, as Mr Pusey reports,† a powerful fertiliser where common

* Vol. xiii., Part ii., p. 366.

† *Ibid.* p. 349.

salt is of no avail. But it is needless to enlarge upon this, for even if it were conceded that soda is the more important constituent of nitrate of soda, considered as a fertiliser, it is manifest that it must make a difference to a plant whether soda be supplied to it combined with carbonic, hydrochloric, sulphuric, or nitric acid; and that in the case of nitrate of soda, the plant must in some way dispose of the nitric acid before it can avail itself of the soda; so that the question must be answered, What becomes of the nitric acid which enters plants?

To this, one reply is offered in the quotation which I commenced by reading. The presence of nitrates in tobacco, sunflower, and certain other plants, is thought to shew that if they do not even possess the power of producing nitric acid, they at least cannot decompose it. But surely this is proving too much. For if the presence of undecomposed nitric acid in a plant shews that it cannot decompose that acid, then the presence of ammonia shews that it cannot decompose ammonia, and the presence of undecomposed sulphuric acid shews that it cannot decompose this acid; and for the same reason, as plants all contain undecomposed chlorides, carbonates, water, and carbonic acid, it should be held that they can decompose none of these. In short, it should be contended that a plant can decompose nothing, and that its existence is a chemical contradiction. If we refuse to draw this conclusion in the case of the other oxides and acids which are found in plants, we must extend our refusal to nitric acid, which is circumstanced exactly as the others are.

As for the opinion that plants may *produce* nitric acid, it is quite possible that they can, although for reasons to be presently mentioned it does not seem likely that they generally do; but it would be unwise to speak confidently on the matter. The only conclusion certainly deducible from the presence of nitrates in plants is, that at least nitric acid does not act injuriously on them.

Thus far, then, it has been I think satisfactorily shewn—

Firstly, That nitrates are largely offered to plants, both as they grow wild and as they are artificially cultivated.

Secondly, That plants do not refuse the nitrates thus offered them.

Thirdly, That the nitrates which enter plants do not, if properly diluted, do injury to any class of them; whilst,

Fourthly, They largely promote the growth of many of the most important among them.

It remains to inquire, Can plants decompose nitric acid, and avail themselves of its nitrogen? I can offer no direct or demonstrative proof that plants possess the power of effecting this decomposition. Direct proof is not to be had in the matter, but the following powerful considerations may be urged in support of the belief that plants can decompose nitric acid.

From the moment of the discovery that water is the oxide of hydrogen, chemists perceived that the great characteristic function of a living plant, considered as a piece of chemical apparatus, was to deprive oxides of their oxygen,

or to deoxidise them. The earliest teachers of this doctrine, CAVENDISH, WATT, MEUSNIER, and LAVOISIER, supposed this deoxidising power to be chiefly expended upon water. At a later period, when the fact that carbonic acid is an oxide of carbon was discovered, and PRIESTLEY's experiments on the conversion of fixed air into free oxygen, by the green leaves of plants in the presence of sunshine, were recalled, the deoxidising powers of a plant were supposed to be mainly expended on carbonic acid. At present, we should decline to say whether this acid or water was most the subject of deoxidation in plants; and we should add to those oxides, sulphuric acid, as constantly undergoing separation into its elements. To such a conclusion we are driven by the fact, that whilst unoxidised sulphur is found in many of the constituents of plants, sulphates are the only compounds of sulphur which are found entering them.

Whatever else, however, is doubtful, this is certain, and is acknowledged by chemists of every school, viz., that a plant is like a blast-furnace, which the sun kindles every day into full action; and that no oxide can pass through such an apparatus, without risking the loss of all its oxygen. With what consistency, then, can it be contended, that water, carbonic acid, and sulphuric acid, cannot pass through a plant in the presence of sunshine, without being deprived in whole or in part of their oxygen, but that the much more easily deoxidised nitric acid, in the same circumstances, will not suffer deoxidation? It might as well be affirmed that a blast-furnace may be competent to reduce the refractory oxide of iron, and yet be incompetent to reduce the easily reducible oxide of lead.

No one I think will deny, that out of a plant, sulphates are deprived of oxygen with much more difficulty than nitrates are; if, however, the deoxidising force at work within a plant can deprive sulphates of their oxygen, *à fortiori* it can deprive nitrates of their oxygen, and we must concede their deoxidability and deoxidation.

But further, the alkaline nitrates which are the medium of the introduction of nitric acid into plants, will certainly within them separate more or less completely into acid and alkali, and let the former become free. Those who contend that nitrate of soda profits plants only in so far as it contains soda, imply by this statement that the nitric acid is set free from the soda, and in *some* way disposed of. All chemists, moreover, will acknowledge that the large amount of fixed alkaline bases found present in every plant in union with organic acids, compels us, whatever theory we hold, to look upon these acids developed within the plant, as having taken the place of the inorganic or mineral acids which accompanied the bases into its structure. Nitric acid must therefore be often set free within vegetable organisms; and when set free, must more rapidly than any uncombined inorganic oxide which is present in plants, suffer instant deoxidation. This proposition, I think, needs no proof. Uncombined carbonic or sulphuric acid, cannot be deoxidised by any known artificial process, so as to separate its oxygen as free gas. Water can be made to yield free oxygen only by a powerful voltaic current,—by an intense

white heat, assisted by platina,—or by chlorine and its congeners, with their affinities for hydrogen exalted by sunshine; but nitric acid is the frailest of oxides. It not only parts with oxygen to the immense majority of metals, and of metallic and organic compounds, but the simple application of heat deoxidises it; and sunlight, which so greatly intensifies the inherent deoxidising power of a plant, can, without the co-operation of its complex organic apparatus, compel nitric acid to undergo deoxidation.

If, therefore, sunlight *alone* can deoxidise nitric acid, sunlight, co-operating with a powerful deoxidising apparatus, will not be less efficacious; and those chemists who declare that a plant can deoxidise water, carbonic acid, and sulphuric acid, but cannot deoxidise nitric acid, are uttering the paradox, that the more easy the decomposition of an oxide is, the more difficult does a plant find it to be to decompose it; so that if it be exceedingly susceptible of deoxidation, then the plant, whose greatest chemical power is a deoxidising one, cannot deoxidise it at all.

No one, I think, would articulately defend such a doctrine. The opposite conclusion is surely the just one, that if nitric acid be conveyed into plants, it will be reduced by loss of oxygen finally to the condition of nitrogen, and as such be as available for the production of azotised vegetable compounds as the nitrogen of ammonia.

Teachers of chemistry appear to be reluctant to admit two sources of nitrogen for plants, because it complicates their statements, and multiplies their formulæ; but the partial representations of truth, to which all teachers are compelled, however catholic in spirit, can never justify the expression of one-sided views, as the counterpart of the multiform unity of Nature. Those, moreover, who have been accustomed to trace back all azotised vegetable compounds to ammonia, need only postulate that nitric acid having been deoxidised into nitrogen, that element unites with hydrogen to form ammonia before any organic compound is developed; and thereafter they may carry out the ammonia theory as before. Such a conversion of nitric acid into ammonia is not hypothetical, for it can be readily effected by diluting the acid largely with water, and dissolving zinc in it.

It would more consist with the modesty of true science, to be less dogmatic than we generally are on the phenomena which occur within the inscrutable recesses of a living plant; and to admit the probability of its being able to employ as food various azotised, as well as other compounds. If, however, we are required to reduce to its simplest chemical expression the conclusion which our present science warrants regarding the inorganic origin of the nitrogen so essential to plants, we must not say that only ammonia, or only nitric acid, is its source, but that both are; or, in a word, that the chief mineral or inorganic representative and parent of the nitrogenous constituents of plants and animals is the Nitrate of Ammonia.

XLII.—*Some Observations on Fish, in relation to Diet.* By John Davy, M.D., F.R.S. Lond. & Edin., Inspector-General of Army Hospitals, &c.

(Read 18th April 1853.)

What are the nutritive qualities of fish, compared with other kinds of animal food? Do different species of fish differ materially in degree in nutritive power? Have fish, as food, any peculiar or special properties? These are questions, amongst many others, which may be asked, but which, in the present state of our knowledge, I apprehend it would be difficult to answer in a manner at all satisfactory.

On the present occasion, I shall attempt little more than an opening of the inquiry, and that directed to a few points,—chiefly those alluded to in the foregoing queries.

1. *Of the Nutritive Power of Fish.*

The proposition probably will be admitted, that the nutritive power of all the ordinary articles of animal food, at least of those composed principally of muscular fibre, or of muscle and fat, to whatever class belonging, is approximately denoted by their several specific gravities, and by the amount of solid matter which each contains, as determined by thorough drying, or the expulsion of the aqueous part, at a temperature such as that of boiling water, not sufficiently high to effect any well-marked chemical change.

In the trials I have made, founded on this proposition, the specific gravity has been ascertained in the ordinary hydrostatical way;—the portions subjected to trial, in the instance of fish, have been taken from the thicker part of the back, freed from skin and bone, composed chiefly of muscle. And the same or similar portions have been used for the purpose of determining their solid contents, dried in platina or glass capsules of known weight, and exposed to the process of drying till they ceased to diminish in weight.

The trials on the other articles of diet, made for the sake of comparison, both as regards specific gravity (excepting the liquids), and the abstraction of the hygroscopic water, or water capable of being dissipated by the degree of temperature mentioned, have been conducted in a similar manner.

The balance used was one of great delicacy, at home, or a small portable one, when from home, of less delicacy, yet turning readily with one-tenth of a grain.

The results obtained are given in the following tables. In the first, on some different species of fish; in the second, on some other articles of animal food.

I have thought it right, whenever it was in my power, to notice not only the time when the fish were taken, but also the place where they were procured,—not

always so precise as I could wish,—as both season and locality may have an influence on their quality individually. When the place mentioned is inland, it must be understood, that, in the instance of sea-fish, they were from the nearest seaport.

TABLE I.

Species of Fish.	Specific Gravity.	Solid Matter, per cent.	Place where got, and Time.
Turbot, *Rhombus maximus*,	1062	20·3	March. Liverpool.
Brill, *R. vulgaris*, . .	1061	20·2	October. Penzance.
Haddock, *Gadus æglefinus*,	1056	20·2	August. Ambleside.
Hake, *G. merlucius*, .	1054	17·4	October. Penzance.
Pollack, *G. pollachius*, .	1060	19·3	October. Penzance.
Whiting, *Merlangus vulgaris*,	1062	21·5	March. Chester.
Common Cod, *Morrhua vulgaris*,	1059	19·2	April. Ambleside.
Red Gurnard, *Trigla cuculus*,	1069	23·6	October. Penzance,
Dory, *Zeus faber*, . .	1070	22·9	October. Penzance.
Mackerel, *Scomber scombrus*,	1043	37·9	October. Penzance.
Sole, *Solea vulgaris*, .	1065	23·0	February. Ambleside.
Do. *do.*, . .	1064	21·1	February. Ambleside.
Thornback, *Raia clavata*,	1061	22·2	October. Penzance.
Salmon, *Salmo salar*, .	1071	29·4	March. River Boyne, Ireland. Fresh run from the sea.
Sea-Trout, *S. eriox*, .	...	41·2	June. Ambleside.
Charr, *S. umbla*, . .	1056	22·2	November. Windermere.
Trout, *S. fario*, . .	1053	22·5	March. Lough Corrib, Ireland, Weight about ½ lb., in good condition.
Do. *do.* . .	1050	18·7	October. River Brathay. A small fish of about 2 oz.
Smelt, *S. eperlanus*, .	1060	19·3	March. Liverpool.
Eel, *Anguilla latirostris*,	1034	33·6	June. Ambleside.

TABLE II.

Kinds of Food.	Specific Gravity.	Solid Matter, per cent.	Place and Time.
Beef, sirloin, . . .	1078	26·9	March. Ambleside.
Veal, loin, . . .	1076	27·2	November. Ambleside.
Mutton, leg, . . .	1069	26·5	November. Ambleside.
Pork, loin, . . .	1080	30·5	January. Ambleside.
Pemican, composed of beef and suet, . . .	...	86·25	Victualling-yard, Portsmouth.
Common fowl, breast, .	1075	27·2	November. Ambleside.
Grey Plover, breast, .	1072	30·1	November. Ambleside.
Cow's milk, new, before the cream had separated, .	1031	11·2	November.
White of hen's egg, .	1044	13·9	
Yolk of the same, .	1032	45·1	

These results I would wish to have considered merely as I have proposed in introducing them, viz., as approximate ones. Some of them may not be perfectly correct, owing to circumstances of a vitiating kind, especially the time of keeping. Thus, in the case of the whiting, which was brought from Chester, its specific gravity, and its proportion of solid matter may be given a little too high, owing to some loss of moisture before the trials on it were made. Casting the eye over the first table, it will be seen that the range of nutritive power, as denoted by the specific gravity, and the proportion of solid matter, is pretty equable, except in a very few instances, and chiefly those of the salmon and mackerel. The one exhibiting a high specific gravity, with a large proportion of solid matter; the other, a low specific gravity, with a still larger proportion of matter, viz., muscle and oil, and, in consequence of the latter, the inferior specific gravity. A portion of the mackerel, I may remark, merely by drying and pressure between folds of blotting paper, lost 15·52 per cent. of oil. Oil also abounded in the sea-trout and eel, and hence the large amount of residue they afforded.

Comparing *seriatim* the first table with the second, the degree of difference of nutritive power of those articles standing highest in each, appears to be inconsiderable, and not great in the majority of the others, exclusive of the liquids,—hardly in accordance with popular and long received notions.

2. *Of the Peculiar Qualities of Fish, as Articles of Diet.*

I am not prepared to enter into any minute detail on this important subject, from want of sufficient data.

That fish generally are easy of digestion, excepting such as have oil interfused in their muscular tissue, appears to be commonly admitted, as the result of experience,—a result that agrees well with the greater degree of softness of their muscular fibre, comparing it with that either of birds or of the mammalia, such as are used for food.

A more interesting consideration is, whether fish, as a diet, is more conducive to health than the flesh of the animals just mentioned, and especially to the prevention of scrofulous and tubercular disease.

From such information as I have been able to collect, I am disposed to think that they are. It is well known that fishermen and their families, living principally on fish, are commonly healthy, and may I not say above the average; and I think it is pretty certain, that they are less subject to the diseases referred to than any other class, without exception. At Plymouth, at the Public Dispensary, a good opportunity is afforded of arriving at some positive conclusion—some exact knowledge of the comparative prevalency of these diseases in the several classes of the community. The able physician of that institution, my friend, Dr Cookworthy, at my request, has had the goodness to consult its records, and from a communication with which he has favoured me, it appears that of 654 cases of

"confirmed phthisis and of hæmoptysis, the probable result of tuberculosis," entered in the register of the Dispensary, 234 males, 376 females, whose ages and occupations are given individually, the small number of four only were of fishermen's families,—one male and three females,—which is in the ratio of one to 163·2; and of watermen "who fish with hook and line, when other work is scarce, generally very poor, and of habits generally by no means temperate or regular," the number, including their families, did not exceed eleven, of whom ten were males, one a female, which is in the ratio of one to 58·8. The entries from which the 654 cases are extracted, Dr Cookworthy states, exceed 20,000. He assures me, that had he taken scrofula in all its forms, the result would, he believes, have been more conclusive.

Such a degree of exemption as this return indicates in the instances of fishermen and boatmen, is certainly very remarkable, and deserving of attention, especially considering the prevalency of tubercular consumption, not only in the working classes generally throughout the United Kingdom, but also amongst the regular troops, whether serving at home or abroad, and having an allowance of meat daily, but rarely tasting fish.*

If the exemption be mainly owing to diet, and that a fish diet, it may be presumed that there enters into the composition of fish, some element not common to other kinds of food, whether animal or vegetable. This I believe is the case, and that the peculiar element is iodine.

I may briefly mention, that in every instance in which I have sought for this substance in sea-fish, I have found distinct traces of it, and also, though not so strongly marked, in the migratory fish, but not in fresh-water fish. The trials I have hitherto made have been limited to the following, viz., the Red Gurnet, Mackerel, Haddock, Common Cod, Whiting, Sole, Ling, Herring, Pilchard, Salmon, Sea-Trout, Smelt, and Trout. In each instance, from about a quarter a pound to a pound of fish was dried and charred, lixiviated, and reduced to ashes, which were again washed. From the sea-fish, the washings of the charcoal afforded a good deal of saline matter on evaporation; the washings of the ash less. The saline matter from both consisted principally of common salt, had a pretty strong alkaline reaction, and with starch and aqua regia, afforded, by the blue hue produced, clear proof of the presence of iodine. In the instance of the fresh-run Salmon, Sea-Trout, and Smelt, a slight trace of iodine was thus detected; in the spent Salmon descending to the sea, only a just perceptible trace of it was observable, and not a trace of it either in the Parr or in the Trout.

That iodine should enter into the composition of sea-fish, is no more perhaps than might be expected, considering that it forms a part of so many of the inhabi-

* In 1205 fatal cases, not selected, in which the lungs were examined at the General Hospital, Fort Pitt, Chatham, tubercles were found to exist in 734 (61·7 per cent.) See the author's "Notes on the Ionian Islands and Malta," vol. ii., p. 312, for details.

tants of the sea on which fish feed;—to mention only what I have ascertained myself,—in the common Shrimp I have detected it in an unmistakeable manner, and also in the Lobster and Crab; and likewise in the common Cockle, Mussel, and Oyster.

The medicinal effects of cod-liver oil, in mitigating if not in curing pulmonary consumption, appear to be well established. And as this oil contains iodine, the analogy seems to strengthen the inference that sea-fish generally may be alike beneficial.

Should further inquiry confirm this conclusion, the practical application of it is obvious; and fortunately, should fish ever come into greater request as articles of food, the facility with which they may be preserved, even without salt, by thorough drying, would be much in favour of their use. I lay stress on thorough drying, as that seems essential,—for preservation, I believe even hygroscopic water should be excluded. Even in the instance of those articles of food which can be preserved in their ordinary dry state, the expulsion of this water would be advantageous under certain circumstances, were it merely on account of diminution of weight. Thus, referring to the second table, it will be seen that the Pemican, carefully prepared in the Portsmouth Victualling Office, lost by thorough drying 13·75 per cent., so much being the water it contained in a hygroscopic state,—a lightening of weight that, to the Arctic land explorer, could not fail to be welcome and useful.

The inference regarding the salutary effects of fish depending on the presence of iodine, in the prevention of tubercular disease, might be extended to some other diseases, especially to that formidable malady goître, the mitigation or cure of which has, in so many instances, been effected by iodine; and which, so far as I am aware, is entirely unknown amongst the inhabitants of seaports and sea-coasts, who, from their situation, cannot fail to make more or less use of fish.

Amongst the many questions that many be asked in addition to those I have proposed, I shall notice one more only, and that in conclusion. It is, whether the different parts of the same fish are likely to be equally beneficial in the manner inferred,—the beneficial effect, it is presumed, depending on the presence of iodine. From the few experiments I have yet made, I am led to infer, reasoning as before, that the effects of different parts will not be the same, inasmuch as their inorganic elements are not the same. I may instance liver, muscle, and roe or milt. In the ash of the liver and muscle of sea-fish, I have always found a large proportion of saline matter, common salt abounding, with a minute portion of iodine,—rather more in the liver than in the muscle,—and free alkali, or alkali in a state to occasion an alkaline reaction, as denoted by test paper; whilst in their roe and milt I have detected very little saline matter, no trace of iodine, or of free alkali; on

the contrary, a free acid, the phosphoric, analogous to what occurs in the ash of the yolk of the domestic fowl,—and in consequence of which, the complete incineration of the roe of the fish and its milt, like that of the yolk of the egg, is very difficult.

The same conclusion, on the same ground, viz., the absence of iodine, is applicable to fresh-water fish,—a conclusion that can hardly be tested by experience, nor is it of practical importance, since fish of this kind enters so sparingly into the ordinary diet of the people.

Lesketh How, Ambleside,
April 14, 1853.

P.S.—I have mentioned briefly the test employed to detect iodine. To prevent obscurity, may I be permitted to add a few particulars relative to the mode of proceeding? On a portion of starch in fine powder, that is, in its granular state, aqua regia is poured, or about equal parts of nitric and muriatic acid, in a platina capsule, and then well mixed, using a glass rod. The salt to be tested, either in solution or solid, is then added. The blue tint due to the presence of iodine is immediately produced, if any of this substance, or a sufficiency of it to take effect, be present. The delicacy of this test is, I believe, well known. I have by means of it detected iodine, when one-tenth of a grain of the iodide of potassium was dissolved in 16775 grains of water. Relative to this method, I may further remark, that by well mixing the acid and starch, not only is the starch reduced to a gelatinous state favourable for being acted on by the iodine as liberated, by the action of the chlorine, but also that the excess of chlorine is, to a great extent, got rid of. The platina capsule has appeared preferable to one of glass, as shewing the effect of colour by reflected light more readily and distinctly; and also, I am disposed to think, from some peculiar influence which the metal exercises, favouring the combination of the starch and iodine, similar, it may be, to that of spongy platinum, in effecting the union of oxygen and hydrogen.

In seeking for iodine in animal substances by incineration, it may be well to keep in mind, that, experimentally considered, the liability to error lies in underrating, rather than in overrating the result by the methods employed, and that mainly in consequence of more or less of loss of iodine being sustained in the process of combustion, incineration, and evaporation used. To illustrate this by a simple experiment, I may mention that a portion of water, equivalent to about 1525 grains, in which were dissolved 10 grains of common salt, and ·09 grain of iodide of potassium, was quickly evaporated to dryness by boiling. Previously, the iodine could be detected in the mixture by the test I have used: but not afterwards, when the

residual salt was dissolved in the same quantity of water; proving how there had been a loss of the iodine in the operation of boiling; a loss chemists are familiar with, of substances in themselves not volatile, carried off suspended in aqueous vapour.

In stating the comparative exemption of fishermen and their families from pulmonary consumption, as indicated by the Plymouth Dispensary return, I have not given the total number of this class of persons. This deficiency I am now able to supply. From information which I have received, for which I am indebted to the Registrar-General, it would appear, that of the total male population of Plymouth (24,605), the number of fishermen is 726, exclusive of 37 pilots. This large proportional number renders the fact of their exemption the more remarkable, and especially comparing them with a class of the population, altogether different in their habits, and, it may be presumed in their diet, using fish only occasionally when abundant and cheap,—these are the cordwainers or shoemakers, whose number altogether (males) is 608. Now, on consulting the Dispensary return, I find, that the total number of this class that have died of the disease under consideration, has been 37, viz., 19 males and 18 females!

Reflecting on the fact, that iodine has been detected in all the trials I have hitherto made on sea-fish, it seemed probable that guano, considering its origin, would not be destitute of this substance; and the result of experiments has been confirmatory; using the test-method noticed above, a distinct indication of its presence was obtained, both in the instance of the Peruvian and African guano, the only two I have yet tried.

Lesketh How, *June* 1, 1853.

BRITISH
MUSEUM
NATURAL HISTORY

NATURAL HISTORY

Fig. 1

Fig. 2

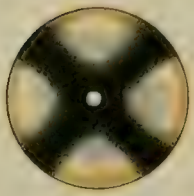


Fig. 3.

Fig. 4

Fig. 7

Fig. 8

Fig. 9

Fig. 10

Fig. 6.

Fig. 13

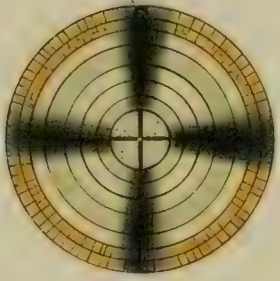


Fig. 11.

Fig. 5.

Fig. 14.

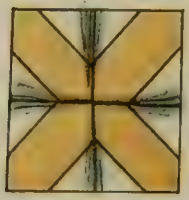


Fig. 12

A.R. Lewis Chromo Litho.

BRITISH MUSEUM
NATURAL HISTORY

Fig 15

Fig. 16

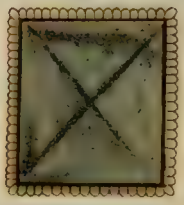


Fig. 17.

Fig. 18

Fig 20.

Fig 19

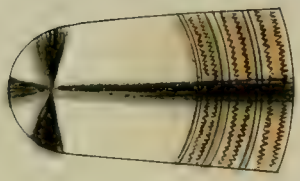


Fig 21

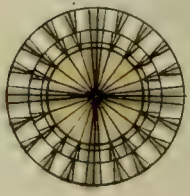


Fig. 22.

Fig 23.

Fig. 24.

Fig. 25.

W.H. Lizars Chromo Litho.

XLIII.—*On Circular Crystals.* By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., V.P.R.S. Edin., and Associate of the Institute of France. (With Two Plates.)

(Read 21st March 1853.)

In 1836, Mr FOX TALBOT communicated to the Royal Society a paper "On the Optical Phenomena of certain Crystals" which he obtained by dissolving a crystal of Borax in a drop of somewhat diluted Phosphoric acid. When the acid and the salt are in proper proportions, "the field of view of the microscope is seen covered with minute circular spots, each of which is like a tuft of silk radiating from a centre, and is composed of a close assemblage of delicate acicular crystals forming a star." Among these crystals are seen interspersed "a number of circular transparent bodies, which are tufts or stars of acicular crystals, in such close assemblage as to be in optical contact with each other, and to produce the appearance of a single individual."

When the field of the polarising microscope is dark, "the little circles become luminous, and we see upon each of them a well-defined and dark cross, dividing the crystal into four equal parts." With a high power, Mr TALBOT observed upon each circle one or more coloured rings arranged concentrically; the innermost, which was deeply coloured or black, enclosing a central space of white light, traversed by the black cross already mentioned. "The general appearance," he adds, "resembles the Fig. 98* in BREWSTER's Treatise on Optics, which is a representation of the rings in uniaxal crystals."

About twenty years† before the publication of Mr TALBOT's paper, I had produced minute circular crystals in *Oil of Mace*,—in a mixture of *Oil of Mace* and *Rosin*, and also in *Tallow*, when these substances were melted between two plates of glass, and cooled under pressure. The crystals thus produced were not themselves visible in the microscope, but, in polarised light, they exhibited their existence and their structure, in the formation of four luminous sectors traversed by a black cross.

When we look through a plate of Oil of Mace properly prepared at a bright and small disc of light, the light is generally surrounded with a single halo, though sometimes with two. When the disc of light is polarised and subsequently analysed, the halo is divided into four luminous sectors, separated by a rectangular black cross, which, from the divergency of its branches, had the appearance of four dark sectors. The arms of the black cross were always parallel

* Fig. 120 in the New Edition, just published.

† *Phil. Trans.*, 1815, p. 49, 38; and 1816, p. 97.

and perpendicular to the plane of primitive polarisation, and the disc of bright light disappeared at their point of intersection. In the opposite position of the analyser, the luminous disc appeared at the point of intersection, and the two luminous sectors that were horizontal were brighter than the two vertical ones. These phenomena are shewn in Figs. 1 and 2, Plate XVI.

In some cases, the circular space enclosing the sectors was very small, and in others large, and frequently when two halos were produced, there were two sets of luminous sectors, separated by an interval equal to that between the halos.

It is very obvious that the halos were produced by the crystals of the *Oil of Mace*, the smaller halo by the larger crystals, and the larger halo by the smaller crystals existing among the larger ones. In order to explain the luminous sectors, I inferred that each halo was composed of two, the one lying above the other, and having every alternate sector polarised in opposite planes; or, in other words, that these two halos were the two images formed by the double refraction of the elementary crystals, and were oppositely polarised, as all such images are. But though this inference was correct, as I afterwards proved, yet I could not see with the microscope the actual form of the circular crystals by which the double refraction and polarisation were produced.

After the publication of Mr Talbot's paper, I repeated the experiment with oil of mace, and having adopted different methods of cooling it under pressure, I soon discovered with the microscope, and by the aid of polarising films when the microscope could not alone detect the structure, that the phenomena which I have described were produced by circular crystals varying from invisibility to the 200th or 300th of an inch in diameter, and exhibiting, when of this size, distinct and beautiful sectors in polarised light.

Having thus discovered a method* of distinguishing true quaquaversus polarisation, or that which is produced by invisible crystalline particles with their axes lying in all directions, from that *apparent* quaquaversus polarisation which is produced by the same class of particles combined in circular crystals, I was anxious to prosecute the subject of circular crystallisation, by examining a great number of doubly-refracting substances.

With this view I received from Mr Talbot the preparation of Borax and Phosphoric acid which he had found to give the best circular crystals, and from Dr Dowler of Richmond a quantity of the *Lithoxanthate of Ammonia*, which yields circular crystals with more certainty and less trouble than the preparation of

* This method consists in placing a film of selenite (sulphate of lime) between the polariser and the substance to be examined. If the polarising structure is produced by circular crystals, it will appear covered with spots, or minute sectors, of two different colours, the one being a tint a little lower, and the other a tint a little higher, than that of the selenite. The higher tint is the sum of the tints of the two substances, and the lower their difference, the tint of the selenite being increased by that of two of the sectors, and diminished by that of the other two.

Borax. I was thus enabled to study the phenomena which they exhibit in their formation, their structure, and their subsequent decomposition.

In the expectation of obtaining a greater variety of structure, and discovering new phenomena, I submitted to examination about 300 doubly-refracting substances, and among these I discovered nearly *seventy* that give circular crystals, about *thirty* of which are *positive*, like *Zircon*, and *forty negative*, like *Calcareous spar*.

In the course of these experiments, which have occupied much of my time, I have observed many new and splendid phenomena, which lay open an extensive field of research, and promise to throw much light on those abnormal crystallisations which take place under the constraining influences of heat and pressure, and also on their subsequent decomposition and return to their molecular state.

In submitting to the Society an account of these experiments, I shall begin with the *Lithoxanthate of Ammonia*, as it exhibits a greater variety of phenomena, and is more easily converted into circular crystals than any other salts with which I am acquainted.

1. *Lithoxanthate of Ammonia.*—This substance, under ordinary circumstances, crystallises in minute prisms, often in beautiful dendritic forms, and in spherical groups of crystals in which the prisms are not in optical contact, and yet sufficiently united to exhibit the black cross at the centre of the sphere.

When the circular crystals are produced, and are transparent, they have very different aspects in different specimens. In their simplest form, they are united in a continuous film, each circular crystal exhibiting four luminous sectors separated by a black cross, the arms of which are, of course, always parallel and perpendicular to the plane of primitive polarisation. The light polarised by the sector is the *blue* of the first order, often rising to the *white*, and sometimes to the *yellow*, of the same order.

When we look at a small and bright luminous disc through a film of such crystals, we see a halo, and sometimes two halos, the diameter of the halo diminishing as the circular crystals increase in size. When the film is placed in the polariscope, the halo is converted into four luminous sectors, and into eight when it is double, exactly the same as those produced by oil of mace, and shewn in Figs. 1 and 2.

When the circular crystals are separate, their structure is more complex, and their appearance more beautiful. In one of these, shewn in Fig. 3, I have observed, but only once, the *three* first orders of colours of thin plates, exactly like the uniaxal system of rings in regular crystals; and consequently, the thickness of the spicular crystals which composed them must have increased from the centre outwards, according to the law in Newton's Table of Periodical Colours. This result was so remarkable, that I determined the character of the three

orders of tints, by compensating them with the corresponding tints of plates of selenite.

In other discs the rings 2 and 3 have each the same colour throughout,—the one generally *red* and the other *green*, and having no relation, either to the central tint at 1, or to one another. In some cases the order of colours is completely inverted, as in Fig. 4, where the central tints are a *blue* of the second order, gradually passing through *red* and *yellow* to a brilliant *white* of the first order. In other crystals I have found the central tints *red*, *green*, and *yellow*, of high orders; but in these cases the discs are not regularly formed, and the elementary crystals not wholly in optical contact.

The most perfect circular crystals are those in which the central tints are the *blue* and *white* of the *first* order. This arises from the extreme minuteness of the crystals, which thus form a more uniform disc, and cause the *black cross* to have a degree of sharpness, which it requires a considerable magnifying power to exhibit. In such crystals, the central portion is surrounded with a black and narrow ring, beyond which there is another annulus of sectors, sometimes *white* like the inner ones. This is again terminated by a black circle, beyond which is a third series of sectors, sometimes *white* and sometimes a *blue* of the first order. This structure is shewn in Fig. 5, where the black cross starts into different breadths, in passing from one set of sectors to the others,—an effect which is produced by an inferior degree of optical contact in the elementary crystals of the outer sectors.

An interesting structure is shewn in Fig. 6, where all the tints are *white*, the central ones terminating in a dark circle, beyond which are four large sectors, whose tint is the *bluish white* of the first order, lower than the central tint. Each of these sectors, however, is divided into four portions by very faint circular lines, which scarcely depolarise the incident light, the tint being there a minimum, and increasing to the middle point between them.

In Figs. 7 and 8, we have represented structures consisting of crystals, shooting out, as it were, from the centre, and all of a *golden-yellow* colour. In Fig. 7, the black cross is seen, but in Fig. 8 there is such imperfection of contact between all the radial crystals, that the darkness of those under the black cross is scarcely visible.

In discs like Fig. 7, a very singular effect is sometimes produced, as shewn in Fig. 9, where the black cross is so divergent and wide, that the golden-coloured crystals half-way between its branches, have the appearance of a yellow rectangular cross.

Under favourable circumstances, the discs assume a very interesting and complex appearance, as shewn in Fig. 10. Beyond the central golden-yellow radiations is a broad annulus of *pale blue* of the first order, divided by a faint, dark, and narrow band, scarcely luminous. This annulus is surrounded by a sharp and broad line, perfectly black, which is succeeded by a similar line separated from

the other by a faint line of light, which, in some crystals, reaches the *yellow* of the first order. Beyond this is another annulus of pale blue light, divided, like the first, by a faint line. In some discs this annulus is divided into three, by two faint bands. Each sector of this annulus is subdivided by dark radial lines, into four or five spaces, and, sometimes, beyond this there is another annulus similarly divided, the tint of both being a *white* of the first order.

The interesting fact in this description, and which will afterwards occupy our attention, is, that the two sharp black circular lines, or spaces, are *wholly devoid of matter*, and that the interior part of the disc is separated by them from the exterior part.

Among the almost infinite variety of crystallisations which this substance presents to us, I shall describe only another which, though we shall afterwards find it fully developed in other substances, occurs only in circular sectors of 30° or 45°. It is represented in Fig. 11, in its complete state, and consists of a series of concentric circles, composed of crystalline patches, which generally polarise tints not higher than the *yellow* of the first order. Each concentric circle appears at first to be separated from its neighbour, and each crystalline patch from those adjacent to it; but though this is in some crystallisations the case, yet in general, we can observe between the patches, in all directions, crystalline matter so exceedingly attenuated, that its existence is not made visible by its action on polarised light.

2. *Salicine.*—In this substance, whether dissolved in water or in alcohol, I have found the most splendid circular crystallisations. They are generally very large, and their character is *negative*, like the rings in calcareous spar. When the crystals are small, and require a considerable power to be seen, their tint is the palest *blue* of the first order, but when their diameter is between the one-fifth and the one-thirtieth of an inch, and their tints those of the *first* and *second* orders, they form, in the estimation of all who have seen them, one of the finest objects for the polarising microscope.

One of the smaller crystals is shewn in Fig. 12, where the tint of the four sectors is *bluish-white*, while that of the circular rim is absolutely black, arising from the great thinness of the crystals which compose it. That they are transparent crystals and not opaque matter is proved in this, and in all similar cases, by turning round the analyser when the light freely permeates the rim, and has a slightly yellow tinge, being complementary to what Newton calls, in his Table of Periodical Colours, the *Beginning of Black*.

A larger disc of Salicine is shewn in Fig. 13, where there is a sharp black cross in the centre, surrounded with five or six narrow and concentric *black* rings, which become *white* by turning the analyser; or we shall in future express it, in the white field. Beyond these central sectors, the black cross is wide and divergent.

The whole of this annulus, which forms the greater part of the disc, is composed of crystals radiating from the centre, and of inequal thickness in their breadth, so that we have the luminous sectors not of one colour, as in the Lithoxanthate of Ammonia, but of various tints from *white* of the first to *blue* of the second order. The radiating crystals are sometimes sectors of 10° or 15°, of uniform thickness, and giving the same colour; and hence, the black cross is composed of sectors of different degrees of blackness as they are brought into the plane of primitive polarisation. Beyond this annulus, the disc terminates in a rim, like that of a carriage-wheel, composed of two or more concentric circles, between which the crystals are disposed in radial lines, sometimes not in optical contact, but exhibiting the same colours as those in the larger annulus. In discs of a considerable size, there are seen exceedingly minute and dark circles, about ten or twelve in number, which I have found to be cracks or lines of cleavage, and which are accompanied with short lines of cleavage, passing radially from the one to the other.

In these discs, there is another peculiarity which deserves to be noticed. In the coloured sectors, there are often circular spots and rings, in which the tint descends to *zero*, as if a drop of some solvent had fallen upon the crystal: and there are spots of an opposite kind, where the tint rises from that of the sector to higher tints, an effect probably produced by a particle of the crystal forming around itself, while dissolving, a thicker film, becoming thinner as it recedes from the particle.

In some of the circular discs of Salicine, I have found the outer rim as wide as the interior portion, and in this case it polarises a *bluish-white* of the first order; but, what is peculiarly worthy of notice, this rim is subdivided by faint concentric rings of different degrees of darkness, into, sometimes, twelve or fifteen annuli of different degrees of brightness. This seldom takes place in the interior portion of the disc, but when it does occur, and the tints are brilliant, the subdivison of the annulus into a number of concentric circles of different colours is singularly beautiful.

3. *Asparagine.*—The circularly polarising discs which this substance displays, resemble very much those of *Salicine*. They are more varied in their structure, and more beautiful in their tints. The rims of the discs are more highly coloured, and more uniform in their texture; and the concentric tints, whether they are all of different degrees of whiteness, or of higher orders of colours, are so perfectly regular, and so sharply defined, that the observer stands before them in mute admiration, and feels himself unable either to describe or to draw them. There are two peculiarities, however, which deserve to be noticed; the one, the existence of discs in which there is no circularly polarising structure; and the other, of discs exactly resembling, in the succession of black and white narrow rings, the systems

of rings seen round the star Capella, with annular apertures, and drawn by Sir JOHN HERSCHEL.*

4. *Manna.*—This substance gives fine circular crystals, which are negative, whether obtained from fusion or an aqueous solution. The crystals obtained by melting the Manna are the most perfect and beautiful. The intersection of the arms of the black cross is so sharp that it sometimes requires a considerable power to develope it, and the four minute sectors around it. Beyond this the crystals radiate uninterruptedly till they are stopped by meeting with other crystals, and the whole of them are joined together in a hexagonal mosaic pavement. The colours are very bright, varying from the *white* of the first to the *blue* and *green* of the second order, and there is a uniformity in the tints, and consequently in the shading of the black cross, which indicates great equality in the elementary prisms, and in the forces which keep them in optical contact. The discs are seldom found separate, and they have no rims, no annuli, and no concentric cracks.

5. *Disulphate of Mercury.*—This salt, dissolved in nitric acid, gives no circular crystals by rapid cooling; but, when the solution is cooled slowly, it yields positive circular crystallisations of a square form, as shewn in Fig. 14, which undergo interesting variations. The rectangular cross is sometimes wanting, and is, as it were, replaced by black lines, which meet at the centre. These lines are sometimes black in the white field, and are then junction lines where the optical contact is imperfect. The greater number of the crystals in which these lines are more or less perfectly seen are rounded at the angles. Sometimes they are nearly circular, and the tint which they polarise is very little above the *beginning* of black of NEWTON's Table.

When the crystals are thicker, they exhibit a singular variety of forms, of which I have given a specimen in Figs. 15, 16, 17, and 18, the relation of which to Fig. 14, will be easily recognised. The crystals shewn in Figs. 16 and 18 were obtained from a weak solution of the salt, and are very interesting. In the dark field of the microscope, we see only the brilliant golden-yellow border, and it requires a strong light and a very high power to discover, in the black interior of the square, minute specks of light equally diffused over its surface. By a slight turn of the analyser, we perceive the slightly darker diagonal cross shewn in Fig. 16. These squares are often wholly and uniformly filled up with crystals of the same tint as their outline; and occasionally only part of the square is thus occupied. The small and often shapeless crystals (occasionally oval and pear-shaped), which form the outline of the square in Fig. 16, and of the cross in Fig.

* *Treatise on Light*, § 770, Figs. 155, 156, 157, Plate IX.

18, have placed themselves in these positions after the interior crystal has been formed; that is, they are not increments deposited by the solution, but have been formed at a distance from the crystal, and carried to their new position. This is proved by the fact that sometimes a mass of them surround several of the square crystals, while individual ones take their place at random upon the face of the square. When the crystals are deposited from a strong solution, the square ones become almost opaque, and the irregular ones highly coloured, and of exceedingly various shapes. I have not been able to obtain any square crystals of the disulphate of mercury from its solution in muriatic acid.*

6. *Parmeline.*—This substance, dissolved in water, has a tendency to give circular crystals. In alcohol it gives very fine ones, producing, when small, beautiful halos like oil of mace, with blue light in their centre.

7. *Asparagine and Salicine mixed.*—After standing several months, this mixed salt produced small circular crystals, apparently of asparagine. These crystals gave brilliant halos of red and green light, of such a diameter that the individuals were only $\frac{1}{7500}$th of an inch in diameter. Among these small crystals were placed large circular discs, with curved sectors and black crosses, which gave them the appearance of the corolla of a flower with party-coloured petals.

8. *Palmic Acid.*—This substance, when melted by heat, gives very fine negative circular crystals like those in the hexagonal mosaic of manna. The insulated discs have a rim sometimes divided by broad black bands, where the substance was too thin to polarise the light. When the rim is broad and single, it is composed of narrow luminous sectors, radiating from points in the circumference of the disc. The rims are sometimes of a different colour from the principal sectors, and the latter are often subdivided by numbers of black and equidistant concentric circles.

9. *Nitrate of Uranium.*—This salt gives fine negative circular crystals in water, alcohol, and ether. The crystals formed in the alcoholic solution deliquesced in an instant, forming hemispherical bells, which polarised the light by oblique refraction, giving four luminous sectors, and a black cross very wide at the centre, like the sectors and cross produced by the hemispherical cups of de-

* In making these observations, and on many other occasions, I have felt the great inconvenience of the present, and in general, perhaps the best, arrangement of the compound microscope. High powers being always obtained by object-glasses of short focal length, it is almost impossible, in transparent structures, to develope them, when they consist of lines or parts of different thickness. Vision is destroyed by the refractions and diffractions of the intromitted light. The only remedy for this is to use $\frac{1}{2}$-inch, or even 1 or 2-inch object-glasses, and obtain the power that is required at the eye-piece, by means of grooved and other lenses of diamond, garnet, &c.

composed glass. After the deliquescence of the crystals, I attempted to make another crop, but having failed, I set the piece of glass aside. In the course of half-an-hour, however, I found it covered with a fine and splendidly-coloured set of circular crystals, which dissolved wholly when placed in castor-oil with the view of preserving them. The light polarised by the bells above mentioned, formed a double ring, *red* on one side, and *green* on the other, with a black space between.

Upon examining the solution in castor-oil, after having stood upwards of four years, I find that circular crystals of three different kinds have been formed, some small and very perfect, with four sectors and no rim; others with broad rims, with quaquaversus polarisation; and a third set in which the structure has been entirely decomposed, and the circular form of the disc preserved.

10. *Palmine.*—This substance melts like tallow into a uniformly luminous film, apparently with quaquaversus polarisation; but upon examining it with a high power in the polarising microscope, it exhibits millions of circular crystals, each bearing its little black cross. These crystals are so minute as to produce splendid halos, which, in the polariscope, give four luminous sectors exactly like those in oil of mace.

11. *Chromic Acid.*—The circular crystals of this substance, dissolved in water, are of a very peculiar kind. They are negative, and are very imperfectly represented in Fig. 11, where the circular disc is composed of a great number of concentric circles, whose tint is the *blue* of the first order, rising, in some cases, to the *yellow* of the same order. These circles may be described as rippled lines consisting of minute crystals, separated by others still more minute, and incapable of polarising the light. The system of concentric rings is traversed by the usual black cross. This salt gives another kind of crystals, in which are separate concentric rings without the black cross, and consequently with quaquaversus polarisation.

12. *Berberine.*—This salt gives very fine circular crystals which are negative, and form beautiful halos like those in oil of mace. The ordinary crystals often form a number of crystalline rings in contact, each of which contains circular crystals of different sizes, and occasionally prismatic crystals along with them.

13. *Sulphate of Cadmium.*—The sulphuret of cadmium, dissolved in nitric acid, is converted into sulphate, which gives beautiful negative circular crystals, varying from the 800th of an inch to the 3000th. After the sulphuret is melted, and the acid driven off, no crystallisation is seen, but in an hour or two a deliquescence takes place, and the circular crystals gradually appear. There are many of them so small and thin, that they have no action on polarised light.

The solutions of this substance in muriatic and acetic acids gave no circular crystals.

14. *Sulphate of Ammonia and Magnesia.*—When an aqueous solution of this salt is brought into a viscous state by heat, and slowly cooled, very beautiful circular crystals are formed, sometimes large, and sometimes smaller than the 1000th of an inch. Round some of them there is formed a radiant crystalline halo, separated by a black circle from the disc which gives the four luminous sectors. The polarisation of the crystals is *positive*, and they are perfect at the centre, unless when large and badly formed.

15. *Hatchetine*, *Cacao Butter*, *White Wax*, *Tallow*, *Adipocire*, and all *Soaps* and different kinds of *Fat*, produce circular crystals like *Oil of Mace*, and give the same halos, which, in the polariscope, are divided into four sectors.

16. *Borax in Phosphoric Acid.*—The crystals produced by this combination are those discovered by Mr Talbot, and have been already described. I mention them again, in order to notice the interesting hemispherical bells which I have observed when an aqueous solution is raised into froth by heat. These bells or bubbles indurate and polarise the light by refraction, as in the case of *nitrate of uranium* already mentioned. They are traversed by the black cross, and exhibit rings of colour, which are *green* at the centre, then *red*, then *green* and *red* again. Some of these bells contained smaller ones within them, in one case no fewer than eight; and in one of them I distinctly observed a crystalline structure, the minute crystals radiating from the apex of the bell.

17. *Mannite.*—This substance gives circular crystals with more facility and certainty than any other which I have examined. When Mannite is melted by heat, it gives beautiful circular crystals. When dissolved in water they are very good. They are not good in alcohol or ether; but in acetic acid the finest circular crystals are formed. The black concentric circles are peculiarly fine, and are, so far as the microscope can shew it, entirely free of matter. In the crystals from acetic acid, the sectors shade off into the arms of the black cross with such perfection, that the circular disc loses its flat appearance, and seems to be composed of *four solid cones*, whose apices meet in the centre. In place of being circular the crystals are sometimes drawn out, as it were, into long cones, as shewn in Fig. 19, rounded at their summits, and having the appearance of solids of that shape. The black cross appears at the summit of the rounded cones, one of its arms, and sometimes two, according to the position of the plane of primitive polarisation, stretching out to the termination of the rounded cone. These cones are often crossed by *two*, *three*, or *four* concentric arches, perfectly black. In these elon-

gated crystallisations, the elementary particles are in perfect optical contact, the tint which they produce being a bright white of the *first* order. At this period of their formation, a crystallised and semi-opaque crust is formed above many of them, the opacity arising from an imperfection in the optical contact. This crust sometimes cracks and falls off, leaving the perfect crystal beneath, or when it merely cracks, shewing the perfect crystal through the fissure. These incrustations sometimes occupy the middle of the spaces between the black arches, *m n*, *o p*, &c., and raise the tint to an orange-brown. In a specimen preserved in Canada balsam, the balsam has insinuated itself between the imperfectly-united elementary crystals, and made the crust so transparent, that the crystal beneath it is most distinctly seen, as if through a piece of glass.

In some specimens, the optical contact is so imperfect, that groups of discs have a pale nut-brown semi-transparency, with the concentric black bands finely developed.

In other specimens, we have every degree of transparency, up to absolute opacity. In some discs, the black cross is scarcely seen, and they seem as if they were composed of fine threads of worsted, from the sides of which other finer threads diverge. Such crystals are beautifully white by reflected light, and look as if they were formed of fibres of white satin.

An interesting peculiarity in the larger discs is shewn in Fig. 21, where each successive ring is formed by radiations from the margin of the preceding ring. These radiations or tufts are occasionally separate, as in the figure, but generally in optical contact, so as to form a luminous ring in which the tints are not uniform.

In weak solutions of mannite, the crystallisations are exceedingly delicate, and the light which they polarise scarcely visible.

18. *Oxalurate of Ammonia* (*pure.*)—This salt, to which my attention was called by Professor Gregory, and which, according to that chemist, is probably identical with the Lithoxanthate of Ammonia, gives very beautiful *negative* circular crystals.

With weak aqueous solutions the discs are small and beautiful, and very much like those from *Lithoxanthate of Ammonia*, the cross sometimes consisting wholly of circular discs, and at other times of a few discs interspersed among dendritic crystallisations.

From strong solutions the discs are often nearly opaque, and round them are formed concentric rings, consisting of marginal radiations, as in Fig. 20, their elements being often in optical contact, and yielding different polarised tints. Occasionally we find discs, sometimes large ones, in which the central circle consists of tints of the *green* of the second order, with a feebly-developed black cross, descending to the *white* of the first order. This is followed by a narrow black concentric space, beyond which the *white* tint reappears, and rises to the *yellow* of the second order, which again descends to *white*, thus completing the *second* ring.

Other three rings follow in succession, the *white* tint rising to the *yellow*, and again falling to its original colour. Each of these five rings have precisely the same tints throughout their circumference, and when a number of such crystals appear in the dark field, they form objects of singular beauty.

In some specimens, the discs have the appearance of cones, as in *Mannite*. They have, in the centre of the black cross, another cross whose arms bisect the sectors, having sometimes a *white*, or *yellow*, or *green* tint. This cross is surrounded with a faint ring, which separates it from large sectors of a bright *pink* colour.

The circular discs are often composed of radial lines of different thicknesses, and in imperfect optical contact. Their tints consequently vary throughout the disc, and have a remarkable appearance. When the crystals are very small, they produce the polarised halos given by oil of mace.

19. *Hippuric Acid.*—This salt gives imperfect discs when melted. With water, it gives good circular crystals, but very fine ones with alcohol. They have a great variety of forms and tints, depending on the strength of the solution; but they differ from other circular crystals in two points. The radial lines are often separated from one another by black spaces of the same breadth as the luminous radial lines, and the whole disc is covered with almost invisible concentric black circles, at equal distances from one another. They are seen most distinctly in the white field. The four central sectors are often surrounded with a ring separated from them by a black space entirely free from matter. In some specimens, the discs consist of eight or ten sectors of uniform thickness and tint, which become black when in the plane of primitive polarisation. In other specimens, the crystallisations are large, irregular, and highly coloured.

Having thus described the phenomena exhibited by some of the more important circular crystals, I shall give a tabular list of the other substances in which I have found the property of giving circular crystallisations, arranging them under the heads of *Positive* and *Negative*, as formerly explained.

Positive Circular Crystals.

Sulphate of ammonia and magnesia.
... red oxide of manganese.
Hydrate of potash.
Citrate of potash.
Muriate of morphia.
... magnesia.
Almond soap.
Starch.
Substance in garnet.
... mica.
Chloride of strontian.
Sulphate of cobalt and ammonia.
Sulphate of iron and ammonia.
... ... potash.
... manganese and ammonia.
... magnesia and ammonia.
... zinc and potash.
Disulphate of mercury.
Mannite.
Citrate of ammonia.
Myristic acid.
Cuprose sulphate of potash.
Kreatinine.

Negative Circular Crystals.

Borax in phosphoric acid.
Lithoxanthate of ammonia.
Salicine.
Asparagine.
Manna.
Parmeline.
Palmic acid.
Palmine.
Esculine.
Berberine.
Cinchonine.
Theine.
Oil of mace.
Cacao butter.
Hatchetine.
Animal fat.
White wax.
Chrysoleplinic acid.
Succinate of zinc.
Chromic acid.
Citric acid.
Nitrate of uranium.
... urea.
... brucine.
... strychnine.
Gallic acid.
Thianuret of ammonia.
Sulphuret of cadmium.
Sulphate of copper and iron.
... ... zinc.
... ... magnesia.
... magnesia of potash.
... copper of ammonia.
... zinc of ammonia.
... zinc.
Substance in garnet.
Stearine.*
Stearic acid.
Palmitic acid.
Acetate of strontian.
... quinine.
Chloride of zinc.
Oxide of uranium.
Protoxide of nickel.
Phosphate of nickel.
Carbonate of nickel.
Substance in mica.
Adipocire.
Margaric acid.
Ethal.
Oxalurate of ammonia.
Kreatine.
Carbazotate of potash.
Sulphuret of potassium.
Hippuric acid.
Santonine.

To this list of substances which, under certain favourable conditions of crystallisation, after solution or fusion, give circular crystals, the perfection of which depends on causes over which the observer has little control, I may add the following animal substances, in which the circular phenomena are produced, and in which, with one exception, the structure is negative, as the greater number of such structures seem to be.

Hoof of horse, both transverse and vertical.
... ass, transverse section.
Transparent aperture in the wing of the beetle.
Hoof of rhinoceros.
Horn of rhinoceros, transverse and vertical.
... antelope.
Hairs of animals, sections of.

In examining the crystallisations of the *Chromate* and *Protochloride* of *Mercury*, and of the *Sulphuret of Bismuth*, I found that they exhibited the hemispherical

* Stearine gives the same polarised halos as oil of mace.

bells already described, in which oblique refraction and the thinness of the film combine to produce beautiful coloured rings, with a black cross.

In other crystals, such as *Muriate* and *Citrate* of *Quinine*, *Codeine*, and *Nitrate* of *Codeine*, I have observed the luminous sectors, and the black cross round the air-bubbles, which are formed after fusion, a phenomenon exactly the same as that which takes places round cavities in diamonds, amber, and other substances.

Having thus described the principal phenomena of circular crystals, I shall now proceed to make a few observations on their formation and decomposition. Circular crystals are abnormal aggregations, which owe their existence to some disturbing cause. The natural tendency of the elementary molecules of the most perfect of them, is to combine with their homologous axes parallel to one another, and to form regular crystals; and it is only when this tendency is counteracted by the quick application of heat or cold, by pressure, or by the nature of the solvent or of the combined ingredients, as in the case of borax and phosphoric acid, that the molecules are constrained to arrange themselves round a centre, not merely in radiating prisms, as in *Wavellite* and some other minerals, but according to laws which could not have been anticipated from any known principles of crystallisation. If, owing to any disturbing cause, two molecules should be deposited with their axes at right angles to each other, or four with their similar poles directed to the same point, this will lead to the formation of a circular disc, which will be of limited thickness, if the crystallisation takes place between two plates of glass pressed together, or to the formation of a spherical crystal, as in the Lithoxanthate of Ammonia, when there is room for its growth in all directions. The disc, or the sphere, might thus increase to a considerable size, if there was only one centre of crystallisation, but as the same causes have been operating all around, the size of the circular crystal is limited by the number of molecules within its sphere, or by its junction with the other discs around it. In this last case, they form a sort of mosaic, in which their shape is not circular, but hexagonal, as in manna, oil of mace, and many other substances.

In the greater number of circular crystallisations, the tints are a minimum at the centre of the disc, and increase outwards,—that is, the molecules form a thinner film at the centre, which increases in thickness towards the circumference; but in other cases the reverse of this takes place, and in the disc represented in Fig. 3, where the tints are those of Newton's rings, some cause, which we cannot even conjecture, must have determined the atoms to unite according to the complex law which connects these tints with the thicknesses at which they are produced. A cause of an opposite kind must have given birth to the disc shewn in Fig. 4, where the molecules form a thick film at the centre, which diminishes in thickness from nine to three as the tint passes from the central *blue* to the *white* at the circumference.

It is equally difficult to assign any reason for the production of the concentric bands of a uniform tint, which suddenly pass to another tint belonging to a different order of colours, and produced by a different thickness of material. A circular ring of *green*, for example, will pass *per saltum*, to a *red* of the next order, from a thickness of 9 to a thickness of 18; and this, according to a law which operates at every point of the circumference of the ring. Nor is this phenomenon less remarkable when the transition takes place in the very lowest order of tints, and at the smallest thickness of the film, as shewn in Figs. 6, 10, and 13, where the tint passes in repeated alternations from the pale *blue* to the beginning of *black*, rising to a *maximum* of *blue*, and again descending to the *minimum* of *black*.

The black rings or circles shewn in Figs. 5, 6, 10, and 13, require to be carefully studied, and with the finest microscopes. In most cases they seem to be spaces devoid of crystalline matter; but they have in general another origin. A line often appears perfectly black, when it corresponds with the *violet* of the *second* order, which separates the *indigo* of the same order from the *red* of the *first* order. Another set of lines appear black, from their being the junction lines of crystals not in perfect optical contact. A third set of black circles are produced by the extreme thinness of the substance, which is not capable of polarising the *very black* of NEWTON'S scale, and the existence of which upon the glass plate can be ascertained only by the highest powers of a fine microscope. But though in all these examples there is no breach of continuity in the circular disc, yet there are cases, as in the double black ring in Fig. 10, where *the corresponding space is devoid of all crystalline matter*. The crystallisation of the disc had been completed at the inner margin of the first black ring, and by some repulsive power the molecules in the solution were kept at a distance from the completed disc, and deposited themselves in a scarcely visible ring around the outer margin of the first black ring. The repulsive power again came into play; and another black ring intervened, the molecules being deposited at the same distance as formerly from the last-formed ring. What repulsive power this is, if it is not electrical, and how it operates, if it is electrical, we cannot even conjecture.

Another remarkable peculiarity in circular crystals is shewn in Figs. 11 and 12, where, as in chromic acid, the disc consists of alternations of dark and luminous circles, equidistant from each other. The dark circles are composed of the acid in particles too small to polarise light, and the luminous ones of separate patches of crystalline matter thick enough to give the *blue* and sometimes the *white* of the first order, and separated from one another by matter too thin to polarise light. In some rare cases, the spaces between the circles and between the patches are, like the black rings formerly described, devoid of crystalline matter. The separation of the patches in this case, is no less remarkable than the separation of the luminous circles. In the Adipocire from Paris, the tint of

the patches sometimes reaches the *yellow* of the first order, and its crystallisation has a very singular appearance.

When the molecules of the same body, or those of different bodies, are combined under the influence of disturbing causes, we may reasonably expect that their union will neither be strong nor permanent. When regular crystals are melted by heat, either alone or along with other bodies, their molecules are forced into positions of unstable equilibrium, and the natural tendency of similar poles to unite is aided by every mechanical vibration, and every variation of temperature to which they are exposed. Different kinds of glass, for example, in which earths, alkalies, and metals may have been combined by fusion, are thus completely decomposed by time, and the elementary particles, liberated from their constrained position, resume their place in crystals regularly formed. The specimens of ancient glass found at Nineveh, and in various parts of Italy and Greece, have undergone the most remarkable decomposition, and some of it converted into a sort of indurated mass, which can be broken between the fingers. The character of these decompositions, and the process by which they are effected, I have had occasion to describe in the *Appendix* to Mr Layard's new work on Nineveh and Babylon.* The same principles operate in the decomposition of circular crystals, and the same phenomena are exhibited in their restoration to their original state.

In circular crystals the decomposition takes place in different ways. In those from borax and phosphoric acid, which I have had occasion to watch month after month for several years, the decomposition generally begins at the centre, which is dissolved, or occupied by a number of minute prisms, with their axes lying in every direction. These prisms sometimes are arranged in a ring round the centre, and I have seen them like a St Andrew's cross. In other crystals, the decomposition goes on in radial lines or streaks, where the optical contact has not been complete; but in the more perfect crystals it takes place in concentric circles, sometimes double, the colours between each pair of circles being different. Numerous cavities are formed,—pieces of the crystal separate, and irregular crystals are often formed in the solution. Decomposition sometimes takes place without solution: the crystal preserves its form, the black circles are granulated, and the colours wholly disappear. In one of the specimens in my possession, every crystal has vanished, and their elements converted into beautiful prisms, united like a bunch of straw tightened at the middle. Between these groups there are numerous flat crystals, of considerable size, and of a perfectly uniform tint. All these decompositions have been the work of several years; and in the course of one year more there will not be found a vestige of the original crystals.

In *Manna* the transformation of the circular into their component crystals

* Discoveries in the Ruins of Nineveh and Babylon. By Austen H. Layard, M.P. Appendix, p. 674–676.

goes on more slowly, and in a more singular manner. It commences at the hexagonal junctions of the discs, all of which become *black* by transmitted, but *white* by reflected light. These minute crystals, which are transparent when separate, diffuse themselves around, as if they had fallen in a shower. The same kind of decomposition goes on in radial lines, and a granular decomposition takes place over the coloured sectors, commencing at their centre, obliterating the black cross, and destroying the tints of all orders.

In *Oil of Mace*, the decomposition is effected in a single night. The area of the disc is filled with drops of fluid and atoms of solid matter which have no action upon light, while an opaque ingredient occupies its circular margin.

In *Palmerine* and some other crystals, the film decays in spots, where the tint descends from that of the film to *zero* in concentric circles, while in other spots the tint rises in similar rings, as if the atoms, liberated from one spot, had been deposited in another.

Such are the details respecting the nature, formation, and decomposition of circular crystals, which I wish to submit to the Society. Lengthened as they are, they are but a brief abstract of the numerous observations, which, during the last ten years, I have made on this class of bodies. Their bearing upon unsettled questions in the molecular philosophy cannot be doubted. If it is in the agency of its ordinary laws that we recognise the beauty and harmony of the material universe, it is in the abnormal phenomena which so often perplex us, that Nature discloses her mysteries and reveals her laws.

St Leonard's College, St Andrews,
15*th March* 1853.

PROCEEDINGS

OF THE

STATUTORY GENERAL MEETINGS,

AND

LIST OF MEMBERS ELECTED AT THE ORDINARY MEETINGS,

SINCE DECEMBER 3, 1849;

WITH

LIST OF DONATIONS TO THE LIBRARY,

FROM DEC. 3, 1849, TILL APRIL 18, 1853.

PROCEEDINGS, &c.

Monday, November 26, 1849.

At a Statutory General Meeting, Dr CHRISTISON, V.P., in the Chair, the following Office-Bearers were duly elected :—

Sir T. MAKDOUGALL BRISBANE, Bart., G.C.B., G.C.H., President.

Sir D. BREWSTER, K.H.,
Very Rev. Principal LEE,
Right Rev. Bishop TERROT,
Dr CHRISTISON,
Dr ALISON,
Hon. Lord MURRAY,
} Vice-Presidents.

Professor FORBES, General Secretary.

Dr GREGORY,
Professor SMYTH,
} Secretaries to the Ordinary Meetings.

JOHN RUSSELL, Esq., Treasurer.

Dr TRAILL, Curator of Library and Instruments.

JOHN STARK, Esq., Curator of Museum.

COUNSELLORS.

Sir WM. JARDINE, Bart.	Sir JOHN M'NEILL, G.C.B.
Rev. Dr J. ROBERTSON.	JOHN CAY, Esq.
CHARLES MACLAREN, Esq.	Professor KELLAND.
J. T. GIBSON-CRAIG, Esq.	Very Rev. E. B. RAMSAY.
JAMES DALMAHOY, Esq.	Dr J. Y. SIMPSON.
Dr GEORGE WILSON.	JAMES WILSON, Esq.

The following Committee was appointed to audit the Treasurer's accounts :—

J. T. GIBSON-CRAIG, Esq. JOHN CAY, Esq. D. SMITH, Esq.

The Meeting then adjourned.

(Signed) R. CHRISTISON, *V.P.*

Memorandum.—January 7, 1850.—At the Ordinary Meeting of this date, Sir THOMAS M. BRISBANE, Bart., in the Chair, the Chairman stated that the Council had caused to be prepared a Memorial to the proper authorities, expressive of a desire that the Trigonometrical Survey should now be carried on in Edinburghshire. The Memorial was read, and, on the motion of Dr FLEMING, seconded by Mr SMITH of Jordanhill, was unanimously adopted, and ordered to be transmitted to the Superintendent of the Trigonometrical Survey of Great Britain.

Memorandum.—February 18, 1850.—Of this date, the Council reported that, having been requested to nominate a Committee of the Society, to act as Members of a General Committee about to be formed for the furtherance of the Great Exhibition of Manufactures, &c., in London, in 1851, they had named Dr TRAILL, Professor C. PIAZZI SMYTH, and Dr GEORGE WILSON. Thereafter Mr Sheriff CAY'S name was proposed and accepted as an additional Member of Committee.

Memorandum.—Monday, March 4, 1850.—At the Ordinary Meeting of this date, the following motion, of which notice had been given at last Meeting, was made by Dr CHRISTISON, seconded by Dr TRAILL, and unanimously carried,—"That Edinburgh having been fixed upon for the Twentieth Annual Meeting of the British Association for the Advancement of Science, which is to take place in August next, it is requisite that a Subscription be immediately set on foot for defraying the necessary expenses attendant on the reception of that Body.

"That the noblemen and gentlemen of Scotland generally, especially those resident in Edinburgh, be as soon as possible requested to enrol their names as subscribers to such a fund; and that a hope be expressed that Public Bodies and Chartered Societies will be induced to contribute their aid, as far as they may possess the means, in increasing the amount of this fund.

"That the Royal Society of Edinburgh having been one of the channels through which the invitation to the British Association to hold its Meeting this year in Edinburgh was conveyed, shall forthwith commence a Subscription among its own Fellows, and shall nominate a Committee to co-operate with the local Office-Bearers of the Association, and with the other Public Bodies in promoting this and other objects connected with the Meetings.

"That the sum of £50 be paid out of the funds of the Royal Society on account of this Subscription, and that all the Members of the Society be specially invited to contribute individually to this fund, it being understood that no donation should be of less amount than One Guinea."

Monday, November 25, 1850.

At a Statutory General Meeting, Hon. Lord MURRAY, V.P., in the Chair, the following Office-Bearers were duly elected :—

Sir T. MAKDOUGALL BRISBANE, Bart., G.C.B., G.C.H., President.

Sir D. BREWSTER, K.H.,
Very Rev. Principal LEE,
Right Rev. Bishop TERROT,
Dr CHRISTISON,
Dr ALISON,
Hon. Lord MURRAY.
} Vice-Presidents.

Professor FORBES, General Secretary.

Dr GREGORY,
Professor SMYTH,
} Secretaries to the Ordinary Meetings.

JOHN RUSSELL, Esq., Treasurer.
Dr TRAILL, Curator of Library and Instruments.
JAMES WILSON, Esq., Curator of Museum.

COUNSELLORS.

JAMES DALMAHOY, Esq.	Dr J. Y. SIMPSON.
Dr GEORGE WILSON.	His Grace the Duke of ARGYLL.
Sir JOHN M'NEILL, G.C.B.	Hon. Lord IVORY.
JOHN CAY, Esq.	Rev. Dr FLEMING.
Professor KELLAND.	Professor GOODSIR.
Very Rev. E. B. RAMSAY.	Rev. J. HANNAH.

The following Committee was appointed to audit the Treasurer's accounts :—

J. T. GIBSON-CRAIG, Esq. JOHN CAY, Esq. JAMES WALKER, Esq.

The Meeting then adjourned.

(Signed) JOHN A. MURRAY, *V.P.*

Memorandum.—January 20, 1851.—At the Ordinary Meeting of this date it was agreed, on the motion of His Grace the Duke of ARGYLL, seconded by Professor FORBES, that this Society should address Her Majesty's Government in favour of a more urgent prosecution of the Trigonometrical Survey of Scotland. A remit was made to the Council to prepare and transmit a Memorial on the subject, and the names of ALEXANDER KEITH JOHNSTON, Esq., and DAVID MILNE, Esq., were added for that purpose to those of the Council Committee.

Monday, November 24, 1851.

At a Statutory General Meeting, JOHN RUSSELL, Esq., Treasurer, in the Chair, the following Office-Bearers were duly elected :—

Sir T. MAKDOUGALL BRISBANE, Bart., G.C.B., G.C.H., President.
Sir D. BREWSTER, K.H.,
Very Rev. Principal LEE.
Right Rev. Bishop TERROT.
Dr CHRISTISON.
Dr ALISON.
Hon. Lord MURRAY.
} Vice-Presidents.
Professor FORBES, General Secretary.
Dr GREGORY,
Professor SMYTH,
} Secretaries to the Ordinary Meetings.
JOHN RUSSELL, Esq., Treasurer.
Dr TRAILL, Curator of Library and Instruments.
JAMES WILSON, Esq., Curator of Museum.

COUNSELLORS.

Professor Kelland.	Rev. J. Hannah.
Dr J. Y. Simpson.	Dr Anderson.
His Grace the Duke of Argyll.	Robert Chambers, Esq.
Hon. Lord Ivory.	J. T. Gibson-Craig, Esq.
Rev. Dr Fleming.	Wm. Swan, Esq.
Professor Goodsir.	Professor William Thomson.

The following Committee was appointed to audit the Treasurer's accounts :—

J. T. Gibson-Craig, Esq., W.S. James Walker, Esq., W.S. William T. Thomson, Esq.

The Meeting then adjourned.

(Signed) John Russell.

Memorandum.—November 24, 1851.—At a Statutory General Meeting of this date, John Russell, Esq., Treasurer, in the Chair, a letter from Sir Thomas M. Brisbane, Bart., to the Secretary was read, stating that as, on account of the illness of one of his family, he intended passing the winter in England, he considered it right to tender his resignation as President of the Society.

It was moved by the Chairman, and unanimously agreed to, that "the Meeting should direct the Secretary to express to Sir Thomas M. Brisbane their regret at the cause which is likely to prevent his attendance this session; but, as they trust that this cause will be temporary, they would request him to withdraw his resignation, and hope that at a future period he will be able to resume the Chair, the duties of which he has so ably discharged."

Memorandum.—December 12, 1851.—At a Meeting of Council of this date, the acting General Secretary read extract of letter from Sir T. M. Brisbane to Professor Forbes, in which he expressed his compliance with the request of the Society, made at the first General Meeting of this session, that he would withdraw his resignation of the Presidentship which he had then tendered.

Memorandum.—January 5, 1852.—At an Ordinary Meeting of this date, Sir D. Brewster, V.P., in the Chair, a Memorial to the Lords of the Treasury regarding the formation in Edinburgh of a Museum of Economic Geology, and praying for the extension of the Geological Survey to Scotland, was read and approved of, and ordered to be transmitted, after signature by Sir Thomas M. Brisbane, to the Treasury.

N.B.—The Memorial above referred to was transmitted accordingly, and its receipt acknowledged, on 13th February 1852, by Mr George Cornwall Lewis, the Secretary to the Treasury.

Monday, November 22, 1852.

At a Statutory General Meeting, Right Rev. Bishop Terrot, V.P., in the Chair, the following Office-Bearers were duly elected :—

Sir T. Makdougall Brisbane, Bart., G.C.B., G.C.H., President.

Sir D. Brewster, K.H.,
Very Rev. Principal Lee,
Right Rev. Bishop Terrot,
Dr Christison,
Dr Alison,
Hon. Lord Murray,
} Vice-Presidents.

Professor Forbes, General Secretary.

Dr Gregory,
Professor Smyth,
} Secretaries to the Ordinary Meetings.

John Russell, Esq., Treasurer.

Dr Traill, Curator of Library and Instruments.

James Wilson, Esq., Curator of Museum.

COUNSELLORS.

Rev. Dr Fleming.	Wm. Swan, Esq.
Professor Goodsir.	Professor William Thomson.
Rev. J. Hannah.	Dr J. H. Bennett.
Dr Anderson.	Dr J. H. Balfour.
Robert Chambers, Esq.	Andrew Coventry, Esq.
J. T. Gibson-Craig, Esq.	Rev. Dr James Grant.

The following Committee was appointed to audit the Treasurer's accounts:—

J. T. Gibson-Craig, Esq. John Mackenzie, Esq. James Cunningham, Esq. W.S.

The Meeting then adjourned.

(Signed) C. H. Terrot, *V.P.*

Memorandum.—Monday, November 22, 1852.—At a Statutory General Meeting of this date, the Right Rev. Bishop Terrot in the Chair, a letter was read from Professor Forbes of 6th curt., in which, in consequence of the state of his health requiring a renewed leave of absence from Edinburgh, he unreservedly resigned the office of General Secretary, which the Society had done him the honour to confer upon him for a series of years, an honour which he stated he ever highly prized. After a few observations by the Right Rev. Chairman, expressive of the Society's regret at this intimation, and of the high estimation in which Professor Forbes was deservedly held (in which observations the Society most cordially concurred), it was moved by Professor Christison, "That Professor Forbes be requested to withdraw his resignation." This motion was immediately and unanimously adopted by the Society, and the acting General Secretary was instructed to communicate the resolution to Professor Forbes.

Memorandum.—In reference to Professor Forbes's proposed resignation of the General Secretaryship, and the resolution of the Society regarding it, as recorded in the preceding Minutes of the last Statutory General Meeting, it is to be here noted that the acting General Secretary communicated to Professor Forbes the unanimous resolution and request of the Society that he should withdraw his resignation, and that a letter of date the 24th day of November was duly received from Professor Forbes, in which, in accordance with the wishes of the Society, he withdrew his resignation.

MEMBERS ELECTED.

December 3, 1849.

His Grace the Duke of Argyll.

December 17, 1849.

The Most Noble the Marquis of Tweeddale.

January 7, 1850.

W. J. Macquorn Rankine, Esq., C.E.

January 21, 1850.

Alex. Keith Johnston, Esq. Dr John Scott, F.R.C.P.
Dr Sheridan Muspratt, Liverpool.

February 18, 1850.

Dr James Stark. (Re-admitted).

March 4, 1850.

Lieut. W. Driscoll Gosset, R.E. Dr William Seller, P.R.C.P.E.

March 18, 1850.

Professor Blackburn, Glasgow. Thomas Grainger, Esq., C.E.

April 15, 1850.

Alexander Kemp, Esq.

December 2, 1850.

Dr R. D. Thomson, Glasgow. Dr Mortimer Glover, Newcastle.

December 16, 1850.

Beriah Botfield, Esq. Dr J. S. Combe.
Dr Spittal. (Re-admitted).

February 3, 1851.

Sir David Dundas, Bart.

February 17, 1851.

Sir George Douglas, Bart.

March 3, 1851.

John Stewart, Esq. Dr John Kinnis.

April 7, 1851.

E. W. Dallas, Esq.

April 21, 1851.

Rev. Dr James Grant.

December 15, 1851.

Rev. A. Barry, Glenalmond. Sir James Ramsay, Bart.

January 19, 1852.

Eyre B. Powell, Esq., Madras. Thomas Miller, Esq., Perth Academy.
Allen Dalzell, Esq.

February 2, 1852.

Dr John Wylie, late Physician-General, Madras.

February 16, 1852.

James Cunningham, Esq., W.S.

April 5, 1852.

James W. Grant, Esq. of Elchies.

December 6, 1852.

Alex. James Russell, Esq., C.S. Dr Andrew Fleming, Bengal.

January 4, 1853.

Major Edward Madden. Dr James Watson, Bath.
Lieut. Robert Maclagan, Bengal Engineers.

February 7, 1853.

Rev. Dr Robert Lee. Professor J. S. Blackie.
Right Rev. Bishop Trower, D.D.

February 21, 1853.

James M. Hog, Esq. of Newliston. Rev. John Cumming, D.D., London.

LIST OF THE PRESENT ORDINARY MEMBERS,

IN THE ORDER OF THEIR ELECTION.

General Sir THOMAS M. BRISBANE, Bart., G.C.B, &c., F.R.S. Lond.

PRESIDENT.

Date of Election.

1798 Alexander Monro, M.D.

1799 Robert Jameson, Esq., *Professor of Natural History.*

1807 John Campbell, Esq., *of Carbrook.*

1808 James Wardrop, Esq.

Sir David Brewster, K.H., LL.D. F.R.S, Lond., *St Andrews.*

1811 General Sir Thomas Makdougall Brisbane, Bart., G.C.B., G.C.H., F.R.S. Lond.

James Jardine, Esq., *Civil Engineer.*

J. G. Children, Esq., F.R.S. Lond.

Alexander Gillespie, Esq., *Surgeon.*

W. A. Cadell, Esq., F.R.S. Lond.

1812 James Pillans, Esq., *Professor of Humanity.*

Sir George Clerk, Bart., F.R.S. Lond.

1813 William Somerville, M.D., F.R.S. Lond.

1814 Right Honourable Lord Viscount Arbuthnot.

John Fleming, D.D., *Professor of Natural Science, New College.*

Alexander Brunton, D.D.

1815 Henry Home Drummond, Esq., *of Blair-Drummond.*

William Thomas Brande, Esq., F.R.S. Lond., *Professor of Chemistry in the Royal Institution.*

1816 Leonard Horner, Esq., F.R.S. Lond.

Honourable Lord Fullerton.

1817 John Wilson, Esq., *late Professor of Moral Philosophy.*

Alexander Maconochie, Esq., *of Meadowbank.*

William P. Alison, M.D., *Professor of the Practice of Physic.*

Robert Bald, Esq., *Civil Engineer.*

Date of Election.

1818 Robert Richardson, M.D., *Harrowgate.*
Patrick Miller, M.D., *Exeter.*
John Watson, M.D.
Right Honourable John Hope, *Lord Justice-Clerk.*
1819 Patrick Murray, Esq., *of Simprim.*
Thomas Stewart Traill, M.D., *Professor of Medical Jurisprudence.*
Alexander Adie, Esq.
Marshall Hall, M.D., *London.*
Richard Philips, Esq., F.R.S. Lond.
Reverend William Scoresby, *Exeter.*
George Forbes, Esq.
1820 James Keith, M.D., *Surgeon.*
Charles Babbage, Esq., F.R.S. Lond.
Sir John F. W. Herschel, Bart., F.R.S. Lond.
John Shank More, Esq., *Professor of Scots Law.*
Robert Haldane, D.D., *Principal of St Mary's College, St Andrews.*
Dr William Macdonald, *Professor of Natural History, St Andrews.*
Sir John Hall, Bart., *of Dunglass.*
Sir George Ballingall, M.D., *Professor of Military Surgery.*
1821 Sir James M. Riddell, Bart., *of Ardnamurchan.*
Archibald Bell, Esq., *Advocate.*
John Clerk Maxwell, Esq., *Advocate.*
John Lizars, Esq., *Surgeon.*
John Cay, Esq., *Advocate.*
Robert Kaye Greville, LL.D.
Robert Hamilton, M.D.
1822 James Smith, Esq., *of Jordanhill,* F.R.S. Lond.
William Bonar, Esq.
George A. Walker-Arnott, LL.D., *Professor of Botany, Glasgow.*
Very Reverend John Lee, D.D., *Principal of the University of Edinburgh.*
Sir James South, F.R.S. Lond.
Lieutenant-General Martin White.
Walter Frederick Campbell, Esq.
Sir W. T. Trevelyan, Bart., *Nettlecombe, Somersetshire.*
Sir Robert Abercromby, Bart., *of Birkenbog.*
Dr Wallich, *Calcutta.*
John Russell, Esq., P.C.S.
John Dewar, Esq., *Advocate.*
823 Sir Edward Ffrench Bromhead, Bart., A.M., F.R.S. Lond., *Thurlsby Hall.*
Captain Thomas David Stuart, *of the Hon. East India Company's Service.*
Andrew Fyfe, M.D., *Professor of Medicine and Chemistry, King's College, Aberdeen.*
Robert Bell, Esq., *Advocate.*
Admiral Norwich Duff.
Warren Hastings Anderson, Esq.

Date of Election.

1823 Alexander Thomson, Esq., *of Banchory.*
Liscombe John Curtis, Esq., *Ingsdon House, Devonshire.*
Robert Christison, M.D., *Professor of Materia Medica.*
John Gordon, Esq., *of Cairnbulg.*

1824 Alexander Wilson Philip, M.D., *London.*
Robert E. Grant, M.D., *Professor of Comparative Anatomy, University College, London.*
Reverend Dr William Muir, *one of the Ministers of Edinburgh.*
W. H. Playfair, Esq., *Architect.*
John Argyle Robertson, Esq., *Surgeon.*
James Pillans, Esq.
James Walker, Esq., *Civil Engineer.*
William Wood, Esq., *Surgeon.*

1825 Honourable Lord Wood.

1826 Sir David Hunter Blair, Bart.
Dr John Macwhirter.

1827 John Gardiner Kinnear, Esq.
James Russell, M.D.
Reverend Dr Robert Gordon, *one of the Ministers of Edinburgh.*
James Wilson, Esq.
Very Reverend Edward Bannerman Ramsay, A.M., Camb.
George Swinton, Esq.

1828 Erskine Douglas Sandford, Esq., *Advocate.*
David Maclagan, M.D.
Sir William A. Maxwell, *of Calderwood*, Bart.
John Forster, Esq., *Architect, Liverpool.*
Thomas Graham, A.M., *Professor of Chemistry, London University.*
David Milne Home, Esq., *Advocate.*
Dr Manson, *Nottingham.*
William Burn Callander, Esq., *of Prestonhall.*

1829 A. Colyar, Esq.
Sir William Gibson-Craig, Bart.
James Ewing, LL.D., *Glasgow.*
Right Honourable Duncan M'Neill, *Lord Justice-General.*
Venerable Archdeacon Sinclair, *Kensington.*
Arthur Connell, Esq., *Profesor of Chemistry, St Andrews.*
Bindon Blood, Esq., M.R.I.A.
James Walker, Esq., W.S.
William Bald, Esq., M.R.I.A.

1830 J. T. Gibson-Craig, Esq., W.S.
Sir Archibald Alison, Bart., *Sheriff of Lanarkshire.*
Honourable Mountstuart Elphinstone.
James Syme, Esq., *Professor of Clinical Surgery.*
James L'Amy, Esq., *Sheriff of Forfarshire.*
Thomas Barnes, M.D., *Carlisle.*

Date of Election.

1831 James D. Forbes, Esq., F.R.S. Lond., *Professor of Natural Philosophy.*
Right Honourable Lord Dunfermline.
Donald Smith, Esq.
O. Tyndal Bruce, Esq., *of Falkland.*
David Boswell Reid, M.D., *London.*

1832 John Sligo, Esq., *of Carmyle.*
James F. W. Johnston, A.M., *Professor of Chemistry in the University of Durham.*
William Gregory, M.D., *Professor of Chemistry.*
Robert Allan, Esq., *Advocate.*
Robert Morrieson, Esq., *Hon. E.I.C. Civil Service.*
Montgomery Robertson, M.D.

1833 Captain Milne, R.N.
His Grace the Duke of Buccleuch, K.G.
David Craigie, M.D.
Sir John Stuart Forbes, Bart., *of Pitsligo.*
Alexander Hamilton, Esq., LL.B., W.S.
Right Honourable Earl Cathcart.

1834 Mungo Ponton, Esq., W.S.
Isaac Wilson, M.D., F.R.S. Lond.
David Low, Esq., *Professor of Agriculture.*
Patrick Boyle Mure Macredie, Esq., *Advocate.*
John Davies Morries Stirling, Esq.
Thomas Jameson Torrie, Esq.
John Haldane, Esq., *Haddington.*
William Sharpey, M.D., *Professor of Anatomy, University College, London.*

1835 John Hutton Balfour, M.D., *Professor of Botany.*
Right Honourable Lord Campbell.
William Brown, Esq., F.R.C.S.
R. Mayne, Esq.

1836 David Rhind, Esq., *Architect.*
Archibald Robertson, M.D., F.R.S. Lond.

1837 John Archibald Campbell, Esq., W.S.
John Scott Russell, Esq., A.M.
Charles Maclaren, Esq.
Archibald Smith, Esq., M.A. Camb., *Lincoln's Inn, London.*
Richard Parnell, M.D.
Peter D. Handyside, M.D., F.R.C.S.

1838 Thomas Mansfield, Esq., *Accountant.*
Alan Stevenson, Esq., *Civil Engineer.*

1839 David Smith, Esq., W.S.
Adam Hunter, M.D.
Rev. Philip Kelland, A.M., *Professor of Mathematics.*
William Alexander, Esq., W.S.
F. Brown Douglas, Esq., *Advocate.*

Date of Election.

1839 Colonel Swinburne.

1840 Alan A. Maconochie, Esq., *Professor of Civil Law, Glasgow.*
Martyn J. Roberts, Esq.
Robert Chambers, Esq.
James Forsyth, Esq.
Sir John M'Neill, G.C.B.
John Cockburn, Esq.
Sir William Scott, Bart., *of Ancrum.*
Right Reverend Bishop Terrot.
Edward J. Jackson, Esq.
John Learmonth, Esq., *of Dean.*
John Mackenzie, Esq.
James Anstruther, Esq., W.S.

1841 John Miller, Esq., *Civil Engineer.*
George Smyttan, M.D.
James Dalmahoy, Esq.

1842 James Thomson, Esq., *Civil Engineer, Glasgow.*
John Davy, M.D., *Inspector-General of Army Hospitals.*
Robert Nasmyth, Esq., F.R.C.S.
Sir James Forrest, Bart., *of Comiston.*
James Miller, Esq., *Professor of Surgery.*
John Adie, Esq.
John Goodsir, Esq., *Professor of Anatomy.*

1843 A. D. Maclagan, M.D., F.R.C.S.
John Rose Cormack, M.D., F.R.C.P., *Putney.*
Allen Thomson, M.D., *Professor of Anatomy, Glasgow.*
Joseph Mitchell, Esq., *Civil Engineer, Inverness.*
Duncan Davidson, Esq., *of Tulloch.*
Andrew Coventry, Esq., *Advocate.*
John Hughes Bennett, M.D., F.R.C.P., *Professor of Physiology.*
D. Balfour, Esq., *Younger of Trenaby.*
Henry Stephens, Esq.

1844 The Honourable Lord Murray.
Arthur Forbes, Esq., *of Culloden.*
J. Burn Murdoch, Esq., *Advocate.*
Archibald Campbell Swinton, Esq., *Professor of Civil Law.*
James Begbie, M.D., F.R.C.S.
James Y. Simpson, M.D., *Professor of Midwifery.*
David Stevenson, Esq., *Civil Engineer.*
Thomas R. Colledge, M.D., F.R.C.P.E.

1845 James Andrew, M.D.
George Wilson, M.D.
John G. M. Burt, M.D.
Thomas Anderson, M.D., *Professor of Chemistry, Glasgow.*

Date of Election.

1846 A. Taylor, M.D., *Pau.*
S. A. Pagan, M.D.
Reverend Dr James Robertson, *Professor of Divinity and Ecclesiastical History.*
Alexander J. Adie, Esq., *Civil Engineer.*
William Murray, Esq., *of Henderland.*
George Turnbull, Esq.
L. Schmitz, LL.D., Ph.D., *Rector of High School.*
Charles Piazzi Smyth, Esq., *Professor of Practical Astronomy.*
1847 George Makgill, Esq., *of Kemback.*
David Gray, Esq., *Professor of Natural Philosophy, Marischal College, Aberdeen.*
William Thomson, Esq., M.A. Camb., *Professor of Natural Philosophy, Glasgow.*
J. H. Burton, Esq., *Advocate.*
James Nicol, Esq., *Professor of Natural History, Aberdeen.*
William Macdonald Macdonald, Esq., *of St Martins.*
Robert Handyside, Esq., *Solicitor-General.*
Alexander Christie, Esq.
John Wilson, Esq., *Agricultural College, Cirencester.*
Moses Steven, Esq., *of Bellahouston.*
1848 James Tod, Esq., W.S., *Secretary to the Royal Scottish Society of Arts.*
Thomas Stevenson, Esq., C.E.
James Allan, M.D., *Haslar Hospital.*
Reverend John Hannah, D.C.L., *Rector of the Edinburgh Academy.*
Henry Davidson, Esq.
Patrick Newbigging, M.D.
William Swan, Esq.
Reverend Francis Garden.
Patrick James Stirling, Esq.
1849 William Stirling, Esq., *of Keir*, M.P.
John Thomson Gordon, Esq. *Sheriff of Mid-Lothian.*
Right Honourable Lord Rutherfurd.
D. R. Hay, Esq.
William Thomas Thomson, Esq.
Honourable Lord Ivory.
Honourable Lord Anderson.
William E. Aytoun, D.C.L., *Professor of Rhetoric and Belles Lettres.*
W. H. Lowe, M.D., *Balgreen.*
Honourable B. F. Primrose.
John Stenhouse, M.D., *Islington.*
David Anderson, Esq., *of Moredun.*
W. R. Pirrie, M.D., *Professor of Surgery, Marischal College, Aberdeen.*
Right Honourable The Earl of Minto, G.C.B.
Right Honourable The Earl of Aberdeen, K.T.
Right Honourable The Earl of Haddington, K.T.
His Grace The Duke of Argyll.

Date of Election.

1849 The Most Noble the Marquis of Tweeddale, K.T.

1850 William John Macquorn Rankine, Esq., C.E.
Alexander Keith Johnston, Esq.
Sheridan Muspratt, M.D., *Liverpool.*
James Stark, M.D. (Re-admitted.)
Captain W. Driscoll Gosset, R.E.
William Seller, M.D., F.R.C.P.E.
Hugh Blackburn, Esq., *Professor of Mathematics, Glasgow.*
Alexander Kemp, Esq.
R. D. Thomson, M.D., *London.*
Mortimer Glover, M.D., *Newcastle.*
Beriah Botfield, Esq.
J. S. Combe, M.D.

1851 Sir David Dundas, Bart., *of Dunira.*
Sir George Douglas, Bart., *of Springwood Park.*
John Stewart, Esq., *of Nateby Hall.*
E. W. Dallas, Esq.
Reverend Dr James Grant, *one of the Ministers of Edinburgh.*
Reverend A. Barry, *Glenalmond.*
Sir James Ramsay, Bart., *Banff House.*

1852 Eyre B. Powell, Esq., *Madras.*
Thomas Miller, Esq., *Perth Academy.*
Allen Dalzell, Esq.
James Cunningham, Esq., W.S.
James W. Grant, Esq., *of Elchies.*
Alexander James Russell, Esq., C.S.
Andrew Fleming, M.D., *Bengal.*

1853 Major Edward Madden.
James Watson, M.D., *Bath.*
Lieut. Robert Maclagan, *Bengal Engineers.*
Reverend Dr Robert Lee, *Professor of Biblical Criticism and Biblical Antiquities.*
John S. Blackie, Esq., M.A., *Professor of Greek.*
Right Reverend Bishop Trower, D.D.
James M. Hog, Esq., *of Newliston.*
Reverend John Cumming, D.D., *London.*

LIST OF NON-RESIDENT AND FOREIGN MEMBERS,

ELECTED UNDER THE OLD LAWS.

NON-RESIDENT.

Sir James Macgrigor, Bart., M.D.
Richard Griffiths, Esq., *Civil Engineer.*

FOREIGN.

Dr S. L. Mitchell, *New York.*
M. P. Prevost, *Geneva.*

LIST OF HONORARY FELLOWS.

His Majesty the King of the Belgians.
His Imperial Highness the Archduke John of Austria.
His Imperial Highness the Archduke Maximilian.
His Royal Highness Prince Albert.

FOREIGNERS (LIMITED TO THIRTY-SIX.)

* M. Arago,	*Paris.*
* M. Biot,	*Do.*
* M. de Hammer,	*Vienna.*
* M. de Humboldt,	*Berlin.*
M. Agassiz,	*United States.*
M. de Bernstein.	*Berlin.*
M. Cauchy,	*Paris.*
M. de Charpentier,	*Bex.*
M. Cousin,	*Paris.*
M. Degerando,	*Do.*
M. Charles Dupin,	*Do.*
M. Ehrenberg,	*Berlin.*
M. Elie de Beaumont,	*Paris,*
M. Encke,	*Berlin.*
M. Flourens.	*Paris.*
M. Gauss,	*Göttingen.*
M. Guizot,	*Paris.*
M. Haidinger,	*Vienna.*

N.B.—The four names marked thus * in the preceding list, were included in the original Honorary List prior to the change of the Law distinguishing British Subjects from Foreigners.

M. Hansteen,	*Christiania.*
M. Hausmann,	*Göttingen.*
M. Lamont,	*Munich.*
M. Leverrier,	*Paris.*
M. Liebig,	*Giessen.*
M. Melloni,	*Naples.*
M. Mitcherlich,	*Berlin.*
M. Müller,	*Do.*
M. Necker,	*Geneva.*
M. Plana,	*Turin.*
M. Quetelet,	*Brussels.*
M. Gustav Rose,	*Berlin.*
M. Struve,	*Pulkowa.*
M. Thenard,	*Paris.*
M. Tiedemann,	*Heidelberg.*

BRITISH SUBJECTS (LIMITED TO TWENTY, BY LAW X.)

J. C. Adams, Esq.,	*Cambridge.*
G. B. Airy, Esq.,	*Greenwich.*
Robert Brown, Esq.	*London.*
Dr Faraday,	*Do.*
Sir John Franklin,	*Do.*
Professor Graham,	*Do.*
Henry Hallam,	*Do.*
Sir W. R. Hamilton,	*Dublin.*
Sir John F. W. Herschel, Bart.,	*Collingwood.*
Sir William J. Hooker,	*Kew.*
W. Lassell, Esq.,	*Liverpool.*
Dr Lloyd,	*Dublin.*
Sir Charles Lyell,	*London.*
Sir Roderick I. Murchison,	*Do.*
Richard Owen, Esq.,	*Do.*
Sir W. E. Parry,	*Do.*
Earl of Rosse,	*Parsonstown.*
Rev. Dr Whewell,	*Cambridge.*

LIST OF FELLOWS DECEASED, RESIGNED, OR CANCELLED,

FROM JULY 1849 to SEPTEMBER 1853.

HONORARY FELLOWS DECEASED.

M. Berzelius, *Stockholm.*
M. Gay-Lussac, *Paris.*
M. Audubon, *United States.*
M. Alexandre Brongniart, *Paris.*
M. de Buch, *Berlin.*
M. Bürg, *Vienna.*
M. Jacobi, *Konigsberg.*
M. Neander, *Berlin.*
M. Oersted, *Copenhagen.*
M. Schumacher, *Altona.*
Sir M. I. Brunel, *London.*
William Wordsworth, *Rydal Mount.*

ORDINARY FELLOWS DECEASED.

Thomas Thomson, M.D., F.R.S.L., *Professor of Chemistry.*
George Dunbar, Esq., *Professor of Greek.*
Thomas Thomson, Esq., *Advocate,* P.C.S.
Sir Henry Jardine.
Patrick Neill, LL.D.
Robert Stevenson, Esq., C.E.
Henry Colebrooke, Esq.
Right Honourable Earl of Wemyss and March.
Sir D. J. Hamilton Dickson, M.D.
James Muttlebury, M.D.
Right Honourable David Boyle.
Thomas Guthrie Wright, Esq.
Sir John Mead, M.D.
A. R. Carson, LL.D.
Dr Lawson Whalley.
Admiral Sir Charles Adam, K.C.B.
Sir William Newbigging.
Right Honourable Lord Ruthven.
John Stark, Esq.
Thomas Brown, Esq., *of Lanfine.*
Captain Sir Samuel Brown, R.N.
T. S. Davies, Esq., A.M.
James Dunlop, Esq.
George Buchanan, Esq., C.E.

William Copland, Esq., *of Colliston.*
Reverend Edward Craig.
Sir J. Macpherson Grant, Bart.
Sir Alexander Gibson Carmichael, Bart.
William Nicol, Esq.
Henry Marshall, M.D., *Deputy Inspector-General of Hospitals.*
John Scott, M.D., F.R.C.P.E.
John Kinnis, M.D.
Robert Bryson, Esq.
Thomas Grainger, Esq., C.E.
James Spittal, M.D.
John Wylie, M.D., *late Physician-General, Madras.*
John Steuart Newbigging, Esq., W.S.
General Morison, C.B., *Madras Artillery.*

RESIGNATIONS.

A. T. J. Gwynne, Esq.
William Scot, Esq., H.E.I.C.S.
James Auchinleck Cheyne, Esq.
George J. Gordon, Esq.
John Hall Maxwell, Esq.
William Balfour, Esq.

ELECTIONS CANCELLED.

The Venerable Archdeacon Williams.
W. Preston Lauder, M.D.

The following Public Institutions and Individuals are entitled to receive Copies of the Transactions and Proceedings of the Royal Society of Edinburgh;—

ENGLAND.

The British Museum.
The Bodleian Library, Oxford.
The University Library, Cambridge.

The Royal Society.
The Linnean Society.
The Society for the Encouragement of Arts.
The Geological Society.
The Royal Astronomical Society.
The Royal Asiatic Society.
The Zoological Society.
The Royal Society of Literature.
The Horticultural Society.
The Royal Institution.
The Royal Geographical Society.
The Statistical Society.
The Institution of Civil Engineers.
The Institute of British Architects.
The Ordnance Geological Survey.
The Hydrographical Office, Admiralty.
The Medico-Chirurgical Society.
The Athenæum Club.
The Cambridge Philosophical Society.
The Manchester Literary and Philosophical Society.
The Yorkshire Philosophical Society.
The Chemical Society of London.
The Museum of Economic Geology.
The United Service Museum.
The Institute of Astronomers, London.
The Leeds Philosophical and Literary Society.

SCOTLAND.

Edinburgh, University Library.
... Advocates' Library.
... College of Physicians.
Edinburgh Highland and Agricultural Society.
... Wernerian Society.
... Royal Medical Society.
... Society of Arts.
Glasgow, University Library.
St Andrews, University Library.
Aberdeen, Library of King's College.

IRELAND.

The Library of Trinity College, Dublin.
The Royal Irish Academy.

COLONIES, &c.

The Asiatic Society of Calcutta.
The Literary and Historical Society of Toronto.

CONTINENT OF EUROPE.

Amsterdam, Royal Institute of Holland.
Berlin, Royal Academy of Sciences.
Berne, Society of Swiss Naturalists.
Bologna, Academy of Sciences.
Bonn, Cæsarean Academy of Naturalists.
Brussels, Royal Academy of Sciences.
Buda, Literary Society of Hungary.
Copenhagen, Royal Academy of Sciences.
Frankfort, the Senkenbergian Museum.
Geneva, Natural History Society.
Göttingen, University Library.
Haarlem, Natural History Society.
Lille, Royal Society of Sciences.
Lisbon, Royal Academy of Sciences.
Munich, Royal Academy of Sciences.
Moscow, Imperial Academy of Naturalists.
Neufchatel, Museum of Natural History.
Paris, Royal Academy of Sciences.
... Geographical Society.
... Royal Society of Agriculture.

Paris, Society for Encouragement of Industry.
... Geological Society of France.
... Ecole des Mines.
... Marine Depôt.
... Museum of Jardin des Plantes.
Rotterdam, Batavian Society of Experimental Philosophy.
Stockholm, Royal Academy of Sciences.
St Petersburg, Imperial Academy of Sciences.
... M. Kupffer, Pulkowa Observatory.
Turin, Royal Academy of Sciences.
Turin, M. Michelotti.

UNITED STATES OF AMERICA.

Boston, the Bowditch Library.
Philadelphia, American Philosophical Society.
Yale College, Professor Silliman.
Washington, the Smithsonian Institution.

(*All the Honorary and Ordinary Fellows of the Society are entitled to the Transactions and Proceedings.*)

The following Institutions and Individuals receive the Proceedings only ;—

ENGLAND.

The Scarborough Philosophical Society.
The Whitby Philosophical Society.
The Newcastle Philosophical Society.
The Geological Society of Cornwall.
The Ashmolean Society of Oxford.
The Literary and Philosophical Society of Liverpool.

SCOTLAND.

The Philosophical Society of Glasgow.

COLONIES.

The Literary and Philosophical Society of Quebec.

CONTINENT OF EUROPE.

Utrecht, the Literary and Philosophical Society.

UNITED STATES.

Professor Dana, Connecticut.
Academy of Natural Science, Philadelphia.

LIST OF DONATIONS.

(Continued from Vol. XVI., p. 648.)

December 3, 1849.

DONATIONS.	DONORS.
Address delivered at the Anniversary Meeting of the Geological Society of London, 16th February 1849. By Sir H. de la Bêche. 8vo.	The Author.
Proceedings of the American Philosophical Society. Vol. v., No. 41. 8vo.	The Society.
Journal of the Statistical Society of London. Vol. xii., Part 2. 8vo.	Ditto.
Scheikundige Onderzoekingen, gedaan in het Laboratorium der Utrechtsche Hoogeschool. 5de Deel, 1ste, 3de, & 4de Stuk. 8vo.	The University.
The American Journal of Science and Arts. Vol. vii., Nos. 21, 22, and 23. 8vo. Edited by Professors Silliman and Dana.	The Editors.
Journal of the Asiatic Society of Bengal. Edited by the Secretaries. New Series, No. 25. 8vo.	Ditto.
Quarterly Journal of the Geological Society of London. Nos. 16 and 18. 8vo.	The Society.
Bulletin de la Société Géologique de France. Tom. xiv., & Tom. i. & ii. 2de Série. 8vo.	Ditto.
Sixteenth Annual Report of the Royal Cornwall Polytechnic Society, 1848. 8vo.	Ditto.
The Journal of Agriculture, and Transactions of the Highland and Agricultural Society of Scotland. No. 25, N.S., July 1849. 8vo.	The Publishers.
The Journal of the Royal Geographical Society of London. Vol. xix., Part 1, 1849. 8vo.	The Society.
Verhandelingen der Eerste Klasse van het Kongeliche Nederlandsche Instituut van Wetenschappen, Letterkunde, en Schoone Kunsten te Amsterdam. 3de Reeks, 1sten Deels, 2de Stuk. 4to.	The Institute.
Tijdschrift voor Wis-en Natuurkundige Wetenschappen, uitgegeven door de Eerste Klasse van het K. Nederlandsche Instituut van Wetenschappen, Letterkunde, en Schoone Kunsten. 2de Deel, 3e & 4e Afleverings. 8vo.	Ditto.
Report of the Eighteenth Meeting of the British Association for the Advancement of Science, held at Swansea, in August 1848. 8vo.	The Association.
Neue Denkschriften der Allgemeine Schweizerischen Gesellschaft für die gesammten Naturwissenschaften. Bde. viii. & ix. 4to.	The Society.
Verhandlungen der Schweizerischen Naturforschenden Gesellschaft bei ihrer Versammlung zu Winterthur 1846 & 1847. 8vo.	Ditto.
Mittheilungen der Naturforschenden Gesellschaft in Bern. Nos. 87–134. 8vo.	Ditto.
Die Wichtigsten Momente aus der Geschichte der drei ersten Jahrzende der Schweizerischen Naturforschenden Gesellschaft. 1848. 8vo.	Ditto.
Antiquités Celtiques et Antediluviennes. Mémoire sur l'Industrie primitive et les arts à leur origine. Par M. Boucher de Perthes. 8vo.	The Author.

DONATIONS.	DONORS.
Meteorologische Beobachtungen angestellt auf Veranstaltung der Naturforschenden Gesellschaft in Zürich. 1837–46. 4to.	The Society.
Denkschrift zur Feier des hundertjahrigen Stiftung festes der Naturforschenden Gesellschaft in Zürich am 30 November 1846. 4to.	Ditto.
Mittheilungen der Naturforschenden Gesellschaft in Zurich. Heft i. (No. 1–13). 8vo.	Ditto.
Proceedings of the American Philosophical Society. Vol. v., January, March, 1849. No. 42. 8vo.	Ditto.
The Progress of the development of the Law of Storms, and of the Variable Winds, with the practical application of the subject to Navigation. By Lieut.-Colonel William Reid. 8vo.	The Author.
On the Geological Structure of the Alps, Apennines, and Carpathians, more especially to prove a transition from Secondary to Tertiary Rocks, and the development of Eocene Deposits in Southern Europe. By Sir Roderick Impey Murchison. 8vo.	Ditto.
Account of the effect of a Storm on Sea-Walls or Bulwarks on the coast near Edinburgh, as illustrating the principle of the construction of Sea-Defences. By W. M. Rankine. 8vo.	Ditto.
An Equation between the Temperature and the maximum elasticity of Steam and other vapours. By W. M. Rankine. 8vo.	Ditto.
Journal of the Asiatic Society of Bengal. Edited by the Secretaries. No. 200, February 1849. 8vo. And N.S., No. 28, April 1849, and No. 203.	Ditto.
Journal of the Statistical Society of London. Vol. xii., Parts 3 and 4. 8vo.	The Society.
Journal of the Geological Society of Dublin. Vol. iv., Part 1. 8vo.	Ditto.
Catalogue of the Calcutta Public Library. 8vo.	The Council.
Flora Batava. 159 Aflevering. 4to.	King of Holland.
A Letter addressed to the Earl of Rosse, President-Elect of the Royal Society. By Marshall Hall, M.D. 8vo.	The Author.
On the Neck as a Medical Region, and on Trachelismus; on Hidden Seizures; on Paroxysmal Apoplexy, Paralysis, Mania, Syncope, &c. By Marshall Hall, M.D. 8vo.	Ditto.
Astronomical Observations made at the Radcliffe Observatory. By Manuel J. Johnson. 1842, 1843, 1844, 1845, 1846, 1847. Vol. iii.–viii. 8vo.	Radcliffe Trustees.
Journal of the Indian Archipelago and Eastern Asia. Vol. iii., Nos. 1, 2, 3, 4. 8vo.	The Editor.
Memoirs of the Ganglia and Nerves of the Uterus. By Robert Lee, M.D. 4to.	The Author.
On the Ganglia and Nerves of the Heart. By Robert Lee, M.D. 4to.	Ditto.
Athenæum.—Rules and Regulations, List of Members, &c. 1847. 12mo.	The Athenæum.
———— Annual Report—General Abstract of Accounts. 1848.	Ditto.
Description of a Machine for Polishing Specula, with Directions for its use. By W. Lassell, Esq. 4to.	The Author.

December 17, 1849.

The Astronomical Journal. Vol. i., No. 1. 4to.	The Editor.
Twenty-Ninth Report of the Council of the Leeds Philosophical and Literary Society. 1848–49. 8vo.	The Society.
Smithsonian Contributions to Knowledge. Vol. i. Published by the Smithsonian Institution. 4to.	The Institution.
Report, &c., of Smithsonian Institution. 1849. 8vo.	Ditto.
Fauna Antiqua Sivalensis, being the Fossil Zoology of the Sewalik Hills, in the North of India. By Hugh Falconer, M.D., and Proby T. Cautley, F.G.S. Parts i. and ix. Fol.	The Authors.
Do. do. Letterpress. Part i. 8vo.	Ditto.
United States Exploring Expedition during the years 1838, 1839, 1840, 1841, and 1842, under the command of Charles Wilkes, U.S.N. Atlas. Zoophytes. By James D. Dana, A.M. Imp. Fol.	Ditto. Ditto.

DONATIONS.	DONORS.
Astronomical Observations made at the Royal Observatory, Greenwich, in the year 1847, under the direction of George B. Airy, Esq. 4to.	The Observatory.
Philosophical Transactions of the Royal Society of London for the year 1849. Parts i. and ii. 4to.	The Society.
List of Fellows, &c., of the Royal Society, 30th November 1848. 4to.	Ditto.
Proceedings of the Royal Society. 1848. Nos. 71 and 72. 8vo.	Ditto.

January 7, 1850.

The Phenomena Diosemeia of Aratus, translated into English verse, with Notes. By John Lamb, D.D. 8vo.	The Author.
Abstract of Exposition on the Strength of Materials. Read before the Royal Scottish Society of Arts at the request of the Council. By George Buchanan, F.R.S.E. 8vo.	Ditto.
Sopra alcuni punti della Teoria del Moto dei Liquidi. Memoria del Prof. P. Tardy. 4to.	Ditto.
Annalen der Königliche Sternwarte bei München, herausgegeben von Dr J. Lamont. Bde. 1 & 2. 8vo.	The Observatory.
Journal of the Asiatic Society of Bengal. Edited by the Secretaries. Nos. 204 and 205. 8vo.	The Editors.
Journal of Agriculture, and Transactions of the Highland and Agricultural Society of Scotland. No. 27, N.S., January. 8vo.	The Society.
Mémoires de l'Académie Royale des Sciences, &c., de Belgique. Tom. xxviii. 4to.	The Academy.
Annuaire del'Académie Royale des Sciences, &c., de Belgique. Tom. xv., 2^{me} partie. Tom. 16^{me}, 1re partie. 8vo.	Ditto.

January 21, 1850.

Annuaire Magnétique et Météorologique du Corps des Ingénieurs des Mines; ou Recueil d'Observations Météorologiques et Magnétiques faites dans l'entendue de l'empire du Russie, par A. T. Kupffer. Nos. 1 & 2, 1849. 4to.	The Russian Government.
Verhandelingen der Eerste Klasse van het Kongeliche Nederlandsche Instituut van Wetenschappen, Letterkunde, en Schoone Kunsten te Amsterdam. 3de Reeks, Deel 1, Stuk 3 en 4. 4to.	The Academy.
Tijdschrift voor de Wis-en Natuurkundige Wetenschappen uitgegeven door de Eerste Klasse van het Kongeliche Nederlandsche Instituut van Wetenschappen te Amsterdam. 3de Deel, 1 & 2 Afleverings. 8vo.	Ditto.
Jaarboek van het Kongeliche Nederlandsche Instituut van Wetenschappen, Letterkunde, en Schoone Kunsten te Amsterdam, 1847, 1848, 1849. 8vo.	Ditto.
Catalogue of 2156 Stars, formed from the Observations made during Twelve Years, from 1836 to 1847, at the Royal Observatory, Greenwich. 4to.	The Royal Society, London.
Proceedings of the Philosophical Society of Glasgow, 1848–9. Vol. iii., No. 1. 8vo.	The Society.
Quarterly Journal of the Chemical Society of London. No. 8. 8vo.	Ditto.
Proceedings of the Royal Astronomical Society. Vol. x., No. 2. 8vo.	Ditto.

February 4, 1850.

Philosophical Transactions of the Royal Society of London, 1849. Part II. 4to.	The Society.
Kongl. Vetenskaps Akademiens Handlingar, för 1847 and 1848. 8vo.	The Academy.
Årsberättesler om Botaniske Arbeten och Uptäckter för 1843 and 1844. 8vo.	Ditto.
Årsberättelse om Framstegen i Kemi under År. 1847. 8vo.	The Academy.
Årsberättelse om Technologiens Framsteg. 1842, 1843, 1844, 1846. 8vo.	Ditto.
Öfversigt af Kongl. Vetenskaps. Akademiens Forhandlingar. 1848. 8vo.	Ditto.

February 18, 1850.

DONATIONS.	DONORS.
The London University Calendar. 1850. 12mo.	The Publishers.
The American Journal of Science and Arts. Conducted by Professors Silliman and Dana. Vol. ix., No. 25. 8vo.	The Editors.
Mémoires de l'Académie Impériale des Sciences de St Pétersbourg. Sixième Série. Sciences Mathématiques, Physiques et Naturelles. Tome viii^{me}, 2^{me} partie. Sciences Naturelles. Livraisons 3^{me}, 5^{me}, et 6^{me}. 4to.	The Academy.
Mémoires présentés à l'Académie Impériale des Sciences de St Pétersbourg, par divers Savants et lus dans ses Assemblées. Tome vi^{me}. Livraisons 2^{me} et 3^{me}. 4to.	Ditto.
Messungen zur Bestimmung des Höhenunterschiedes zurischen dem Schwarzen und Caspischen Meere. Von G. Fuss, Sawitsch, und Sabler. 4to.	The Authors.
Rapport fait à l'Académie Impériale des Sciences de St Pétersbourg, par W. Struve, sur une mission scientifique dont il fut chargé en 1847. 4to.	The Author.
W. Struve sur la Dilatation de la Glace d'après les expériences faites en 1845 et 1846 à l'Observatoire Central de Poulkova, par MM. Schumacher, Pohrt, et Moritz. 4to.	The Authors.
Über Prof. Mädlers Untersuchungen über die Eigenen Beweyungen der Fixsterne, von C. A. F. Peters, Dr. 4to.	The Author.
P. H. Fuss Nachricht über eine Sammlung Unedirter Handschriften Leonhard Eulers, & über die Begonnene gesammtausgabe seiner Ueineren schriften. 8vo.	Ditto.
Über die Genanig-keit der in Lalandes Catalog, publicirt von der *British Association*, enthaltenen Sternörter. Von Dr Lindhagen. 8vo.	Ditto.
Verhandlungen der Schweizerischen Naturforschenden Gesellschaft bei ihrer Versammlung zu Slothurn. 1848. 8vo.	The Society.
Mittheilungen der Naturforschenden Gesellschaft in Bern, aus dem Jahre, 1848–9. Nos. 135–161. 8vo.	Ditto.

March 4, 1850.

Transactions of the Cambridge Philosophical Society. Vol. viii. 4to.	The Society.
The Astronomical Journal. Vol. i., Nos. 2, 3, and 4. 4to.	The Editor.
Proceedings of the Royal Astronomical Society. Vol. x., No. 3. 8vo.	The Society.
Proceedings of the Linnæan Society of London. Nos. 30–40. 8vo.	Ditto.
Charter and Bye-Laws of the Linnæan Society. 1848. 8vo.	Ditto.
List of the Linnæan Society. 1849. 4to.	Ditto.
Journal of Agriculture, and Transactions of the Highland and Agricultural Society of Scotland. No. 28, N.S. 8vo.	Ditto.
Annales des Sciences Physiques et Naturelles, d'Agriculture et d'Industrie, publiées par la Société Nationale d'Agriculture, &c., de Lyon. Tom. ii. 1848. 8vo.	Ditto.
A Collection of Maritime Charts, with corresponding Descriptions.	French Government.

March 18, 1850.

Some Account of the last Yellow Fever Epidemic of British Guiana. By Daniel Blair, M.D. Edited by John Davy, M.D., F.R.S.L. & E. 8vo.	The Author.
Das peripherische Nervensystem der Fische, Anatomisch und Physiologisch untersucht. Von Dr Hermann Stannius. 4to.	Ditto.
Neue Denkschriften der Allg. Schweizerischen Gesellschaft für die gesammten Naturwissenschaften. Bd. x., mit. xiii. Tafeln. 4to.	The Society.
On the Diffusion of Liquids. By Thomas Graham, Esq., F.R.S., F.C.P. 4to.	The Author.
Description of the Instruments and Process used in the Photographic Self-registration of the Magnetical and Meteorological Instruments at the Royal Observatory, Greenwich. 4to.	The Astronomer-Royal.

DONATIONS.	DONORS.
Proceedings of the Royal Astronomical Society. Vol. x., No. 4. 8vo.	The Society.
Description of the Observatory at Cambridge, Massachusetts. By William Cranch Bond. 4to.	The Observatory.
Astronomical Observations made at Cambridge Observatory, Massachusetts, 1847–8. 8vo.	Ditto.

April 1, 1850.

Magnetical and Meteorological Observations made at the Royal Observatory, Greenwich, 1847. 4to.	The Observatory.
Journal of the Statistical Society of London. Vol. xiii., Part 1. 8vo.	The Society.
Deuxième Mémoire sur le Daltonisme, ou la Dyschromatopsie. Par E. Wartmann. 4to.	The Author.
The Accommodation of the Eye to Distances. By William Clay Wallace, M.D. 8vo.	Ditto.
Transactions of the Zoological Society of London. Vol. iii., Parts 5 and 6. 4to.	The Society.
Proceedings of Do. Parts 15 and 16. 8vo.	Ditto.
Reports of Council of Do. 1849. 8vo.	Ditto.

April 15, 1850.

Transactions of the Royal Scottish Society of Arts. Vol. iii., Part 4. 8vo.	The Society.
Journal of the Asiatic Society of Bengal. Edited by the Secretaries. N.S., No. 32. 8vo.	Ditto.
Annali di Fisica dell' Abbate Francesco Car. Zantedeschi. Fascicolo 4. 8vo.	The Author.
Quarterly Journal of the Chemical Society. No. 9. 8vo.	The Society.
Scheikundige Onderzoekingen gedaan in het Laboratorium der Utrechtsche Hoogeschool. 5de Deel. 6de Stuk. 8vo.	The University.
Bulletin de la Société de Géographie. 3^{me} Série. Tom. 12^{me}, 1849. 8vo.	The Society.

December 2, 1850.

Essai de Phytostatique appliqué à la Chaine du Jura et aux contrées voisines. Par M. Thurmann. 2 Tom. 8vo.	The Author.
The American Journal of Science and Arts. Conducted by Professors Silliman and Dana. Vol. ix., No. 26; Vol. x., Nos. 28 and 29. 8vo.	The Editors.
Annals of the Lyceum of Natural History of New York. Vol. v., No. 1; Vol. iv., No. 12. 8vo.	The Lyceum.
Journal of the Asiatic Society of Bengal. Edited by the Secretaries. Nos. 207 and 212. 8vo.	The Society.
Memorie della R. Accademia delle Scienze di Torino. Serie 2da, Tom. x. 4to.	The Academy.
Journal of the Statistical Society of London. Vol. xiii., Part 2. 8vo.	The Society.
Proceedings of the American Philosophical Society. Vol. v., No. 44. 8vo.	Ditto.
Proceedings of the Royal Society. 1849. Nos. 73 and 74. 8vo.	Ditto.
Memoirs of the American Academy of Arts and Sciences. N. S. Vol. iv., Part 1. 4to.	The Academy.
Proceedings of the Royal Astronomical Society. Vol. x., No. 7. 8vo.	The Society.
Quarterly Journal of the Chemical Society. No. 10. 8vo.	Ditto.
Report of the 19th Meeting of the British Association for the Advancement of Science. 1849. 8vo.	The Publisher.
Scientific Researches, Experimental and Theoretical, in Electricity, Magnetism, Galvanism, Electro-Magnetism, and Electro-Chemistry. By William Sturgeon. 4to.	The Author.
Journal of Agriculture, and Transactions of the Highland and Agricultural Society of Scotland. Nos. 29 and 30, N. S., 1850. 8vo.	The Society.
Astronomical, Magnetical, and Meteorological Observations made at the Royal Observatory, Greenwich, 1848. 4to.	The Observatory.
Medico-Chirurgical Transactions, published by the Medico-Chirurgical Society of London. Vol. xxxiii. 8vo.	The Society.

DONATIONS.	DONORS.
An Enquiry into M. Antoine d'Abadie's Journey to Kaffa, to discover the source of the Nile. By C. T. Beke. 8vo.	The Author.
Jahrbuch der Kaiserlich-Königlichen Geologischen Reichsenstalt. 1850. No. 1. Jan. Feb. Marz. 8vo.	The Institute.
Philosophical Transactions of the Royal Society of London. 1850. Part 1. 4to.	The Society.
Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften. Wien. 1848–50. 8vo.	The Academy.
Case of Catalepsy, with Remarks. By James Stark, M.D. 8vo.	The Author.
Two Cases of Rupture of the Crucial Ligaments of the Knee-Joint. By James Stark, M.D. 8vo.	Ditto.
Journal of the Royal Asiatic Society of Great Britain and Ireland. Vol. xii., Part 2. 8vo.	The Society.
La Thermacrose, ou la Coloration Calorifique. Par M. Melloni. 8vo.	The Author.
On the Pelorosaurus; an undescribed gigantic terrestrial reptile whose remains are associated with those of the Iguanodon, &c. On a Dorsal Dermal Spine of the Hylæosaurus, recently discovered in the Strata of Tilgate Forest, Sussex. By G. A. Mantell, LL.D. 4to.	Ditto.
Supplementary Observations on the Structure of the Belemnite and Belemnostenthis. By G. A. Mantell, LL.D. 4to.	Ditto.
Quarterly Journal of the Chemical Society. October 1850, No. 11. 8vo.	The Society.
Collection of French Admiralty Charts.	French Government.
Proceedings of the Philosophical Society of Glasgow. 1849–50. Vol. iii., No. 2. 8vo.	The Society.
Bulletin de la Société Impériale des Naturalistes de Moscou. 1847, No. 3; 1848, Nos. 1 and 2. 8vo.	Ditto.
Flora Batava. Parts 163 and 164. 4to.	King of Holland.
Journal of the Royal Geographical Society of London. Vol. xx., Part 1. 8vo.	The Society.
Bulletin de la Société de Géographie. 3^{me} Serie. Tom. xiii. 8vo.	Ditto.
Gelehrte Anzeigen herausg. von Mitgliedern der K. Bayerischen Akademie der Wissenschaften. Bde. 28 and 29. 4to.	The Academy.
Det Kongelike Dánske Videnskab. Selskabs Skrifter. Femte Række. Naturvidenskabelig og Mathematisk Afdeling. 1te Bd. 4to.	The Society.
Astronomical Observations made at the Royal Observatory, Edinburgh, by the late T. Henderson, Esq. Vol. ix. 1843. 4to.	The Observatory.
Results of the Observations made by Rev. F. Fallows, at the Royal Observatory, Cape of Good Hope, in the years 1829, 1830, 1831. Reduced under the superintendence of G. B. Airy, Esq. 4to.	The Editor.
Abhandlungen über das Wesen der Imponderabilien. Von L. Ph. Wüppermann. 1^{r} Theil. 1^{e} Abtheil. 8vo.	The Author.
Abhandlungen der Philosophisch-Philologischen Classe der Königliche Bayerischen Akademie der Wissenschaften. Bd. 5. Abtheil. 3. 4to.	The Academy.
Abhandlungen der Mathematisch-Physikalischen Classe der Königliche Bayerischen Akademie der Wissenschaften. Bd. 5. Abtheil. 3. 4to.	Ditto.
Ueber den Anthiel der Pharmacie an der Entwicklung der Chemie. Von Dr Ludwig A. Buchner jun. 4to.	The Author.
Archives du Muséum d'Histoire Naturelle. Tom. iv., Livraisons 3 and 4. 4to.	The Museum.

December 16, 1850.

Journal of the Statistical Society of London. Vol. xiii., Part 3. 8vo.	The Society.
Natuurkundige Verhandelingen van de Hollandsche Maatschappij der Wetenschappen te Haarlem. Diet. 5 and 6. 4to.	Ditto.
Astronomische Beobachtungen auf der K. Universitäts Sternwarte in Königsberg. Herausgegeben von A. L. Busch. Abtheil. 29. Folio.	The Observatory.
Observations made at the Magnetical and Meteorological Observatory at Hobarton, in Van Diemen Island, and by the Antarctic Naval Expedition. Vol. i. 1841. 4to.	Ditto.

DONATIONS.	DONORS.
Proceedings of the American Philosophical Society. Vol. v., No. 44. 8vo.	The Society.
Proceedings of the Zoological Society of London. Nos. 178–189. 8vo.	Ditto.
Proceedings of the Royal Society. Nos. 73 and 74. 8vo.	Ditto.
Seventeenth Annual Report of the Royal Cornwall Polytechnic Society. 1849. 8vo.	Ditto.
Journal of the Asiatic Society of Bengal. N. S. No. 37. 8vo.	Ditto.
Letter to the Right Honourable Lord Brougham and Vaux, containing proposals for a Scientific Exploration of Egypt and Ethiopia. By John James Wild. 8vo.	The Author.
The Accommodation of the Eye to Distances. By William Clay Wallace, M.D. 8vo.	Ditto.
Oversigt over det Kgl. Dánske Videnskabernes Selskabs Forhandlinger og dets Medlemmers Arbeider i Aarets 1847 og 1848. 8vo.	The Society.
Verhandelingen der Eerste Klasse van het K. Nederlandsche Instituut, &c. 3de Reeks. ii. and iiie Deel. 4to.	The Institute.
Jaarboek van het K. Nederlandsche Instituut, &c. Voor 1850. 8vo.	Ditto.
Tijdschrift voor de Wis-En Natuurkundige Wetenschappen, uitgegeven door de 1ste Klasse van het K. Nederlandsche Instituut. 3de Deel. 4e Aflevering. 8vo.	Ditto.
Kongl. Vetenskaps Akademiens Handlingar under Sednare Hälften. 1848. 8vo.	The Academy.
Öfversigt af K. Vetenskaps-Akademiens Förhandlingar. 1849. Nres 1–9. 8vo.	Ditto.
Arsberättelse om Framstegen i Kemi under är 1848. Afgifen till K. Vetenskaps-Akademien af L. F. Svanberg. 8vo.	Ditto.
Medallion of Berzelius.	Ditto.
Mémoires de l'Académie Impériale des Sciences de St Pétersbourg. Sciences Mathématiques, Physiques et Naturelles. Tomes 7me and 8me. 4to.	Ditto.
Mémoires présentés à l'Academie Impériale des Sciences de St Petersbourg. Tome 6me. Livraison 4me. 4to.	Ditto.
Recueil des Actes des Séances publiques de l'Academie Impériale des Sciences de St Pétersbourg, tenues le 28 Decembre 1847 et le 29 Decembre 1848. 4to.	Ditto.
Explication de la Carte Géologique de la France, rédigée par MM. Dufrénoy et Elie de Beaumont. Tomes 1 and 2. 4to.	French Government.
Geological Map of France.	Ditto.

January 20, 1851.

Proceedings of the Academy of Natural Science of Philadelphia. Vol. v., No. 5. 8vo.	The Academy.
The American Journal of Science and Arts. 2d Series, No. 30. 8vo.	The Editors.
Proceedings of the Royal Astronomical Society of London. Vol. ii., No. 2. 8vo.	The Society.
Résumé Météorologique de l'année 1849, pour Genève et le Grand St Bernard. Par E. Plantamour. 8vo.	The Author.
Reasons for returning the Gold Medal of the Geographical Society of France, and of withdrawing from its Membership. In a Letter to M. De la Roquette, from Charles T. Beke. 8vo.	Ditto.
Astronomical and Magnetical and Meteorogical Observations made at the Royal Observatory, Greenwich, in the year 1849. 4to.	The Observatory.
Greenwich Magnetical and Meteorological Results. 1848. 4to.	Ditto.
Astronomical Observations made at the Observatory of Cambridge. Vol. xvi., 4to.	Ditto.
Transactions of the Cambridge Philosophical Society. Vol. ix., Part 1. 4to.	The Society.

February 3, 1851.

Proceedings of the Royal Astronomical Society of London. Vol. ii., No. 1. 8vo.	The Society.
On the Cyclone of November 19 (1850). By the Rev. Humphrey Lloyd, D.D. 8vo.	The Author.
On the Induction of Soft Iron, as applied to the determination of the Changes of the Earth's Magnetic Force. By the Rev. Humphrey Lloyd, D.D. 8vo.	Ditto.
Instructions for making Meteorological and Tidal Observations. Prepared by the Council of the Royal Irish Academy. 8vo.	The Academy.
Second Report of the Council of the Royal Irish Academy, relative to the establishment of a System of Meteorological and Tidal Observations in Ireland. 8vo.	Ditto.
The London University Callendar. 1851. 12mo.	The Publishers.

February 17, 1851.

DONATIONS.	DONORS.
Essai Historique sur le Magnétisme et l'Universalité de son influence dans la Nature. Par M. de Haldat. 8vo.	The Author.
Optique Oculaire suivie d'un Essai sur l'Achromatisme de l'Oeil. Par M. de Haldat. 8vo.	Ditto.
On the Remains of Man, and Works of Art imbedded in Rocks and Strata, as illustrative of the connection between Archæology and Geology. By G. A. Mantell, LL.D. 8vo.	Ditto.
American Journal of Science and Arts. Vol. ii., No. 31. 8vo.	The Editors.
Acta Societatis Scientiarum Fennicæ. Tom. iii., Fasciculus 1. 4to.	The Society.
Novorum Actorum Academiæ Cæsareæ Leopold.-Carol. Naturæ Curiosarum. Vol. xxii., Pars ii. 4to.	The Academy.
Abhandlungen der K. Akademie der Wissenschaften zu Berlin. 1848. 4to.	Ditto.
Monatsbericht der K. Akademie der Wissenschaften zu Berlin. Juli 1849; Juni 1850. 8vo.	Ditto.
French Marine Charts, with corresponding Descriptions.	French Government.
Ueber eine Kochsalz herrührende pseudomorphische Bildung im Muschelkalke der Wifergegend. Von J. F. L. Hausmann. 8vo.	The Author.
Die Bleigewinnung in Südlichen Spanien in Jahre 1829. Von J. F. L. Hausmann. 8vo.	Ditto.
Ueber die Erscheinung des Anlaufens der Mineralkörper. Von J. F. L. Hausmann. 8vo.	Ditto.
Nachrichten von der Georg. Augusts. Universität. und der K. Gesellschaft der Wissenschaften zu Göttingen. Von Jahre 1849. Nr. 1–14. 12°.	The University.

March 3, 1851.

Medico-Chirurgical Transactions. Published by the Royal Medical and Chirurgical Society of London. General Index. Vols. i.–xxxiii. 8vo.	The Society.
The Journal of Agriculture, and Transactions of the Highland and Agricultural Society of Scotland. New Series. No. 32. 8vo.	Ditto.

March 17, 1851.

Philosophical Transactions of the Royal Society of London, for the year 1850. Part 2. 4to.	The Society.
Observations on Days of unusual Magnetic Disturbance, made at the British Colonial Magnetic Observatories, under the departments of the Ordnance and Admiralty. Vol. i., Part 2 (1842–4). 4to.	The British Government.
Annales des Mines. Tom. ii. (1847); Tom. iv., Liv. 1, 5, 6 (1833); Table des Matières des 1re et 2e Séries, 1816–30; Tom. xiv., Liv. 6 (1848); Tom. xix., Liv. 1, 2, 3, (1841); Tom. xx., Liv. 4, 5, 6 (1841). 8vo.	The Ecole des Mines.
Journal of the Statistical Society of London. Vol. xiv., Part 1. 8vo.	The Society.
The Geological Observer. By Sir Henry T. de la Bèche. 8vo.	The Author.
Journal of the Asiatic Society of Bengal. No. 214. 8vo.	The Society.

April 7, 1851.

Primo Decennio di Osservazioni Meteorologiche fatto nella Specula di Bologna, ridotte esposte ed applicate da Alessandro Palagi, M.D. 4to.	The Author.
Neue Denkschriften der Allgemeine Schweizerischen Gesellschaft für die gesammten Naturwissenschaften. Bd. 11. 4to.	The Society.

DONATIONS.	DONORS.
Mittheilungen der Naturforschenden Gesellschaft in Bern. Nos. 144–192. 8vo.	The Society.
Verhandlungen der Schweizerischen Naturforschenden Gesellschaft bei ihrer 35 Versammlung in Aarau. 1850–1. 8vo.	Ditto.
Verhandlungen der Schweizerischen Naturforschenden Gesellschaft bei ihrer 34 Versammlung in Frauenfeld. 1849. 8vo.	Ditto.
Naturwissenschaftliche Abhandlungen gesammelt und durch subscription herausgegeben von. W. Haidinger. Bde. 2, 3. 4to.	The Editor.
Berichte über die Mittheilungen von Freunden der Wissenschaften in Wien. Herausg. von W. Haidinger. Bde. 3, 4, 5, 6. 8vo.	Ditto.
Contributions to the Vital Statistics of Scotland. By James Stark, M.D. 8vo.	The Author.
Journal of the Asiatic Society of Bengal. Nos. 215 and 216. 8vo.	The Society.
Mémoires de l'Institut de France. Académie des Sciences. Tom. 20, 21, 22. 4to.	The Academy.
Mémoires présentés par divers Savants à l'Académie des Sciences de l'Institut National de France. Tom. 11, 12. 4to.	Ditto.
Collection of Specimens illustrating the Geology of the Eildon Hills.	Professor Forbes.

April 21, 1851.

Journal of the Royal Geographical Society of London. Vol. xx., Part 2. 1851. 8vo.	The Society.
Supplement to the Catalogue of the Athenæum Library. 8vo.	The Athenæum.
Abhandlungen der Philosophisch-Philologischen Classe der K. Bayerischen Akademie der Wissenschaften. Bd. vi., Abtheil 1. 4to.	The Academy.
Abhandlungen der Historichen Classe der K. Bayerischen Akademie der Wissenschaften. Bde. i.–vi., Abtheil. 1. 4to.	Ditto.
Gelehrte Anzeigen herausgegeben von Mitgliedern der K. Bayerischen Akademie der Wissenschaften. Bde. xxx., xxxi. 4to.	Ditto.
Almanach der K. Bayerischen Akademie der Wissenschaften, für 1849. 12mo.	Ditto.
Annalen der Königlichen Sternwarte bei München. Bd. iv. 8vo.	The Observatory.
Abhandlung über das Schul. und Lehrwesen der Muhamedaner im Mittelalter. Von Dr D. Haneberg. 4to.	The Author.
Ueber die Praktische Seite Wissenschaftlicher Thätigkeit. Von Fr. v. Thiersch. 4to.	Ditto.
Einige Worte über Wallensteins Schuld. Von Dr Rudhart. 4to.	Ditto.
Ueber die Politische Reformbewegung in Deutschland im xv. Jahrhunderte und den Antheil Bayerns an derselben. Von Dr Const. Höfler. 4to.	Ditto.
Bulletin de la Société de Géographie. 3me Série. Tom. xiv. 8vo.	The Society.
The American Journal of Science and Arts. Vol. ii., No. 32. 8vo.	The Editors.
Experimental Researches on Electricity. By Michael Faraday, LL.D. 4to.	The Author.

December 1, 1851.

Natuurkundige Verhandelingen van de Hollandsche Maatschappij der Wetenschapen te Haarlem. Tweede Versameling, 7 Deel. 4to.	The Society.
An Essay explanatory of the Tempest Prognosticator, in the building of the Great Exhibition for the Works of Industry of all Nations. By George Merryweather, M.D. 8vo.	The Author.
Letters to a Candid Inquirer on Animal Magnetism. By W. Gregory, M.D. 12mo.	Ditto.
Flora Batava. 165 Aflevering. 4to.	King of Holland.
Astronomical Observations made at the Radcliffe Observatory, Oxford, in the year 1848. By M. J. Johnson. Vol. ix. 8vo.	Radcliffe Trustees.
Astronomical Observations made at the Radcliffe Observatory, Oxford, in the year 1849. By M. J. Johnson. Vol. x. 8vo.	Ditto.
Proceedings of the Zoological Society of London, 1835, 1836, 1837, 1838, 1840, 1841, 1844, 1845, 1846. 8vo.	The Society.

DONATIONS.	DONORS.
Reduction of the Observations of Planets, made at the Royal Observatory, Greenwich, from 1750 to 1830, under the superintendence of G. B. Airy, Esq. 4to.	The Observatory.
Reduction of the Observations of the Moon, made at the Royal Observatory, Greenwich, from 1750 to 1830, under the superintendence of G. B. Airy, Esq. 2 vols. 4to.	Ditto.
Catalogue of 2156 Stars, formed from the Observations made during the twelve years from 1836 to 1847, at the Royal Observatory, Greenwich, 4to.	Ditto.
Results of the Observations made at the Royal Observatory, Greenwich, 1847, 1848, 1849. 4to.	Ditto.
Results of the Magnetical and Meteorological Observations made at the Royal Observatory, Greenwich, 1848 and 1849. 4to.	Ditto.
Descriptions of the Instruments and Process used in the Photographic Self-Registration of the Magnetical and Meteorological Instruments at the Royal Observatory, Greenwich. 4to.	Ditto.
Account of Improvements in Chronometers, made by Mr John J. Giffe. 4to.	Ditto.
Papers and Proceedings of the Royal Society of Van Diemen's Land. Vol. i., Parts 1 and 2. 8vo.	The Society.
Astronomische Beobachtungen auf der Königliche Universitäts Sternwarte in Königsberg. Herausgegeben von H. L. Busch. Abtheil. 23. Fol.	The Observatory.
Abhandlungen der Königliche Gesellschaft der Wissenschaften zu Göttingen. Band. 4. 4to.	The Society.
Nachrichten von der Georg-Augusts Universität und der Königliche Gesenschaft der Wissenschaften zu Göttingen, 1850. Nos. 1–17. 12mo.	Ditto.
Beiträge zur Metallurgischen Krystallkunde. Von J. F. L. Hausmann. 4to.	The Author.
Handbuch der Mineralogie. Von J. F. L. Hausmann. 1er. Theil. 8vo.	Ditto.
Plan and description of the Original Electro-Magnetic Telegraph. By W. Alexander, Esq. 8vo.	Ditto.
Minutes of Proceedings of the Institution of Civil Engineers, containing Abstracts of the Papers and of the Conversations. Vol. i.–viii. (1837–50). 8vo.	The Institution.
Catalogue of the Library of the Institution of Civil Engineers. 8vo.	Ditto.
Journal of the Statistical Society of London. Vol. xiv., Part 2. 8vo.	The Society.
Journal of the Horticultural Society of London. Vols. i., ii., iii., iv., v., and vi. Parts 2 and 3. 8vo.	Ditto.
Notice sur les Altitudes du Mont Blanc et du Mont Rose, determinées par des Mesures Barométriques et Géodésiques. Par M. le Commandant Deleros. 8vo.	The Author.
The American Journal of Science and Arts. 2d Series. Vol. ii. No. 33. 8vo.	The Editors.
Journal of the Asiatic Society of Bengal. Nos. 217 and 218. 8vo.	The Society.
Archives du Muséum d'Histoire Naturelle, publiées par les Professeurs-Administrateurs de cet Etablissement. Tom. v., Liv. 1 & 2 (Paris). 4to.	The Museum.
Quarterly Journal of the Chemical Society, No. 14. 8vo.	The Society.
Verzangenheit und Zunkunft der Kaiserlichen Leopoldinisch-Carolinischen Akademie der Naturforscher. Von Dr C. G. Nees v. Esenbeck. 4to.	The Author.
Compendium der Populären Mechanik und Maschinenlehre. Von Adam Ritter von Burg. 8vo.	Ditto.
Compendium der Höheren Mathematik. Von Adam Ritter von Burg. 8vo.	Ditto.
Ueber die von dem Civil. Ingénieur Herrn Kohn, angestellten Versuche um den Einfluss oft wiedénholter Torsionen auf den Molekularzustand des Schmiedeisens auszumitteln. Von A. von Burg. 8vo.	Ditto.
Programm für die Ordentlichen und Ausserordtlichen Vorlesungen welche am K. K. Polytechnischen Institut zu Wien im Studienjahre. 1850–51. Staat finden werden. Von A. von Burg. 4to.	Ditto.
Kuppfertafeln zum Compendium des Populären Mechanik und Maschinenlehre. Von A. von Burg. 4to.	Ditto.
Eighteenth Annual Report of the Royal Cornwall Polytechnic Society. 1850. 8vo.	The Society.
Théorie Mathématique des Oscillations des Baromètre, et recherches de la loi de la variation moyenne de la Température avec la Latitude. Par M. E. Liais. 8vo.	The Author.
Astronomical Observations, made during the year 1846, at the National Observatory, Washington. Vol. ii. 4to.	The Observatory.

DONATIONS.	DONORS.
Annales de l'Observatoire Physique Central de Russie, publiées par A. T. Kupffer. 1847. Nos. 1, 2. 4to.	The Observatory.
Memorias de la Real Academia de Ciencias de Madrid. Tomo 1°, 1ª Partie. 4to.	The Academy.
Resumen de las Actas de la Academia Real de Ciencias de Madrid, en al año Academico de 1849 & 1850.	Ditto.
Contributions to Astronomy and Geodesy. By Thomas Maclear, Esq., F.R.A.S. 4to.	The Author.
Verhandelingen der Eerste Klasse van het Koninklijk-Nederlandsche Instituut van Wetenschappen, Letterkunden, en Schoone Kunsten te Amsterdam. 3th Reeks, 4th Deel. 4to.	The Institute.
Tijdschrift voor de Wis-En Natuurkundige Wetenschappen, uitgegeven door de Eerste Klasse van het Koninklijk-Nederlandsche Instituut van Wetenschappen, Letterkunde, en Schoone Kunsten te Amsterdam. 4th Deel. 8vo.	Ditto.
Memoires of the Royal Astronomical Society. Vol. xix. 4to.	The Society.
Transactions of the Microscopical Society of London. Vol. iii., Parts 1 & 2. 8vo.	Ditto.
Journal of Agriculture, and Transactions of the Highland and Agricultural Society of Scotland. N. S. No. 34. 8vo.	Ditto.
Journal of the Statistical Society of London. Vol. xiv., Part 2. 8vo.	Ditto.
Quarterly Journal of the Chemical Society. No. 15. 8vo.	Ditto.
Papers and Proceedings of the Royal Society of Van Diemen's Land. Vol. i., Part 3. 8vo.	Ditto.
American Journal of Science and Arts. Vol. xii., Nos. 34 and 35. 8vo.	The Editors.
Journal of the Asiatic Society of Bengal. N. S. No. 45. 8vo.	The Society.
Proceedings of the Liverpool Literary and Philosophical Society. Sessions 38 and 39. No. 6. 8vo.	Ditto.
Archives du Muséum d'Histoire Naturelle, publiées par les Professeurs-Administrateurs de cet Etablissement. Tom. v., 3me Liv. 4to.	The Museum.
Muséum d'Histoire Naturelle de Paris. Catalogue Méthodique de la Collection des Reptiles. 1re Liv. 8vo.	Ditto.
——— Catalogue de la Collection Entomologique. Classe des Insectes ordre Coléoptères. 1re et 2me Liv. 8vo.	Ditto.
Transactions of the Linnæan Society. Vol. xx., Parts 2 and 3. 4to.	The Society.
Proceedings of Do. Do. Nos. 41, 42, 43, 44. 8vo.	Ditto.
List of Fellows of the Linnæan Society. 1850. 4to.	The Society.
Bericht über die in Jahren 1848 und 1849 auf den Stationen des Meteorologischen Instituts in Preussischen Staate angestellten Beobachtungen. Von H. W. Dove. Fol.	The Author.
Observations made at the Magnetical and Meteorological Observatory at the Cape of Good Hope. Vol. i., Magnetical Observations, 1841 to 1846. 4to.	The British Government.
Journal of the Horticultural Society of London. Vol. vi., Part 4. 8vo.	The Society.
Journal of the Asiatic Society of Bengal, 1851. No. 4. 8vo.	Ditto.
Mémoires de l'Académie Impériale des Sciences de St Pétersbourg. vime Série. Sciences Mathématiques, Physiques, et Naturelles. Tom. 6me, 1re Partie. Sciences Mathématiques et Physiques. Tom. ivme, Liv. 3 and 4. 4to.	The Academy.
Mémoires présentés à l'Académie Impériale des Sciences de St Petersbourg, par divers Savants et lus dans ses Assemblées. Tom. vime, Liv. 5 & 6. 4to.	Ditto.
Observations faites à Nigré-Taguilsk (Monts Oural), Gouvernement de Perm. Années 1848 et 1849. (1850.) 8vo.	The Observatory.
Proceedings of the American Philosophical Society. Vol. v., Nos. 45 and 46. 8vo.	The Society.
Mémoires sur le Digitaline. Par MM. Homolle et Quevenne. 8vo.	The Authors.
On the Silurian Rocks of the South of Scotland. By Sir R. I. Murchison. 8vo.	Ditto.
Three Letters to the Inhabitants of Ceylon, on the Advantages of Vaccination. By John Kinnis, M.D. 8vo.	Ditto.
Contributions to the Military Medical Statistics of China. By John Kinnis, M.D. —On the Military Stations, Barracks, and Hospital of Hong Kong (written in 1846). On the Health of H. M. and Hon. E. I. Company's Troops serving in China, from 1st April 1845 to 31st March 1846. 8vo.	Ditto

DONATIONS.	DONORS.
Contributions to the Military Medical Statistics of the Bombay Presidency, 1851. By John Kinnis, M.D. 8vo.	The Author.
Proceedings of the Geological Society of London. Vol. iv., Nos. 99, 101, 102, 103. 8vo.	The Society.
Quarterly Journal of the Geological Society of London. Nos. 21–28. 8vo.	Ditto.
Papers relating to the University of Sydney, and to the University College, Sydney, New South Wales. Printed at the desire of Sir J. F. W. Herschel, Bart., G. B. Airy, Esq., Professor Malden, and Henry Denison, Esq. 1851. 8vo.	The Editors.
Journal of the Asiatic Society of Bengal. Nos. 208 to 210. Oct. to Dec. 1849. 8vo.	The Society.
Proceedings of the Royal Astronomical Society. Vol. xi., No. 9. 8vo.	Ditto.

January 5, 1852.

Flora Batava. Aflevering 166. 4to.	King of Holland.
Mémoires de l'Académie Impériale des Sciences de St Pétersbourg. Sciences Mathématiques, Physiques, et Naturelles. Tome iv^{me}, Liv. 3 & 4. 4to.	The Academy.
Mémoires présentés à l'Académie Impériale des Sciences de St Pétersbourg, par divers Savants et lus dans ses Assemblées. Tome vi^{me}, Liv. 5 & 6. 4to.	Ditto.
Memorie della Reale Accademia della Scienze di Torino. Serie 2da. Tomo xi. 4to.	Ditto.
Annales de l'Observatoire Physique Central de Russie. 1848. 3 tom. 4to.	The Observatory.
Compte Rendu Annuel, addressé à M. le Comte Wrontchenko, Ministre des Finances, par le Directeur de l'Observatoire Physique Central de Russie, A. T. Kupffer. 1850. 4to.	The Editor.
The American Journal of Science and Arts. Vol. xii., No. 36. 8vo.	The Editors.
Bulletin de la Société de Géographie. 4^{me} Serie. Tom. i. 8vo.	The Society.
Abhandlungen der Königliche Akademie der Wissenschaften zu Berlin. 1849. 4to.	The Academy.
Monatsbericht der Königliche Akademie der Wissenschaften zu Berlin. 1850, Jan.—Dec.; 1851, Jan.—Juni. 8vo.	Ditto.
Proceedings of the Philosophical Society of Glasgow. 1850. Vol. iii., No 3. 8vo.	The Society.
Bulletin de la Société Impériale des Naturalistes de Moscou. 1850, Nos. 3 & 4; 1851, No. 1. 8vo.	Ditto.
Journal of the Asiatic Society of Bengal. 1851. No. 5. 8vo.	Ditto.

February 2, 1852.

Smithsonian Contributions to Knowledge. Vol. ii. Collection of Various Reports. 4to.	The Smithsonian Society.
Transactions of the Zoological Society of London. Vol. iv., Pt. 1. 4to.	The Association.
Proceedings of the Zoological Society of London. Nos. 201–213. 8vo.	Ditto.
Proceedings of the American Association for the Advancement of Science. August 1850. 8vo.	Ditto.
Transactions of the Horticultural Society of London. 2d Series. Vol. ii. Pts. 3, 4, 5, 6; Vol. iii., Pts. 1, 2, 3. 4to.	The Society.
Novi Commentarii Academiæ Scientiarum Instituti Bononensis. Tom. vi., vii., viii., ix., x. 4to.	The Academy.
Memorie della Accademia delle Scienze dell' Instituto di Bologna. Tomo i. 4to.	Ditto.

February 16, 1852.

Verhandlungen der Kaiserlichen Leopoldinisch-Carolinischen Akademie der Naturforscher. B. 22. 4to.	The Academy.
Journal of the Horticultural Society of London. Vol. vii., Part 1. 8vo.	The Society.

DONATIONS.	DONORS.
Museum of Practical Geology:—On the Science of Geology and its applications. By Andrew C. Ramsay;—On the value of extended knowledge of Mineralogy and the Process of Mining. By W. W. Smyth;—On the Importance of Special Scientific Knowledge. By John Percy, M.D. 8vo.	The Museum.

March 1, 1852.

Philosophical Transactions of the Royal Society of London. 1851. Part 2. 4to.	The Society.
List of Fellows of Do. 30th November 1851. 4to.	Ditto.
Memoirs of the Royal Astronomical Society. Vol. xx. 4to.	Ditto.
Notices of Do. Vol. ii. 1850–1. Nos. 1–9. 8vo.	Ditto.
Quarterly Journal of the Geological Society. Vol. viii., Part 1. 8vo.	Ditto.
American Journal of Science and Arts. Vol. xiii., No. 37. 8vo.	The Editors.
Transactions of the Royal Scottish Society of Arts. Vol. iii., Part 5. 8vo.	The Society.
Transactions of the Architectural Institute of Scotland. Vol. ii., Part 2. 8vo.	The Institute.
Journal of Agriculture, and Transactions of the Highland and Agricultural Society of Scotland. N. S. No. 36. 8vo.	The Society.
Astronomical and Magnetical and Meteorological Observations made at the Royal Observatory, Greenwich, in the year 1850. 4to.	The Royal Society.
Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften. Mathemat. Natur. Classe. Bd. vii., Stück 1 & 2. 8vo.	The Academy.
Flora Batava. No. 167. 4to.	King of Holland.
Guide to Northern Archæology, by the Royal Society of Northern Antiquaries of Copenhagen. Edited by the Right Honourable the Earl of Ellesmere. 8vo.	The Editor.
Papers on Railway and Electric Communications, &c. By Walter White. 12mo.	The Author.
Rules and Regulations, and List of Members, of the Athenæum. 12mo.	The Athenæum.

April 5, 1852.

The Assurance Magazine. Nos. 5 & 7. 8vo.	The Institute of Actuaries.
Nouveaux Mémoires de la Société Impériale des Naturalistes de Moscou. Tome ix. 4to.	The Society.
Bulletin de la Société Impériale des Naturalistes de Moscou. 1851. No. 2. 8vo.	Ditto.

April 19, 1852.

The Nature and Treatment of Diseases of the Heart. By James Wardrop, M.D. 8vo.	The Author.
Twentieth Report of the Scarborough Philosophical Society. 8vo.	The Society.
Minutes of Proceedings of the Institution of Civil Engineers. 1849–51. 8vo.	The Institution.
List of Members of Ditto.	Ditto.
Constitution and Laws of the Institute of Actuaries of Great Britain and Ireland. 8vo.	Ditto.
Transactions of the Cambridge Philosophical Society. Vol. ix. Part 2. 8vo.	The Society.
The American Journal of Science and Arts. Nos. 38–41. 8vo.	The Editors.
Flora Batava. Aflevering 168. 4to.	King of Holland.

December 6, 1852.

Journal of the Statistical Society of London. Vol. xv., Parts 1, 2, 3. 8vo.	The Society.
Journal of the Horticultural Society of London. Vol. vii., Parts 2 & 3. 8vo.	Ditto.
Journal of Agriculture, and the Transactions of the Highland and Agricultural Society of Scotland. N. S. Nos. 37 & 38. 8vo.	The Society.
Quarterly Journal of the Chemical Society. Vol. v., Nos. 2 & 3. 8vo.	Ditto.
Quarterly Journal of the Geological Society. Vol. viii., Parts 2, 3, 4. 8vo.	Ditto.
Address delivered at the Anniversary Meeting of the Geological Society of London, on the 20th of February 1852. By William Hopkins, Esq. 8vo.	Ditto.

DONATIONS.	DONORS.
Journal of the Geological Society of Dublin. Vol. v., Part 2. 8vo.	The Society.
Journal of the Asiatic Society of Bengal. N. S. Nos. 46, 50, 51, 52, 53, 54. 8vo.	Ditto.
Transactions of the Architectural Institute of Scotland. Vol. ii., Parts 3, 4, 5. 8vo.	The Institute.
Assurance Magazine, and Journal of the Institute of Actuaries. Nos. 6, 8, 9. 8vo.	Ditto.
Scientific Memoirs, selected from the Transactions of Foreign Academies of Science and learned Societies. Edited by R. Taylor, F.S.A. Vol. v., Part 20. 8vo.	The Editor.
Address at the Anniversary Meeting of the Royal Geographical Society, 24th May 1852. By Sir R. J. Murchison. 8vo.	The Society.
Catalogue of the Library of the Royal Geographical Society. 8vo.	Ditto.
Proceedings of the Royal Irish Academy. Vol. iv., Parts 2 & 3. Vol. v., Part 1. 8vo.	The Academy.
Annals of the Lyceum of Natural History of New York. Vol. v., Nos. 3–14. 8vo.	The Lyceum.
Medico-Chirurgical Transactions. Published by the Royal Medical and Chirurgical Society of London. Vols. xxiv. & xxv. 8vo.	The Society.
Proceedings of the Philosophical Society of Glasgow. 1851–2. Vol. iii., No. 4. 8vo.	Ditto.
Nineteenth Report of the Scarborough Philosophical Society. 1850. 8vo.	The Society.
Twenty-ninth Annual Report of the Royal Asiatic Society of Great Britain and Ireland. 1852. 8vo.	Ditto.
Nineteenth Annual Report of the Royal Cornwall Polytechnic Society. 1851. 8vo.	Ditto.
Annual Reports of the Leeds Philosophical and Literary Society. 1832–52. 8vo.	Ditto.
Proceedings of the American Association for the Advancement of Science. 1848, 1850, 1851. 8vo.	The Association.
A Notice of the Origin, Progress, and Present Condition of the Academy of Natural Sciences of Philadelphia. By W. S. W. Ruschenberger, M.D. 8vo.	The Academy.
Collection of Reports from the Secretary of the Treasury of the American Government on Scientific Subjects. 8vo.	The American Government.
Proceedings of the Academy of Natural Sciences of Philadelphia. Vol. v., Nos. 10 & 12. Vol. vi., Nos. 1 & 2. 8vo.	The Academy.
Exploration and Survey of the Valley of the Great Salt Lake of Utah, including a reconnoissance of a new route through the Rocky Mountains. By Howard Stansbury, Capt. T.E., U.S. Army. With Plates. 8vo.	The Author.
The Mastodon giganteus of North America. By John C. Warren, M.D. 4to.	Ditto.
Regi Magyar Nyelvemlékek. Kötet. 1, 2, 3. 4to.	Literary Society of Hungary.
A' Magyar Tudós Társásag' Evkönyvei. Kötet. 3, 4, 6, 7. 4to.	Ditto.
Hunyadiak Kora Madyarországou. irta Gróf Teleki Jozsef. 1 Kötet. 8vo.	Ditto.
Fifth Annual Report of the Board of Regents of the Smithsonian Institution for 1850. 8vo.	The Institution.
On Recent Improvements in the Chemical Arts. By Professor James C. Booth, and Campbell Morfit. 8vo.	Ditto.
Catalogue of Stars near the Ecliptic, observed at Markree during the years 1848, 1849, & 1850, and whose places are supposed to be hitherto unpublished. Vol. i. 8vo.	H.M. Government.
Proceedings of the American Philosophical Society. Vol. v., No. 47. 8vo.	The Society.
Catalogue of the Library, and Constitution and Laws of the Institute of Actuaries of Great Britain and Ireland. 8vo.	The Institute.
Mémoires présentés par divers Savants à l'Académie des Sciences de l'Institut National de France. Sciences Mathématiques et Physiques. Tome xiii^{me}. 4to.	The Academy.
Recherches sur la Conductibilité des Mineraux pour l'Electricité Voltaïque. Par M. Elie Wartmann. 4to.	The Author.
Flora Batava, af Afbeelding en Beschrijving van Nederlandsche Gewasse, door Wijlen Jan Kops ; vervolgd door P. M. E. Gevers Deignoot. 16 Aflev. 4to.	The King of the Netherlands.
Historische en Letterkundige Verhandelingen van de Hollandsche Maatschappij der Wettenschappen te Haarlem. 1te Deel. 4to.	The Society.
Nieuwe Verhandelingen van het Bataafsch Genootschap der Prœfondervindelijke Wijsbegeerte te Rotterdam. 11te Deel. 4to.	Ditto.
Det Kongelige Danske Videnskabernes Selskabs Skrifter. Femte Række. Naturvidenskabelig og Mathematisk Afdeling. Andet Bind. 4to.	The Academy.

DONATIONS.	DONORS.
Abhandlungen der Mathematisch-Physischen Classe der Königlich Sächsischen Gesellschaft der Wissenschaften. Band i. 8vo.	The Society.
Naturwissenschaftliche Abhandlungen, gesammelt und durch subscription herausgegeben von Wilhelm Haidinger. Band iv. 4to.	The Editor.
Archives du Muséum d'Histoire Naturelle, publiées par les Professeurs-Administratifs de cet Etablissement. Tome v., Liv. 4; Tome vi., Liv. 1, 2, 3, & 4. 4to.	The Society.
Denkschriften der Kaiserlichen Akademie der Wissenschaften. Mathematisch-Naturwissenschaftliche Classe. Band iii. 1te & 3te Lieferungs. Fol.	The Academy.
Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften. Mathematisch-Naturwissenschaftliche Classe. 1851; Band vii., 3, 4, & 5 Heft: and 1852; Band viii., 1, 2, 3 Heft. 8vo.	Ditto.
Almanach der Königliche Akademie der Wissenschaften. 1852. 12°.	Ditto.
Jahrbuch der Kaiserlich-Königlichen Geologischen Reichsanstalt. 1851; Nos. 1, 2, 3, & 4. 1852; No. 1. 8vo.	The Association.
Mémoires de la Société Nationale des Sciences de l'Agriculture et des Arts de Lille. Année 1850. 8vo.	The Society.
Die Fortschritte der Physik im Jahre 1848. Dargestellt von der Physikalischen Gesellschaft zu Berlin. 8vo.	Ditto.
Bulletin de la Société de Géographie. 4me Série. Tomes ii. & iii. 8vo.	Ditto.
Manuel de la Navigation à la Côte Occidentale d'Afrique. Par M. Charles Philippe de Kerhallet. 2 Tomes. 8vo.	Dépôt-Général de la Marine, Paris.
Annales Hydrographiques, Recueil d'Avis, Instructions, Documents et Mémoires relatifs à l'Hydrographie et à la Navigation, publié par le Dépôt-Général de la Marine. Tomes 4me & 5me. 8vo.	Ditto.
Tafeln zur Reduction der in millemetern abgelesenen Barometerstände auf die Normaltemperatur von 0° Celsus. Berechent von J. J. Pohl & J. Schabus. 8vo.	The Authors.
Tafeln zur Vergleichung und Reduction der in verschiedenen Längenmassen Abgelesenen Barometerstände. Von J. J. Pohl & J. Schabus. 8vo.	Ditto.
Transactions of the Royal Irish Academy. Vol. xxii., Parts 1 & 2. 4to.	The Academy.
American Journal of Science and Arts. Vol. xiv., No. 42. 8vo.	The Editors.
Smithsonian Contributions to Knowledge. Vols. iii. & iv. 4to.	The Smithsonian Institution.
Schoolcraft's History of the Indian Tribes. Part ii. 4to.	The American Government.
Astronomical Observations made at the Royal Observatory, Edinburgh, by the late Thomas Henderson, Esq. Reduced and Edited by Charles Piazzi Smith, Esq. Vol. x. 1844–5–6–7. 4to.	The Observatory.

January 4, 1853.

Memorie della Reale Accademia delle Scienze de Torino. Serie 2. Tom. xii. 4to.	The Academy.
Acta Academiæ Cæsareæ Leopoldino-Carolinæ Naturæ Curiosarum. Vol. xxiii., Pars 2. 4to.	Ditto.
Transactions of the Linnæan Society of London. Vol. xxi., Part 1. 4to.	Ditto.
Proceedings of the Linnæan Society of London. Feb. 4, 1851 to March 16, 1852. 8vo.	The Society.
Abhandlungen der Akademie der Wissenschaften zu Berlin. 1850. 4to.	The Academy.

January 17.

Flora Batava. Part 171. 4to.	King of Holland.
The Assurance Magazine, and Journal of the Institute of Actuaries. No. 10. 8vo.	The Institute.
Journal of the Horticultural Society of London. Vol. vii., Part 4; vol. viii., Part 1. 8vo.	The Society.
Mémoires de l'Académie Royale des Sciences, des Lettres, et des Beaux Arts de Belgique. Tom. xxvi. 4to.	Ditto.

DONATIONS.	DONORS.
Mémoires Couronnés et Mémoires des Savants Etrangers, publiés par l'Académie Royale des Sciences, des Lettres, et des Beaux Arts de Belgique. Tome xxiv. 4to.	The Academy.
Bulletins de l'Académie Royale des Sciences, des Lettres, et des Beaux Arts de Belgique. Tomes xvii.–xix. (1850–1852.) 8vo.	Ditto.
Annuaire de l'Académie Royale des Sciences, des Lettres, et des Beaux Arts de Belgique. (1851 and 1852.) 12°.	Ditto.
Mémoires Couronnés et Memoires des Savants Etrangers, publiés par l'Académie Royale des Sciences, des Lettres, et des Beaux Arts de Belgique. Collection in 8°. Tome v.	Ditto
Annales de l'Observatoire Royal de Bruxelles, publiées aux frais de l'Etablissement, par le Directeur, A. Quételet. Tomes viii. et ix. 4to.	The Editor.
Annuaire de l'Observatoire Royal de Bruxelles. Par A. Quételet. 1851 & 1852. 12°.	Ditto.
Résumé des Observations sur la Météorologie et sur le Magnetisme Terrestre faites á l'Observatoire Royal de Bruxelles en 1850, et communiquées par le Directeur, A. Quételet. 4to.	The Author.
The Canadian Journal; a Repertory of Industry, Science, and Art, and a Record of the Proceedings of the Canadian Institute. October and December 1852. 4to.	The Institute.
Journal of the Royal Geographical Society of London. Vol. xxii. 1852. 8vo.	The Society.
Catalogue Méthodique de la Collection des Reptiles; Muséum d'Histoire Naturelle de Paris. 8vo. Catalogue Méthodique de la Collection des Mammifères de la Collection des Oiseaux; Muséum d'Histoire Naturelle de Paris. 8vo.	The Museum.
Berichte über die Verhandlungen der Königlich Sachsischen Gesellschaft der Wissenschaften zu Leipzig. (1848.) 8vo. Bande i., ii., iii.	The Society.
Catalogue des Manuscrits et Hylographes Orientaux de la Bibliothèque Impériale Publique de St Pétersbourg. 8vo.	The Russian Government.

February 7, 1853.

Craigie's Practice of Physic. 2 vols. 8vo.	The Author.
Abhandlungen der Akademie der Wissenschaften zu Berlin. 1850 and 1851. 4to.	
Monatsbericht der Akademie der Wissenschaften zu Berlin. Juli–Oct. 8vo.	The Academy.
Acta Academiæ Cæsareæ Leopoldino-Carolinæ Naturæ Curiosarum. Vol. xxii., Suppl., and xxiii. 4to.	Ditto.
Memorias della Real Academia de Ciencias de Madrid. Tome i., Part 2. Fol.	Ditto.
Resumen de las Actas de la Academia Real de Ciencias de Madrid. 1850 and 1851. 8vo.	Ditto.

March 7, 1853.

Journal of Agriculture, and Transactions of the Highland and Agricultural Society of Scotland. N.S. No. 40. 8vo.	The Society.
Papers and Proceedings of the Royal Society of Van Diemen's Land. Vol. ii., Part 1. 8vo.	Ditto.
The Canadian Journal; a Repertory of Industry, Science, and Art, and a Record of the Proceedings of the Canadian Institute. January 1853. 4to.	The Institute.
Flora Batava. 172 Aflevering. 4to.	King of Holland.
Acta Regiæ Societatis Scientiarum Upsaliensis. 3d Ser. Vol. i., Fascic. 1. 4to.	The Society.
Berichte über die Verhandlungen der Königlich Sächsischen Gesellschaft der Wissenschaften zu Leipzig. Mathematisch-Physische Class i. 8vo.	Ditto.
Ueber Musikalische Tonbestimmung und Temperatur. Von M. W. Drobisch. 8vo.	The Author.
Beiträge zur Kenntnis der Gefäss-kryptogamen. Von Wilhelm Hofmeister. 8vo.	Ditto.
Jahrbuch der Kaiserlich-Königlichen Geologischen Reichsanstalt. 1852. No. 2. 8vo.	The Institute.
Memoirs of the American Academy of Arts and Sciences. N.S. Vol. iv., Part 2. 4to.	The Academy.

March 21, 1853.

DONATIONS.	DONORS.
Ordnance Survey. Astronomical Observations made with Airy's Zenith Sector, from 1842 to 1850, for the determination of the Latitudes of various Trigonometrical Stations used in the Ordnance Survey of the British Isles. By Captain W. Yolland. 4to.	The Hon. Board of Ordnance.
Archives du Muséum d'Histoire Naturelle, publiées par les Professeurs-Administrateurs de cet Etablissement. Tome vi., Liv. 3 and 4. 4to.	The Editors.
The American Journal of Science and Arts. 2d Series. No. 43. 8vo.	Ditto.

April 18, 1853.

Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences. 4to.	French Government.
Memorie della Accademia delle Scienze dell' Instituto di Bologna. Tom. ii. 4to.	The Academy.
Rendendrionto delle Aduvanze e de' Lavori della Reale Accademia delle Scienze sezione della Societa Reale Borbonica. N.S. Nos. 1–5. 4to.	Ditto.
Relazione Lalla Malattia della Vite apparsa nei contorni Napoli ed altri luoghi della Provincia fatta da una commissione della Reale Accademia delle Scienze. 4to.	Ditto.
Opuscula Matematici di Tito gonella. 4to.	C. Babbage, Esq.
The Assurance Magazine, and Journal of the Institute of Actuaries. No. 11. 8vo.	The Institute.
Catalogue of Ancient and Mediæval Rings and Personal Ornaments formed for Lady Londesborough. 4to.	Lord Londesborough.

INDEX TO VOL. XX.

A

Alison (William Pulteney), M.D. Defence of the doctrine of vital affinity, 385. Observations on the speculations of Dr Brown and other recent metaphysicians, regarding the exercise of the senses, 513.

Anderson (Thomas), M.D. On the constitution of codeine and its products of decomposition, 57. On the products of the destructive distillation of animal substances, 247. Researches on some of the crystalline constituents of opium, 347.

Animal Substances, on the products of the destructive distillation of, 247.

Aqueous Vapour which is condensed on a cold surface, on the weight of, 299.

B

Barometer. On a necessary correction to the observed height of the barometer, depending upon the force of the wind, 377.

Brewster (Sir David). On the optical phenomena and crystallisation of tourmaline, titanium, and quartz, within mica, amethyst, and topaz, 547. On the production of crystalline structure in crystallised powders, by compression and traction, 555. On circular crystals, 607.

Bust, notice of an antique marble, 417.

C

Capric Acid, on a new source of obtaining, and remarks on some of its salts, 219.

Centrifugal Theory of Elasticity, and its connection with the theory of heat, 425.

Charr (*Salmo umbla*), some observations on the, relating chiefly to its generation and early stage of life, 321.

Codeine and its products of Decomposition, on the constitution of, 57.

Comenic Acid, on certain salts and products of decomposition of, 225.

Cometary Physics, some remarks on the theories of, 131.

Compound Series, summation of a, and its application to a problem in probabilities, 541.

Coventry (Andrew). Notice of an antique marble bust, 417.

Crystalline Constituents of Opium, researches on some of the, 347.

Crystalline Structure, on the production of, in crystallised powders, by compression and traction, 555.

Crystallised Powders, on the production of crystalline structure in, by compression and traction, 555.

Circular Crystals, 607.

D

Dalmahoy (James). On the weight of aqueous vapour which is condensed on a cold surface, under given conditions, 299.

Davy (John), M.D. Some observations on the charr (*Salmo umbla*), relating chiefly to its generation and early stage of life, 321. Some observations on fish, in relation to diet, 599.

Differential Calculus, on a process in the, 39.

Dynamical Theory of Heat, on the, 261.

Dynamical Equivalent of Temperature in Liquid Water, and the specific heat of atmospheric air and steam, 191.

E

Eclipse (total) of the Sun, on July 28, 1851, observed at Göteborg; with a description of a new position micrometer, 335.

Eclipses (total) of the Sun, on the red prominences seen during, 461.

Eclipse (total Solar) of 1851, 503.

Eildon Hills, notes on the geology of the, 211.

Elasticity, on the centrifugal theory of, and its connection with the theory of heat, 425.

Equilibrium of Elastic Solids, on the, 87.

Expansive Machines, on the economy of heat in, 205.

F

Fish, in relation to Diet, some observations on, 599.

Fluorine, on two new processes for the detection of, when accompanied by silica, and on the presence of fluorine in granite, trap, and other igneous rocks, and in the ashes of recent and fossil plants, 483.

Forbes (Edward) and John Goodsir. On some remarkable marine invertebrata new to the British seas, 307.

Forbes (James D.). On the volcanic geology of the Vivarais (Ardêche), 1. Notes on the geology of the Eildon Hills in Roxburghshire, 211.

G

Gaseous Fluid, on a method of discovering experimentally the relation between the mechanical work spent, and the heat produced by the compression of a, 289.

Göteborg, on the total eclipse of the sun, on July 28, 1851, observed at Göteborg; with a description of a new position micrometer, 335.

H

Heat, on the mechanical action of, especially in gases and vapours, 147. On the economy of, in expansive machines, 205. On the dynamical theory of, 261, 475. On the mechanical action of, 561, 565.

How (Henry). On certain salts and products of decomposition of comenic acid, 225. On meconic acid, and some of its derivatives, 401.

I

Interfering Light, on the total intensity of, 317.

Invertebrata (Marine) new to the British Seas, on some remarkable, 307.

J

James (Captain Henry). On a necessary correction to the observed height of the barometer depending upon the force of the wind, 377.

K

Kelland (Rev. P.). On a process in the differential calculus, and its application to the solution of certain differential equations, 39.

L

Light (Interfering), on the total intensity of, 317,

Light (Zodiacal), contributions to a knowledge of the phenomena of, 489.

M

Marine Invertebrata new to the British Seas, on some remarkable, 307.

Maxwell (James Clerk). On the equilibrium of elastic solids, 87.

Mechanical Action of Heat, especially in gases and vapours, 147.

Meconic Acid, and some of its derivatives, 401.

Metaphysicians, speculations of Dr Brown and other recent, regarding the exercise of the senses, 513.

N

Nitric Acid, as a source of the nitrogen found in plants, 591.

Nitrogen found in Plants, on nitric acid as a source of the, 591.

O

Opium, researches on some of the crystalline constituents of, 347.

P

Peruvian Musical Instrument, like the syrinx of the Ancients, dissertation on a, 121.

Probabilities, summation of a compound series, and its application to a problem in, 541.

R

Rankine (W. J. Macquorn). On the mechanical action of heat, especially in gases and vapours, 147. Note as to the dynamical equivalent of temperature in liquid water, and the specific heat of atmospheric air and steam, 191. On the power and economy of single-acting expansive steam-engines, 195. On the economy of heat in expansive machines, 205. On the centrifugal theory of elasticity, and its connection with the theory of heat, 425. On the computation of the specific heat of liquid water at various temperatures, from the experiments of M. Regnault, 441. On the absolute zero of the perfect gas thermometer; being a note to a paper on the mechanical action of heat, 561, 565.

Rowney (Thomas). On a new source for obtaining capric acid, and remarks on some of its salts, 219.

S

Salmo umbla, some observations on the charr (Salmo umbla), relating chiefly to its generation and early stage of life, 321.

Senses, observations on the speculations of Dr Brown, and other recent metaphysicians, regarding the exercise of the, 513.

Single-Acting Expansive Steam-Engines, on the power and economy of, 195.

Smyth (Charles Piazzi). Some remarks on the theories of cometary physics, 131. Contributions to a knowledge of the phenomena of zodiacal light, 489. On the total solar eclipse of 1851, 503.

Solar (total) Eclipse of 1851, 503.

Solids, on the equilibrium of elastic, 87.

Steam Engines, on the power and economy of single-acting expansive, 195.

Stokes (Professor). On the total intensity of interfering light, 317.

Sun, on the total eclipse of the, on July 28, 1851, observed at Göteborg, with a description of a new micrometer, 335. On the red prominences seen during total eclipses of the, 461.

Swan (William). On the total eclipse of the sun, on July 28, 1851, observed at Göteborg, with a description of a new position micrometer, 335. On the red prominences seen during total eclipses of the sun, 461.

T

Temperature and Density, on the quantities of mechanical energy contained in a fluid in different states as to, 475.

Temperature in Liquid Water, on the dynamical equivalent of, and the specific heat of atmospheric air and steam, 191.

Terrot (Right Rev. Bishop). Summation of a compound series, and its application to a problem in probabilities, 541.

Thermometer, on the absolute zero of the perfect gas, 561.

Thomson (William). On the dynamical theory of heat, with numerical results deduced from M. Joule's equivalent of a thermal unit, and M. Regnault's observations on steam, 261. On a method of discovering experimentally the relation between the mechanical work spent, and the heat produced by the compression of a gaseous fluid, 289. On the dynamical theory of heat. Part V. On the quantities of mechanical energy contained in a fluid in different states, as to temperature and density, 475.

Tourmaline, Titanium, and Quartz, on the optical phenomena and crystallisation of, within mica, amethyst, and topaz, 547.

Traill (Thomas Stewart), M.D. Dissertation on a Peruvian musical instrument, like the syrinx of the ancients, 121.

V

Vital Affinity, defence of the doctrine of, 385.

Vivarais (*Ardèche*), volcanic geology of the, 1.

W

Water, dynamical equivalent of temperature in liquid, and the specific heat of atmospheric air and steam, 191. On the computation of the specific heat of liquid water at various temperatures, from the experiments of M. Regnault, 441.

Wilson (George), M.D. On two new processes for the detection of fluorine when accompanied by silica, and on the presence of fluorine in granite, trap, and other igneous rocks, and in the ashes of recent and fossil plants, 483. On nitric acid as a source of the nitrogen found in plants, 591.

Wind, on a necessary correction to the observed height of the barometer depending upon the force of the, 377.

Z

Zodiacal Light, contributions to a knowledge of the phenomena of, 489.

END OF VOLUME TWENTIETH.

Neill & Co., Printers, Edinburgh.

LAWS

OF THE

ROYAL SOCIETY OF EDINBURGH.

SEPTEMBER, 1853.

LAWS.

I.

THE ROYAL SOCIETY OF EDINBURGH shall consist of Ordinary and Honorary Fellows.

II.

Every Ordinary Fellow, within three months after his election, shall pay Five Guineas as fees of admission, and Three Guineas as his contribution for the Session in which he has been elected; and annually at the commencement of every Session, Three Guineas into the hands of the Treasurer.*

III.

All Fellows who shall have paid Twenty-five years' annual contribution shall be exempt from farther payment.

IV.

The fees of admission of an Ordinary Non-Resident Fellow shall be £26, 5s., payable on his admission; and in case of any Non-Resident Fellow coming to reside at any time in Scotland, he shall, during each year of his residence, pay the usual annual contribution of £3, 3s., payable by each Resident Fellow; but after payment of such annual contribution for eight years, he shall be exempt from any farther payment.

V.

Members failing to pay their contribution for three successive years (due application having been made to them by the Treasurer) shall be reported to the Council, and, if they see fit, shall be declared from that period to be no longer Fellows, and the legal means for recovering such arrears shall be employed.

VI.

None but Ordinary Fellows shall bear any office in the Society, or vote in the choice of Fellows or Office-Bearers, or interfere in the patrimonial interests of the Society.

* A modification of this rule, in certain cases, was agreed to 3d January 1831.

VII.

The number of Ordinary Fellows shall be unlimited.

VIII.

The Ordinary Fellows, upon producing an order from the TREASURER, shall be entitled to receive from the Publisher, gratis, the Parts of the Society's Transactions which shall be published subsequent to their admission.

IX.

No person shall be proposed as an Ordinary Fellow, without a recommendation subscribed by *One* Ordinary Fellow, to the purport below.* This recommendation shall be delivered to the Secretary, and by him laid before the Council, and shall afterwards be printed in the circulars for three Ordinary Meetings of the Society, previous to the day of the election, and shall lie upon the table during that time.

X.

Honorary Fellows shall not be subject to any Contribution. This class shall consist of persons eminently distinguished for science or literature, Its number shall not exceed Fifty-six, of whom twenty may be British subjects, and thirty-six may be subjects of foreign states.

XI.

Personages of Royal Blood may be elected Honorary Fellows, without regard to the limitation of numbers specified in Law X.

XII.

Honorary Fellows may be proposed by the Council, or by a recommendation (in the form given below)† subscribed by three Ordinary Fellows; and in case

* "A. B., a gentleman well skilled in several branches of Science (*or Polite Literature as the "case may be*), being to my knowledge desirous of becoming a Fellow of the Royal Society of Edin- "burgh, I hereby recommend him as deserving of that honour, and as likely to prove a useful and "valuable Member."

This recommendation to be accompanied by a request of admission signed by the Candidate.

† We hereby recommend _____
for the distinction of being made an Honorary Fellow of this Society, declaring that each of us from our own knowledge of his services to (*Literature or Science as the case may be*) believe him to be worthy of that honour.

(To be signed by three Ordinary Fellows.)

_____

_____

_____

To the President and Council of the Royal Society of Edinburgh.

the Council shall decline to bring this recommendation before the Society, it shall be competent for the proposers to bring the same before a General Meeting. The election shall be by ballot, after the proposal has been communicated *viva voce* from the Chair at one meeting, and printed in the circular for the meeting at which the ballot is to take place.

XIII.

The election of Ordinary Fellows shall take place at the Ordinary Meetings of the Society. The election shall be by ballot, and shall be determined by a majority of at least two-thirds of the votes, provided Twenty-four Fellows be present and vote.

XIV.

The Ordinary Meetings shall be held on the first and third Mondays of every month, from November to June inclusive. Regular minutes shall be kept of the proceedings, and the Secretaries shall do the duty alternately, or according to such agreement as they may find it convenient to make.

XV.

The Society shall from time to time publish its Transactions and Proceedings. For this purpose the Council shall select and arrange the papers which they shall deem it expedient to publish in the *Transactions* of the Society, and shall superintend the printing of the same.

XVI.

The Transactions shall be published in Parts or *Fasciculi* at the close of each session, and the expense shall be defrayed by the Society.

There shall be elected annually, for conducting the publications and regulating the private business of the Society, a Council, consisting of a President; Six Vice-Presidents, two at least of whom shall be resident; Twelve Counsellors, a General Secretary, Two Secretaries to the Ordinary Meetings, a Treasurer, and a Curator, and an Assistant-Curator of the Museum and Library.

XVII.

Four Counsellors shall go out annually, to be taken according to the order in which they stand on the list of the Council.

XVIII.

An Extraordinary Meeting for the Election of Office-Bearers shall be held on the fourth Monday of November annually.

XIX.

Special Meetings of the Society may be called by the Secretary, by direction of the Council; or on a requisition signed by six or more Ordinary Fellows. Notice of not less than two days must be given of such meetings.

XX.

The Treasurer shall receive and disburse the money belonging to the Society, granting the necessary receipts, and collecting the money when due.

He shall keep regular accounts of all the cash received and expended, which shall be made up and balanced annually; and at the last Ordinary Meeting in January, he shall present the accounts for the preceding year, duly audited. At this Meeting, the Treasurer shall also lay before the Council a list of all arrears due above two years, and the Council shall thereupon give such directions as they may deem necessary for recovery thereof.

XXI.

At the Extraordinary Meeting in November, a Committee of three Fellows shall be chosen to audit the Treasurer's accounts, and give the necessary discharge of his intromissions.

The report of the examination and discharge shall be laid before the Society at the last Ordinary Meeting in January, and inserted in the records.

XXII.

The General Secretary shall keep Minutes of the Extraordinary Meetings of the Society, and of the Meetings of the Council, in two distinct books. He shall, under the direction of the Council, conduct the correspondence of the Society, and superintend its publications. For these purposes, he shall, when necessary, employ a clerk, to be paid by the Society.

The Secretaries to the Ordinary Meetings shall keep a regular Minute-book, in which a full account of the proceedings of these Meetings shall be entered: they shall specify all the Donations received, and furnish a list of them, and of the donors' names, to the Curator of the Library and Museum: they shall likewise furnish the Treasurer with notes of all admissions of Ordinary Fellows. They shall assist the General Secretary in superintending the publications, and in his absence shall take his duty.

XXIII.

The Curator of the Museum and Library shall have the custody and charge of all the Books, Manuscripts, objects of Natural History, Scientific Productions, and other articles of a similar description belonging to the Society; he shall take an

account of these when received, and keep a regular catalogue of the whole, which shall lie in the Hall, for the inspection of the Fellows.

XXIV.

All articles of the above description shall be open to the inspection of the Fellows, at the Hall of the Society, at such times, and under such regulations, as the Council from time to time shall appoint.

XXV.

A Register shall be kept, in which the names of the Fellows shall be enrolled at their admission, with the date.

Neill & Co., Printers.

BRITISH MUSEUM
NATURAL HISTORY

ERRATA.

In Mr RANKINE'S paper, "On the Centrifugal Theory of Elasticity," Vol. xx., Part iii.

Article 10, page 437, line 7; in Equation 27, *for* $\int (\tau - z) \frac{d p}{d \tau} d \nabla$ *read* $\int \left\{ (\tau - z) \frac{d p}{d \tau} - p \right\} d \nabla$

Art. 11, page 438, line 12; on each side of the equation, make the same correction.

Art. 12, page 439, second line from the bottom; *for* $J \mu$ *read* $\frac{J}{\mu}$.

In Bishop TERROT'S paper, "Summation of a Compound Series, and its Application to a Problem in Probabilities," Vol. xx., Part iv.

Page 541, line 4; *for* $\overline{m-p} \cdot \overline{m-p+1} \ldots m-\overline{p+q+1}$, *read* $\overline{m-p-q+1} \cdot \overline{m-p-q+2} \ldots \overline{m-p}$

Page 545, line 6; *for* 20, *read* 24.

S 4
C 20